THE BRITISH AND FOREIGN MEDICAL REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY

EDITED BY

JOHN FORBES M.D. F.R.S. F.G.S.

VOL. XVI.

JULY—OCTOBER 1843

LONDON

JOHN CHURCHILL PRINCES STREET SOHO

MDCCCXLIII.

LONDON:
PRINTED BY C. AND J. ADLARD, BARTHOLOMEW CLOSE.

CONTENTS OF No. XXXI.

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British and Foreign Medical Review.

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MEDICAL REVIEW,

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PART FIRST.

Analytical and Critical Reviews.

Art. I.

Traité du Ramollissement du Cerveau. Par Max. Durand-Fardel, M.D. &c. &c.—*Paris*, 1843. 8vo, pp. 526.

Treatise on Softening of the Brain. By Max. Durand-Fardel, M.D. &c. &c.—*Paris*, 1843.

The work of M. Durand-Fardel was composed to compete for a prize offered by the Royal Academy of Medicine for the best memoir on the following subject: "Describe the different species of softening of the nervous centres, (brain, cerebellum, and spinal marrow,) their causes, their signs, and their treatment." The volume before us, the result, as its author states, of several years of clinical observation, gained the prize.

Two very distinct things may be understood, observes the author, by the expression *softening* of the brain: either a particular morbid state, a disease to which a new name must be given when its nature shall have been satisfactorily established; or the mere condition of diminished consistence of the cerebral substance. It is of the former that M. Durand-Fardel treats,—in short, of the disease described originally by Rostan.

The disease may be acute or chronic; a division which M. Fardel, and with some justice, as the sequel will show, blames authors for having failed to recognize in respect of its morbid anatomy, although not a few of them have distinguished its acute and chronic forms symptomatically. The acute disease is first considered.

Acute softening. Softening of the brain during its acute stage is specially characterized by redness and diminished consistence, without disorganization, of the affected part. The cases observed by M. Fardel give further demonstration of a fact long known, that acute softening is by far the most frequent in the convolutions. Of thirty-three cases, thirty-one were examples of this seat of the disease, and in nine of them the convolutions were the sole parts affected. Fifty-three cases, collected

from various sources, give the following results as regards the situation of the morbid change:

Convolutions and white substance . . .	22
Convolutions alone	6
White substance alone	5
Corpus striatum and optic thalamus . . .	6
Corpus striatum alone	11
Optic thalamus alone	4
Pons varolii	3
Crus cerebri	1
Corpus callosum	1
Walls of the ventricles, septum	1
Fornix	1
Cerebellum	1

From these figures, and from some further facts referred to by the author, he concludes that both cerebral substances are almost always simultaneously softened; whereas, à priori views might lead, and did actually lead, some writers to teach that the cortical substance suffered most frequently. But M. Andral long since exposed the error; and M. Fardel's merit is simply that of recounting the figures.

The extent of brain implicated *may* be very considerable. M. Fardel has once seen almost the entire surface of both hemispheres and that of the ventricles softened. Such extensive disease is singularly uncommon however, and still more rare is diminished consistence of the entire mass of one hemisphere. We have the opposite extreme in cases where a space not larger than a pea is solely affected. The symptoms are far from being proportional, either in severity or otherwise, to this great difference in extent of anatomical change.

M. Fardel recommends, as the grand means of establishing the degree of softening in the affected tissue, that a stream of water be allowed to fall upon the part from varying heights. We should have expected that "several years of observation" might have led to the discovery of some more accurate standard than any furnished by this rude and, as experience has long proved, unsatisfactory sort of experiment. M. Fardel thinks it impossible to fix with precision the healthy amount of consistence of the brain, so much is this liable to vary with individual organization, the nature of the fatal disease, the composition of the blood, the state of the atmosphere, &c. He considers it easier and not less important to ascertain the relations of different parts to each other in regard of this property, and gives us the following as the results of the examination of 150 brains: The convolutions of the inferior surface of the anterior and middle lobes are much less firm than those of the convexity; this is especially true of the part answering to the sphenoid fossa; here the softness is often so great as to appear at first the result of disease, the pia mater carrying away with it during its removal shreds of the more superficial substance. On the convex surface the convolutions of the middle part are somewhat more dense than those of the anterior lobes, and those of the occipital end of the brain distinctly harder than the others, sometimes to quite an extraordinary degree. These observations, which the author adds to the more common knowledge respecting the relative consistence of the cerebellum, medulla oblongata, pons, and cerebrum, are of importance, and will, we trust, be confirmed; we wish this for the sake of those frequently engaged in examining brains,

for we have found such persons, (without, we confess, being ourselves able to remove the difficulty,) very sadly puzzled by this same matter of consistence in not a few cases.

Two important modifications of colour attend the progress of acute softening of the brain: redness first, and a yellow tint afterwards.

Redness occurs under three forms; those of injection, of sanguineous infiltration, and of uniform redness. Injection occurs especially in the medullary substance, and in the central parts of the brain, and affects not only the softened part, but those immediately adjacent—the latter, indeed, are often the most abundantly injected. In the greater number of instances, injection, when considerable, is accompanied with infiltration of blood, sometimes with small effusions of this fluid; in other cases the diseased spots have the appearance of patches, irregular in shape and size, red, violet, or blackish in colour. Besides this infiltration, observes the author, there is frequently to be seen a *uniform* red discoloration of the softened parts; this exists in the gray substance in lieu of injection. The tint varies from pale pink to red, never of a deep kind, and appears the result of imbibition rather than of infiltration of blood. There appears to be looseness both of expression and of idea in this mode of stating the varieties of colour of the softened substance; colour is confounded with vascularity and effusion of blood, the effect with the cause. Yet M. Fardel is so enamoured of his arrangement, that he criticises M. Lallemand for establishing the distinction just referred to, which common sense clearly requires. Whatever be its cause, red discoloration is generally much more marked in the gray than the white substance.

The *yellow colour*, often distinguishing softened cerebral substance, is next examined by the author, in respect of its nature and the various aspects under which it presents itself. Here we are favoured with a prolix refutation of M. Lallemand's erroneous notion respecting the necessary dependence of this colour on infiltration with pus: a notion over and over again refuted by experience, and which we scarcely expected to find again made the subject of such detailed exposure. The author himself believes the yellow colour the result of altered hue in blood infiltrated into the softened substance, assimilating it to the tint observed in ecchymoses externally: this is all very well; every one knows that such is the true explanation; but it is really amusing to observe the *naïveté* with which M. Fardel assumes the tone of an instructor in this very simple matter. He states that in cases of violent cerebral congestion he has sometimes seen a slight yellowish tint in the white substance of the hemispheres, "the result of transudation of blood still contained in the vessels," a species of *transudation* which we cannot profess ourselves able to conceive. Among the phenomena of slow poisoning by lead, is a peculiar dirty-yellow colour of the brain, ascribed by M. Tanquerel des Planches to the actual presence of lead,—by M. Martin-Solon connected with the previous existence of delirium, and an hypertrophous state of the brain. This colour M. Fardel suggests may have been the result of repeated congestions.

The period of the disease at which the yellow tint becomes apparent is worthy of investigation. M. Fardel states that it is rarely visible quite at the outset in the cortical substance, on account of the brightness of the red hue. In the medullary substance it is seen earlier either between in

jected points, or around partial infiltrations. But, adds the writer, in acute softening the yellow colour is "the result of actual imbibition of blood infiltrated or still contained in its vessels; accordingly it is almost always coupled with redness." We are again at sea; the imbibition by the brain of blood still contained within its vessels is beyond us; be that as it may, however, the important point is that complete yellowness of colour is extremely rare. Andral's eleventh case (Clin. Méd. t. v.) exemplifies this condition in a very satisfactory manner nevertheless.

Gray discoloration M. Fardel has never observed in cases of acute softening; neither have we, nor do we expect to find it. A greenish hue he regards, like the rest of the world, as evidence of suppuration or putrefaction. He once observed a remarkable deadness of the white colour of the medullary substance; this condition is the common one in the chronic disease.

Modifications in the form of softened parts next engage M. Fardel's attention. Tumefaction is a necessary result of the superabundance of fluids in their interior; this causes dryness of the corresponding meninges, but we have never noticed, nor can we easily understand the production of a particularly tense state of the adjoining dura mater, unless where the tumefaction is very considerable indeed. This state of tumefaction of the softened convolutions has been admirably figured, as many of our readers are aware, by Dr. Carswell. There is nothing new in the author's description, but he connects with this subject an enumeration of the characters of senile atrophy.

Softening of the brain, although sometimes surrounded by injection or infiltration, is generally an accurately circumscribed change; the rest of the organ exhibiting its natural colour and consistence. The meninges, however, appear under very various aspects. The dryness of the arachnoid produced by tumefaction mediately, immediately by the stretching to which the membrane is subjected, has already been mentioned; the surface is sometimes slightly viscid. The conditions of injection of the pia mater observed in twenty-four cases were as follow:

No injection or ordinary injection in	13 cases.
Tolerably marked injection	5 ...
Considerable ditto	4 ...
Injection limited to the diseased hemisphere . . .	1 ...
Injection limited to the point of the surface corresponding to the softened part	1 ...
General suffusion of pia mater	2 ...

In thirteen cases of M. Andral's publication, there were only five in which the pia mater attracted attention by undue vascularity; so that among thirty-seven cases, six only furnished examples of very notable injection. The pia mater would appear from these facts to exercise very little influence in the production of softening.

M. Fardel's observations (as, indeed, we should have stated before) were made upon subjects of advanced age, in whom, as is well known, atrophy of the convolutions, (a far from uncommon state, and admirably described by Dr. Carswell,) is accompanied with *accumulation of serosity* in the anfractuosities. When M. Fardel found this state of atrophy coexistent with serous effusion in the pia mater in cases of softened brain, he correctly regarded the effusion as unconnected with the softening.

When, on the contrary, the convolutions are of natural size and development, he considers it certain that, if serosity be present, it depends upon a morbid action, and is not produced simply to fill the space which would otherwise be empty from the shrivelling of the convolutions. M. Fardel regrets that he cannot in the present place disclose his reasons for adopting this view of the relations of the effused fluid and diminished volume of the convolutions in senile atrophy: we regret it also, for this is precisely the most interesting and debateable point at which the author has yet arrived in the course of his descriptions. No allusion is made to Dr. Carswell's opinions on this subject.

The more closely the liquid approaches simple serosity in its characters, the less important is it pathologically. The gelatinous-looking appearance it sometimes presents depends simply on its being infiltrated in the meshes of the cellular membrane lying between the arachnoid and pia mater. M. Fardel did not meet with a single example of accumulation of serosity in the pia mater having such characters as to prove its origin recent, nor in any case did he discover pus; the cases published by M. Rostan give a similar result; and in a single one only of the thirty-three published by M. Andral was a small quantity of slightly turbid serosity found under the arachnoid. These facts seem to demonstrate a much greater amount of independence on the part of acute softening and affection of the meninges than we should, à priori, have been led to expect. M. Fardel differs altogether from those authors who regard the thickening and opacity of the arachnoid, frequently observed in aged subjects, as capable of explaining the symptoms attending softening during life.

The *adhesions* found between the pia mater and brain are of three kinds, according to this author; they are produced by: 1, a sort of viscosity poured out between them, when no actual liquid is interposed between them, as in cases of compression of the brain and flattening of the convolutions; 2, the vessels passing from the membranes into the brain; 3, old or recent cellular adhesions. M. Fardel appears to have paid attention to a point concerning which much difficulty is frequently experienced at post-mortem examinations, the natural amount of adhesion of the membranes to the subjacent brain at the different points of its surface; we therefore condense the results at which he has arrived. He thinks it may be established as a general proposition, that when the examination is made within forty hours after death, and the temperature not very high (under 77° Fah.), erosion of the surface of the brain, produced in removing the pia mater, is a morbid phenomenon. Cases in which putrefaction has advanced with very notable rapidity are of course to be excepted; and the author would except from the mass of the brain, under all circumstances, the convolutions of the middle and sometimes of the anterior lobe, the natural softness of which renders the separation of the meninges extremely difficult. The meninges are generally easily removable over the convex and lateral surface of the hemispheres; however, some difficulty is met with in detaching them at the union of the upper and internal surfaces of each hemisphere. The firm and narrow convolutions forming the posterior points of the hemispheres are so closely united, that serosity is never found infiltrated between them; the removal of the membranes is consequently very difficult here, for, generally speak-

ing, the facility of removal is directly as the quantity of serosity in the pia mater.

But, as must have struck every one who has raised a patch of meninges and found particles of brain clinging to its under surface, this adhesion may depend either upon the cohesion of the cerebral substance being so much diminished, that it yields under the traction of a naturally adherent pia mater, or upon increased firmness of adhesion of this membrane. Here is a difficulty of which we have often felt the importance. M. Fardel does little to remove it. In general, in cases of acute softening, unless when the disease was very recent, he fancies he found true adhesions, (not of plastic lymph except in the rarest instances,) "consisting in a rather close agglutination of the softened pulp to the pia mater." This is anything but clearly intelligible.

Seventeen cases of acute softening, the great majority of them reported by the author, next follow; and upon the information furnished by these he proceeds to inquire into and minutely consider the nature of the essential elements of the anatomical changes we have already passed in review; congestion, sanguineous infiltration and inflammation.

Congestion of the brain is anatomically characterized in the gray substance by uniform pinkish discoloration, (the author ought to have distinctly said *inflammatory* congestion;) in the white substance by punctiform red injection, and sometimes by patches of reddish staining. M. Fardel considers that it is easy to distinguish active congestion from inflammation in the brain as elsewhere, by the circumstance that in the former state the natural consistence of the part is either natural or slightly increased. MM. Bouillaud and Gendrin, (both fanciful persons, more especially the deputy for Angoulême,) appear to have been the sole observers of an *increase* of the natural consistence under these circumstances. Tumefaction indisputably occurs in simple congestion.

"*Sanguineous infiltration*" is introduced to our notice with a very remarkable flourish of trumpets, so loud and prolonged that we fancied in our innocence the excellent author had discovered a key to the whole pathology of the brain. But the thing is not altogether new, seeing that, according to the author himself, it has been described by Cruveilhier as capillary apoplexy, by Rostan, by Lallemand, MM. Fantonelli, Bravais, Didey, and many other distinguished Frenchmen, whom no one ever heard of before, and seeing further that his first case is taken from Dr. Abercrombie's book. But all these men, with the exception of MM. Fantonelli, &c., were wrong, according to our learned friend, in not studying the anatomical state in question as a distinct and separate condition capable of existing independently of all others. In 1840 appeared M. Fardel's own thesis, which contained "all that medical science possessed on the subject," (that is, according to the impartial estimate of its author,) and numerous original observations.

"Sanguineous infiltration" occurs in very different forms. Sometimes small collections or spots, "smaller *or* larger than a millet-seed," (why we could have guessed *this* ourselves,) scattered here and there through the brain or grouped together, occurring most commonly in flat patches on the surface of the convolutions, characterize this morbid state. In other cases rounded masses, about as large as a nut, single or in numbers,

are observed. In a third form, "which may be called diffuse infiltration, the latter has no determinate appearance, and presents the greatest variety of aspects and sizes." All these forms are characterized by the presence of blood in the cerebral substance, and mixed, more or less, intimately with this. Afterwards, we are told, the blood is "combined with the cerebral substance," (p. 61;) as if this condition and a mixture of the blood with broken-up brain were one and the same thing. Be this as it may, M. Fardel regards sanguineous infiltration of the three kinds described, "as the result of congestion accompanied with the rupture of vessels;" we cannot for the soul of us see (viewing the matter in the light he does, and speaking of broken-up brain,) how he could regard it as anything else. The only point which it would have required the least perseverance or acumen to determine, namely, whether any, and if so what, particular state of the vessels causes rupture to occur in some cases as a consequence of congestion, while in others it passes off without any such result, the author avoids examining, because he might be led thereby into a discussion rather foreign than otherwise to his subject. We apprehend, if he had known much about the matter, he would scarcely have been deterred from an exhibition of his knowledge by any qualms respecting prolixity or want of connexion with his main subject. But the reader will observe that the occurrence of simple transudation through the vessels is not considered.

The condition of the brain affected with this species of hemorrhage, for it is nothing more, however pertinaciously M. Fardel may strive to decorate it with a new title,—varies in respect of firmness. The affected part may be of natural consistence, harder or softer than in health. The induration produced in this way is worthy of consideration, because M. Cruveilhier, with a want of judgment scarcely worthy of his reputation, ascribes it to inflammation. It is obviously produced in precisely the same way as the induration in pulmonary apoplexy; the pouring forth of a quantity of liquor sanguinis into a circumscribed space, amidst the intervascular spaces of a tissue, could scarcely produce any other *primitive* result, provided only the quantity extravasated were sufficiently abundant.

Blood being once infiltrated into the cerebral substance, inflammation generally follows around it; an inflammation produced either because the primary congestion, inflammatory in its nature from the first, follows its natural course to inflammation, or because the effused blood acts as an irritating agent on the tissue into which it has passed. Once softening is developed, "sanguineous infiltration" becomes a state of secondary importance in general, and its history but a part of that of encephalitis and of softening.

A morbid state which always commences by congestion or sanguineous infiltration, is essentially characterized by softening and often attended with tumefaction, establishment of adhesions, &c., must be considered inflammatory in its essence; *red* softening is, in other words, an evidence of encephalitis. And not only of encephalitis, but, according to M. Fardel, of this affection in its *acute* stages; *colourless* softening being the same malady in a *chronic* state. If the first part of this proposition be old, and the doctrine commonly professed, not so the last; this is indeed the most important point started by M. Fardel, and, as we advance, we shall

see the amount of evidence adduced to substantiate a view, certainly in its details at least, both novel and important.

It is difficult to assign very accurate limits to the acute stage; the author nevertheless is enabled to conclude from the analysis of a number of cases that redness very rarely disappears before the twentieth, or remains apparent after the thirtieth day. Acute softening may in some rare cases exhibit itself without redness; in some of these the colour is yellow, as before described, in others suppuration exists.

In many cases of softening of old standing, no trace of vascularity can be discovered; in the majority such traces are very slight; in some, however, numerous well-developed vessels exhibit themselves. These statements are justified by the examination of upwards of 200 cases, the product of the observation of Rostan, Andral, Lallemand, Raikem, and the author. Among this large number of cases there were only four evident examples of chronic softening accompanied with redness; and these only of acute softening unattended either with redness, yellow discoloration, or suppuration. To the question of the anatomical characters of "chronic softening," we shall return by and bye.

Symptomatology of the acute disease. All cases of acute softening may in respect of their symptoms be referred to one of two groups: the one characterized by weakness or abolition of the cerebral functions, the other by perversion or excitement of these functions; in the one case the disease follows the course of hemorrhage, in the other of meningitis. Further comes a number of cases "presenting an association of the characters of both groups, and capable of being placed indifferently in one or the other of them."

Beginning with the beginning, M. Fardel inquires into the correctness of the statements of Professor Rostan respecting the *premonitory symptoms* of the disease, symptoms made by that writer to constitute the first of its "two periods." These premonitory symptoms, according to Rostan, are cephalalgia, vertigo, weakness of the intelligence, changes of temper, tendency to sleep, numbness, formication, sometimes stiffness and pain in the limbs; on other occasions delirium and general agitation; lastly, still according to the same writer, mental alienation and senile dementia often precede softening, to which must be added impaired vision amounting even to perfect blindness, tinnitus aurium, &c. Among these symptoms, put forward as premonitory, appear some, we perfectly agree with M. Fardel, which are evidence of the actual existence of the disease itself, of the existence of softening; for example, delirium, stiffness of the limbs, loss of vision, &c. Others among them, as cephalalgia, vertigo, tendency to sleep, numbness, &c., do actually occur before the development of the disease, and are premonitory; but they may be absent altogether, and cannot under any circumstances be regarded as signifying a *period* of the disease. It is evident, too, that these premonitory symptoms, depending as they do upon cerebral congestion, a state which may precede all varieties of affections of the brain, cannot where they exist have any direct importance as signs of coming softening. Nor are they important from the frequency or regularity of their occurrence. The cases in M. Rostan's book furnish a greater number of examples of absence than of presence of such symptoms, and M. Fardel's own experience, as condensed in the page before us, justifies the asser-

tion that, as is the case with almost all acute maladies, premonitory symptoms may occur in softening, but do not constitute, in the instance of either a special and characteristic period of the disease. In numbers of M. Fardel's cases the affection set in perfectly suddenly; and if it be admitted that in some of these instances premonitory symptoms may really have existed, but not been noticed by the friends or others who were alone able to give any account of the patients, (and this *was* probably the fact, as M. Fardel admits,) it must surely in turn be conceded that symptoms attracting so little attention cannot *practically* be esteemed of great importance. Andral distinctly admits that premonitory symptoms may be absent in the phrase, "softening *may* exhibit a premonitory stage." (Clin. Méd. t. v. p. 582.) We are anxious to impress these facts upon the minds of our readers, because we have met with many persons who conceive that softening is to be readily distinguished by the character of the attack as respects suddenness or gradual development; where a great number of cases of softening and of hemorrhage are compared in respect of this point, no very distinctive character, concludes M. Fardel, is observed in one or the other category. Long since we instituted, for our own information, a similar comparison, and came to a precisely similar result. The author adds, however, that numbness, cramp, formication, weakness, when limited to one side of the body, or especially to one limb, are of all signs the one most valuable as premonitory of softening; a statement, nevertheless, very materially qualified by the addition that when observed they commonly belong to softening actually established. A final corollary from his facts is given by M. Fardel as follows: when acute symptoms declare themselves in an individual already labouring under chronic softening, it is very probable that they depend on superadded acute softening, because the occurrence of hemorrhage under these circumstances is extremely rare.

Invasion. In some cases the disease sets in by general or partial weakening of the intellectual faculties, of movement, and of sensibility. These phenomena lead gradually or by sudden advances to total annihilation of these faculties and properties. In other cases the symptoms of softening set in instantaneously and are constituted by the simultaneous appearance of the same series of phenomena; here is the *apoplectiform* variety. In certain instances, the softening announces itself from the commencement by phenomena of excitation, by spasmodic symptoms connected or not with those of paralysis or collapse: and these phenomena may affect the intelligence only, or the functions of movement alone, and consist, for example, of epileptiform attacks, but such symptoms are far from occurring in the greater number of cases. In 137 cases, collected from his own portfolio and from several other sources, the invasion of the disease was apoplectiform 79 times; 58 times it was of a different kind.

But the apoplectiform variety being considerably more common in the cases of Rostan and of the author, who observed only subjects of very advanced age, than in those of Andral and of others whose experience bore upon subjects of all ages, from ætat is fifteen upwards, it is natural to inquire whether the frequency of that form in the cases of the former writers may not depend upon the age of their patients. This question must be answered in the affirmative, as some other considerations, which it is needless to explain, satisfactorily show.

Disorders of movement next engage the writer's attention. In almost all cases of softening paralysis occurs, commonly limited to one side of the body, sometimes to a single limb. Complete or incomplete, it is occasionally accompanied with persistent contraction (contracture),—a contraction which may amount to no more than slight stiffness, or be so powerful as to be with difficulty overcome by the observer. Stiffness of the joints may occur on the nonparalysed side; under these circumstances, or when the paralysis is very imperfect, there is a fear of mistaking a voluntary or simply automatic contraction, if the intelligence be deeply affected, for a morbid stiffness. Instead of paralysis convulsions may be observed, sometimes general and simulating those of epilepsy, sometimes partial; in some instances tetanic contraction, or simple muscular trembling, &c. These varieties of morbid mobility may alternate with paralysis, precede, follow, or exist without it: sometimes they are only noticed on the side opposite the paralysis. The relative frequency of these different conditions is shown in the subjoined enumeration:

In 32 cases of acute softening:

Paralysis occurred in	23 cases.	
general in	2	
Simply weakened power of motion in	1	=23.
Paralysis limited to the arms in	6	
affecting one entire side in	14	

M. Fardel, in accordance with common experience, never saw the lower more completely paralysed than the upper limb; whereas the converse he frequently observed. A certain harmony is noticed between loss of movement and loss of intelligence; when the disease sets in by a sudden loss of sense, the hemiplegia is in general sudden and complete, or nearly so. In seven cases only did M. Fardel discover stiffness of the paralysed limbs; in five of these it appeared with the invasion, and had increased in two, diminished in one, and disappeared wholly in two cases by the following day. Numerous other varieties in these respects were observed by the author, but we must refer for them to his own pages.

Several of those who have written on acute softening furnish no examples of integrity of the faculty of movement: on the other hand, Raikem, Andral, Lalesque, Faber, and the author, relate cases in which the intellectual faculties alone suffered.

In every one of upwards of two hundred cases of paralysis depending upon various causes, the author invariably found the loss of motion seated on the side opposite that of the cerebral disease. And though there are, indubitably, cases on record in which both were on the same side, these cases are sufficiently rare to render it perfectly incorrect to speak of their occurrence as in any measure frequent,—which appears, nevertheless, to have been rather recently done by M. Rostan. (Gazette des Hôpitaux, 12 Juin, 1841.)

The section on the *state of sensation* in acute softening is introduced by some observations upon the difficulty of forming an accurate judgment respecting the condition of that property. Signs of sensation necessarily exhibit themselves and disappear in the same spot at different periods of the day; besides, there is no small difficulty in distinguishing, in certain cases, simply automatic movements from those provoked by pain, or the cries resulting from real suffering from the inarticulate sounds uttered by patients under the influence of an apoplectiform attack.

Sensation, when developed in a paralysed limb, may give evidence of its existence by moans, by grimaces, by the approximation of the healthy arm to the paralysed side, or by movements of the paralysed limb. But M. Fardel is sufficiently acquainted with the writings of Dr. Marshall Hall to know that movement may occur in limbs perfectly paralysed in respect of voluntary motion and sensation. The precise words of the writer may be worth extraction: "It is certain that in a good number of cases in which sensation and motion were so completely abolished in the limbs that a pin might be driven with impunity in its whole length into the flesh, I have seen movements take place, when the same experiment was repeated in the palm of the hand, the sole of the foot, the internal surface of the forearm, or around the ankles; but almost invariably, besides the slight movement of retraction of the limb, the patients gave some sign of sensation, as moans, or more especially a movement of the opposite arm towards the point irritated. It is important to make these remarks, because they suppose [i. e. the facts they refer to,] a connexion between sensation and volition, and, on the other hand, the production of these movements. It has occurred to me several times, however, as was noted in the experiments of Dr. Marshall Hall, not to be able to detect any sign justifying even a suspicion of consciousness of motion on the part of the patients." The manner in which all this is stated shows how imperfectly Dr. Hall's Memoirs are appreciated by the profession generally in Paris, and indeed M. Fardel very patronisingly admits, that "*perhaps* they are not sufficiently known" in the centre of the civilized world, as his countrymen very modestly term their capital town.

Loss of sensation (anæsthesia) commonly coexists in acute softening with that of movement; but is somewhat less frequent than the latter. This proposition is the expression of general experience,—but M. Fardel's own cases, which give nine instances of retained sensation among twenty-three of paralysis, furnish a higher proportion of the former than we believe to be usual.

Exaggerated sensibility of the skin, or deep-seated pains in the limbs, rarely occur in *acute* softening; they are more common, as we shall see, in the chronic disease. In the former affection they are so rare that the author, out of upwards of two hundred cases, has only been able to find some five or six instances in which they occurred. But there are certain modifications of sensibility more frequently observed in acute softening,—these are numbness, formication, and disagreeable or even painful pricking sensations, seated either in the limbs or face, rarely in the trunk. These may occur either as premonitory symptoms, at the commencement or during the course of the disease, when the paralysis is incomplete, or when it has begun to diminish in intensity.

A sensation of deep-seated cold in the paralysed limb is "very common," according to this writer, at the commencement of the disease; sometimes this occurs before any other direct symptom, and if accompanied with even vague and indecisive signs of cerebral congestion, is far from being without diagnostic value.

Cephalalgia, says M. Lallemand, is one of the most constant premonitory symptoms of encephalitis; it continues during the first period of the disease, &c. M. Fardel has very meritoriously exposed the error of this statement, if received without limitation: in sixty-seven cases

collected by him from different sources, cephalalgia was noted only eighteen times, and, in some of these instances, appeared to depend either on the preexistence of chronic softening, or upon some accessory circumstance. M. Lallemand's patients were generally younger, however, than the others, and the encephalitis, in their cases, attended with meningitis. In some of M. Lallemand's cases there existed disease of the cranial bones, or the softening was traumatic,—all circumstances manifestly modifying the pathological character of the malady. The general result would then appear to be, that cephalalgia is not a symptom of the great importance which it is usually believed to be; "it is frequently absent, and when it does exist presents no peculiarity either in respect of seat or nature." Yet assuredly the next sentence of the author very seriously qualifies the signification of this latter clause: "It is nevertheless certain, that persistent cephalalgia of some intensity, especially if confined to a given spot in the head, may become a valuable means of detecting either a threatened or actually existing softening."

The aberrations of intellect observed in this disease are very various. In some cases, several days before the invasion of softening, the character or the intellectual faculties present some slight modifications; irascibility or sadness, or, on the other hand, dulness and confusion of ideas, have at that period been noted. When the disease is gradually developed, a gradual weakening of the faculties, which may advance to a perfect state of hebetude, or actual coma occurs,—or agitation, loquacity, delirium. Coma, in other instances, is established from the first, at least such is M. Fardel's experience, in opposition to the statement of Rostan, that it scarcely appears until the second stage.

The disease may set in by sudden hemiplegia, without any change in the intellectual faculties; this is rare, but less so in softening than in hemorrhage,—a point of diagnosis to which we shall return.

The frequency with which softening declares itself by loss of sense, and coma, from the first, has led many authors to speak of the disease under the head of apoplexy,—one illustration among a thousand of the grave errors committed by *soi-disant* practical men, who content themselves with the merest outward similarity as evidence of the identity of diseases. "The treatment is the same," say these wiseacres; "what matters aught beyond this?"

In all cerebral affections the *condition of the face* is of extreme importance, both in respect of its expression and of the position of its different parts. In speaking of the deviation of the commissure of the lips, M. Fardel notices a source of error in diagnosis worth being borne in mind: "When the teeth fall out the mouth habitually loses all symmetry, and is drawn or rather falls towards the side upon which are the smallest number of teeth. I have seen more than one person embarrassed, or even led into the commission of error, by this circumstance."

M. Fardel notices as a condition which he believes undescribed, "a notable increase of secretion from the follicles of the mouth and eye," and which occurs frequently in acute softening and congestion, less commonly in hemorrhage. This morbid state of secretion, he alleges, improves at once with an amelioration of the other symptoms.

The state of the speech of subjects affected with acute softening is generally well known; a peculiar state, in which the patient appears

not only to have lost the faculty of articulating, but of uttering a sound even, has once at least been noticed by the author. We have also seen this.

When the patient is in a state of complete coma, *the senses* participate in the general abolition of the intellectual faculties. But, under such circumstances, it is not so much abolition of the sense, as loss of consciousness of the impressions transmitted by its organ to the brain, that the observer ascertains the existence of; and, except under these circumstances, the senses do not appear to suffer materially. M. Fardel appears never to have observed deafness, unless where the patient was in a state of coma. Sight is sometimes, but rarely, lost on the hemiplegic side; and it may be perfectly retained in spite of immobility of the pupils.

The condition of the pupils is made the subject of lengthened examination, introduced by the observation that in the natural state the pupils are generally very, sometimes excessively, small in advanced age. The statements of Lallemand, Carswell, and the cases of Rostan, Andral, and his own, are analysed, and we cannot exhibit more faithfully the general result than by transcribing the account of the state of the pupils in seventeen of the author's own cases.

Pupils natural in	3 cases.
contracted	1 ...
narrow	1 ...
moderately contracted, or rather narrow	3 ...
very much dilated	1 ...
dilated, or moderately dilated	5 ...
on paralysed side dilated; contracted on the non-paralysed side	1 ...
dilated on the non-paralysed side; the cornea being opaque on the other	2 ...

They were besides noted as immoveable in six cases. The pupils are not then habitually contracted in acute softening, as Lallemand and Carswell teach; nor does this contraction or dilatation (as it appears from the facts referred to) present any constant relation to resolution or contraction of the limbs. It is important to observe, that the observations were made at the commencement of the disease, precisely at the period when contraction should, according to the above pathologists, be most obvious. Lallemand has also stated, that the pupils are generally dilated in cerebral hemorrhage. Thirty-one cases (Rochoux ten, the author twenty-one,) give the following results upon this point:

Pupils contracted, or narrow in	18 cases.
dilated	9 ...
natural	4 ...
moderately dilated	3 ...

Hence it would appear, that if any positive diagnostic inference were to be drawn from accurate experience on the point, it must be that their contraction would announce hemorrhage rather than softening; but we perfectly agree with the author in the sageness of his conclusion, that the state of the pupils supplies no means of distinguishing the two affections.

The pulse, unless in the case of complication, deviates, in the majority of instances, in no particular way from the natural condition. In four only of M. Fardel's cases did fever exist, and in one of these there was pneumonia in the third stage; in the others the state of the gastro-intestinal mucous surface appears not to have been ascertained. Perhaps the latter may be a hypercritical objection in the eyes of some, as there

do not seem to have been any notable abdominal symptoms; but cerebral affections so blunt the sensibility in respect of these, that it is not without its importance.

In the brief sections on respiration and nutrition, we discover nothing very remarkable.

The duration of acute softening, terminating by death, is calculated as follows from fifty-nine cases, of which twenty-seven are the author's own, sixteen M. Rostan's, and the same number M. Andral's. Death occurred

Within the first forty-eight hours in	11 cases.
Before the fifth day	26 ...
Before the ninth day	43 ...
From the ninth to the twentieth day	7 ...
From the twentieth to the thirtieth day	9 ...

In a brief section the author gives a condensed view of the progress of acute softening, when following each of five types in respect of its symptoms. These descriptions are most graphically drawn up, and we cannot refrain from extracting one as a specimen of all,—it is of the apoplectic form of the disease:

"...... patients suddenly losing the use of speech, of motion, of intelligence, either after vague suffering of some kind, or without any previous indication of the coming malady whatsoever; extended on the back, the face marked by an expression of hebetude, the features distorted, the eyelids closed or scarcely open; they utter not an articulate sound, and occasionally plaintive murmurs alone give evidence that they are not entirely dead to the external world. On one side of the body their limbs, paralysed and inactive, lie extended and flaccid beside them, or flexed and uselessly contracted, almost always deprived of sensibility; the limbs of the opposite side, on the contrary, are moved either from simple restlessness, or in consequence of pain being excited by medical examination, or to perform some act which their remnant of volition permits them to attempt. After some hours, or a few days, a slight remission of these symptoms is generally observed; the eyes open, a certain return to life, if not to sensation, displays itself in the look; a few ill-articulated words are heard; the paralysed limbs recover some little power of movement and sensation. But ere long the respiration becomes embarrassed; the region of the sacrum, suffering under constant pressure, irritated by the contact of the urine and feces, reddens, excoriates, and ulcerates; the powers of motion and sensation are lost again, and now in every part of the body; the faculties of the intelligence and the senses disappear to return no more, and general death soon follows that of the functions of relation." (p. 156.)

Over the other analogous sketches, as well as a valuable section entitled, "Appreciation of the symptoms of acute softening," we are obliged from limitation of space to pass.

The chapter on *the diagnosis* of the malady contains much, which calls for full notice.

(1.) The most characteristic form of softening is that in which the disease commences by gradual alteration of movement and intelligence, attended with perversion of sensation; as cephalalgia, numbness of the limbs, &c. especially when the unnatural sensations in the latter are limited to one side. But if it be true that softening always commences by congestion, we should be inclined, à priori, to affirm that there can be no symptomatic mark of softening at its outset, which shall distinguish it from mere congestion. And such is, in truth, the fact. It is generally

the continuance, and especially the persistent severity of the symptoms that announces the addition of inflammation to congestion. The predominance of the symptoms on one side of the body has been generally set down as distinctive of softening, but M. Fardel shows that this circumstance is of little value when the disease sets in suddenly by an apoplectic attack; he considers it extremely rare, however, if the symptoms have affected both sides of the body and been slowly evolved, to discover anything but congestion of the brain or serious meningeal effusion.

As regards cerebral hemorrhage nothing is more rare than a gradual and progressive development or "march" of its symptoms. The whole hemorrhage is not always accomplished at once, but its increase then takes place by *sudden and abrupt attacks.* In this statement of the author we perfectly coincide, and consider it right to point out with him what we believe to be a very serious error of Dr. Copland, (Dict. of Med. vol. i. p. 82,) namely, the assertion that in the "gradually increasing or ingravescent apoplexy" of his classification, "extensive extravasation of blood is always met with."

Hemorrhage into the arachnoid, unfortunately for facility of diagnosis, sometimes follows a course perfectly similar to that of the present form of softening. M. Andral's fourth case, (Cl. Méd. t. v.) furnishes an excellent example of the impossibility of distinguishing such cases of meningeal hemorrhage from softening: luckily this species of hemorrhage, a rare morbid state in the first place, does not often follow the course now referred to; of twenty-four cases only five, according to an analysis by M. Fardel, presented any similarity to the progress of this form of softening.

(2.) We have next to consider the apoplectiform variety of the disease. An apoplectiform attack may be the consequence of various morbid states of the brain, besides softening; of congestion (ictus sanguinis),—of meningeal hemorrhage,—of non-inflammatory sanguineous infiltration of the brain; of serous effusion; of purulent meningitis; and, above all, of cerebral hemorrhage. The author, however, endeavours to establish the distinctive marks of softening and of cerebral hemorrhage, and we shall confine ourselves to an abstract of his observations upon this head. The apoplectiform manner of attack in softening is by M. Fardel referred to the congestion (intensely developed) forming its first stage;—a perfectly fair explanation, as "apoplexy" is well known to be the occasional evidence of mere congestion. In these cases then the mode of attack is identical; is their subsequent course different? Cruveilhier, (Anat. Path. livrais xxxiii.) believes that in some cases the diagnosis is only to be made in the dead-house. We shall one by one pass in review certain circumstances said to be more or less distinctive.

a. It has been said that in hemorrhage the symptoms acquired from the first their maximum intensity, whereas in softening they commonly increase by degrees. The statement is true in a certain number of cases; but its fallacy, if made of general application, is well exhibited by M. Fardel. "When," he remarks, "general congestion supervenes abruptly, and partial softening takes place, while the patient is still under the influence of the congestion, death occurs in some cases before this latter has disappeared, and the proper symptoms of softening must then have been masked by it; in other cases death is slower in occurring, and to the

general symptoms of congestion will succeed the partial symptoms of softening,—now under these latter circumstances especially, the successive diminution of the symptoms perfectly simulates the course of hemorrhage." (p. 187.)

b. The existence of precursory phenomena has been considered of great value as a guide to the diagnosis of softening; but as commonly understood, we have already seen from M. Fardel's researches, these phenomena are nothing more than symptoms of an actually existing softening of the gradually developed form, quite a different one from that at present under consideration. He himself is inclined to believe, but he advances this with reserve, that true premonitory symptoms are more common in cerebral hemorrhage than in apoplectiform softening.

If convulsive movements be observed after an apoplectiform attack, the case may be set down as one of softening; however, in order to make this proposition strictly correct in respect of *primary* softening, it is necessary to add that the convulsive movements occur at the outset, if at a later period they might depend upon a *secondary* softening developed around coagula.

It has been said that if the senses be preserved at the moment of attack, the case cannot be one of congestion or hemorrhage; some rare facts show that even this proposition is not universally true, but unquesionably if the intellect be unaffected in an apoplectiform attack, the presumption in favour of its depending on a softening is exceedingly strong.

If an individual labouring under an apoplectiform attack, give evidence of spontaneous pain, or of unnatural sensibility of the skin in the paralysed limbs, the case is certainly one of softening. But these phenomena are very rarely observed in the present form, and belong to the "gradually increasing" and "ataxic" varieties. Retained sensation simply is by no means a distinctive sign of softening; in fifty cases of the disease collected by the author, MM. Rostan and Andral, the cutaneous sensibility was only retained in twenty, in the remainder either entirely impaired or altogether abolished; in twenty-one cases of cerebral hemorrhage by the author, it was retained or but very slightly impaired ten times, completely paralysed in the remaining eleven.

Persistent contraction (*contracture*) has, as is well known, been set down as almost pathognomonic of softening, and its connexion with cerebral hemorrhage scarcely investigated at all. We long since observed cases exposing the complete fallacy of the exclusive doctrine referred to, and the observations of M. Fardel perfectly coincide in nature with, and have much more numerical precision than our own. Now of forty-seven cases of acute softening attended with paralysis, (Rostan, Andral, the author,) *contracture* attended only thirteen; whereas the author finds this symptom noted in nineteen of twenty-nine cases of cerebral hemorrhage observed by himself. But this latter position requires qualification. "Whenever," says M. Boudet, in his treatise on Meningeal Hemorrhage, "the cerebral substance alone is implicated in hemorrhage, so long as no inflammation arises around the effused blood, no *contracture* occurs. But whenever, in addition to lesion of the cerebral substance, rupture of the walls of the ventricles takes place and consequent effusion of blood into those cavities, or on the surface of the brain, *contracture* follows."

This very interesting statement M. Fardel regards as generally true, and upon it may, in many cases, be satisfactorily established the diagnosis of secondary ventricular hemorrhage. Now some propositions respecting the general question before us may be deduced from what has just been said.

When slight apoplectic symptoms coexist with *contracture*, the diagnosis must be softening; because, the fact of *contracture* existing excludes the idea of hemorrhage into the substance of the brain alone; and secondly, the slightness of the symptoms is incompatible with the idea of ventricular hemorrhage from rupture. Again, when symptoms of much severity, announcing a considerable amount of compression, are not accompanied with *contracture*, we are justified in affirming that we have a case of softening before us: because the absence of *contracture* excludes the idea of ventricular hemorrhage; and the severity of the symptoms seems incompatible with the mere existence of effusion into the proper cerebral substance. These marks of distinction are, as M. Fardel admits, minute and perhaps in not a few instances destined to prove far from satisfactory, nevertheless they appear the only ones that can be established; and, as he very justly observes, it is not his fault that the line of distinction between the two maladies is not more clearly defined.

(3.) When acute softening follows the "ataxic" course of M. Fardel, that is, when phenomena of paralysis and excitation, &c. are combined, there is no chance of confounding the case with one of cerebral hemorrhage, but the similarity to meningitis is most puzzlingly close.

In a certain number of cases of softening delirium is the only cerebral symptom; the same is true of meningitis; are there any means of predicating with certainty the source of the delirium in the two instances? None of a positively distinctive character. Nevertheless it is true that the delirium of meningitis is more violent, more noisy than that of softening; the febrile symptoms are more intense in the former than in the latter; and other head symptoms, cephalalgia, photophobia, &c., more prone to coexist, and more severe when present. The age of the subject under observation will sometimes be of material service in guiding our judgment; it is certain that acute meningeal affections are singularly rare in subjects of advanced age, and that in children the constant tendency of encephalic inflammations is to occupy the membranes.

Convulsions, *contracture*, muscular trembling, pains in the limbs, and impaired motility occur too frequently in meningitis to be of great utility in fixing the diagnosis on softening; but if limited to one side of the body they are useful, because in meningitis they almost invariably affect both. In the exceptional cases of meningitis in which it has been found that such symptoms were limited to one side, (Andral, Cl. Méd. t. v. p. 42,) there has always been, we venture to affirm, as in the case now referred to, local cerebral congestion, that is an early stage of softening, to account for it.

M. Fardel's general conclusion is that when either of the two morbid states exists alone, "the general aspect of the patient and the habit on the part of the physician of seeing such cases will be more useful in supplying him with a diagnosis, than all the rules that could be traced beforehand."

CHRONIC SOFTENING. We have now brought to a conclusion our analysis of the first part of the author's volume, and completed the subject of acute softening; the second part introduces us to the chronic form of the malady.

Chronic softening exhibits itself under certain anatomical forms, which the author refers to successive periods of development: the first period (A), pulpy softening; the second period (B), marked by different conditions according as the convolutions or deep-seated parts are implicated; a third period (C), distinguished by the disappearance of the softened pulp. We proceed with the author to consider each of these periods in detail.

A. When softening passes into the *chronic* state, it consists at first like the acute disease, in a simple diminution of consistence, but is distinguished from it by the absence of redness. Those acquainted with the general state of opinion on this subject will at once recognize the fundamental difference of this mode of viewing the nature of white or colourless softening of the brain from those of preceding writers. We simply draw the reader's attention to this point for the present; the progress of our notice will fully develop the subject.

The appearances in white softening were, as is well known, pertinaciously attributed by Lallemand to infiltration of the cerebral substance with pus. It is sufficiently remarkable that a dead white colour and a bright orange yellow, (which we have seen really depends upon infiltration of blood,) should have been both referred to the same cause, purulent infiltration; but this is not theonlypoint in his doctrines which indicates facility in theory, rather than soundness of logic on the part of the Montpellier Professor. Softening or inflammation of the brain we believe M. Fardel to be perfectly correct in affirming is most rarely attended with suppuration, unless when the effect of injuries or disease of the cranial bones. Simple infiltration with pus does not occur in chronic encephalitis; if pus exist under these circumstances, it is collected into abscesses.

Microscopical examination was, as the readers of this Journal are aware, applied some while past by Gluge to the substance of softened brain. We analysed his paper, (Brit. and For. Med. Rev., vol. X, p. 259, July, 1840,) rather because it recorded the results of the first attempt of the kind, than in compliment to the fulness of the investigations or soundness of the conclusions reported. Both are examined by M. Fardel, and the complete want of harmony between Gluge's first conclusion, "pus is present in many cases of white softening," and his facts made (ex ore suo) obvious. Not contented with this, M. Fardel and "his friend Dr. A. Becquerel," must examine for themselves the micrology of the matter. And a precious business the two friends made of it; but not a whit greater is the absurdity of their descriptions than might be anticipated from two babes in the wood like themselves, with just as much knowledge of the microscopical anatomy of the brain, as of last month's fashions among the belles of the moon. There is assuredly something *sui generis* in the unblushing vanity of a Frenchman; fancy, if you can, a young man belonging to any other nation in the world but the *grande* one, taking up a microscope, perhaps for the first time in his life; for aught we know, or he proves, turning it wrong side uppermost as he used it; applying it to the investigation of a most complicated mor-

bid state of the tissue, the most difficult, even in the natural state, to unravel in the whole body; printing a page of his comically absurd "results," and then coolly telling you at the end of it, "I dare not place *too much* confidence in results so different from those obtained *up to the present time* (!) by different micrographers,"—Valentin, Müller, and such small people alluded to by name. By all our—but we must not disgrace the pages of the "British and Foreign" by swearing; we swallow our wrath and pass on.

The redness of acute softening is produced by congestion or infiltration of blood; in the latter case the chronic disease will exhibit a yellow colour, deep in proportion to the quantity of blood infiltrated: in the former the congestion must disappear after a certain time, and the softened tissue recover its natural hue. And not only will the injection diminish, but the very vascularization of the part; the vessels appear in some cases to disappear altogether. Hence parts in a state of chronic softening never—setting aside some infinitely rare examples—present a red colour or become the seat of hemorrhage. In some instances softened parts in which the disease is of old date, present an interlacement of long and good sized vessels, but this is a different thing from a true capillary plexus. According to M. Fardel then, white softening is merely colourless softening, or softening of the part without any unnatural colour being superadded; chronic softening in the gray substance may consequently be of gray, but never of white colour. The same notion was taught by M. Dechambre. In the latter situation, from the frequency of infiltration of blood, it is generally yellow.

Pulpy chronic softening occurs in all parts of the brain: but is less commonly observed on the surface of the convolutions than elsewhere, on account of the rapidity, according to the author, with which the disease in this situation tends to pass to a more advanced stage.

General softening of the brain, a condition which when not preceded by cerebral symptoms we believe to be of cadaveric origin receives some consideration at the hands of M. Fardel; but no new light is thrown by him on the subject.

B. Portions of brain in a state of chronic softening may remain for almost an indefinite period in the pulpy condition just described; but this is rare. At least, according to M. Fardel, when the duration of softening is prolonged, the softened part undergoes certain transformations. These transformations are of a completely different nature in the cortical substance of the convolutions and in the deep-seated parts.

In the cortical substance the tendency is to the production of yellow patches extending over two or more convolutions; flattened; finely tuberculated; remarkably coherent, sometimes so hard as to be with some difficulty torn; on the surface appear very minute vessels, adherent to the pia mater or not. Sometimes *softening*, "less advanced," (a curious contradiction in words, for as every one knows the yellow patch is in a state of induration,) surrounds them; in other cases there is actual destruction of substance in the subjacent cortical substance, encroaching, it may be, on the white. These ulcerations are, according to M. Fardel, the last stage of chronic softening: they are rarely observed; the alleged reason is either that a suspension of the morbid process or the occurrence of death interferes with their production; this is merely stating the

fact in other words. The subjacent medullary substance is rarely healthy under the yellow patches; it is generally in the state to be presently considered. The description here given of the yellow patch of the convolutions contains nothing particularly novel, although the author seems persuaded that it possesses this merit in the very highest degree, while at the same time he admits the morbid appearance has been accurately figured by Cruveilhier, and described by a M. Bravais. Dr. Carswell too has figured the same condition.

We have next to consider the changes in softened medullary substance. The medullary substance disappears over a variable extent of surface, and its place is found filled by loose cellular tissue, exhibiting irregular meshes filled by a turbid and whitish liquid, mixed or not with flocculi which appear to be nothing more than fragments of cerebral pulp.

Of the frequency of this condition, to which he gives the name of "cellular infiltration," the author is persuaded, he himself possessing more than forty cases of the kind, and believing that in some additional instances it must have escaped him. The general seat of this state is the white substance of the hemispheres; it frequently occurs also in the corpora striata, but the author has never met with it in the optic thalami; nor has he seen it in the cortical substance of the convolutions, the secondary changes in which locality have already been shown to be of a different description. It occurs in the cerebellum, also in the medulla spinalis; and is, to say the least, like softening itself, extremely rare in the medulla oblongata. The extent of "cellular infiltration" varies from that of a nut to a mass the size of one or two lobes.

The cellular tissue of the part appears to M. Fardel to be the natural cellular constituent of the brain, exposed by the destruction of the nervous substance. It exhibits itself in the form of filaments, threads, bridles, crossing the space in which it exists, in all directions. It is generally of white colour, though sometimes a little grayish; sometimes it exhibits a slightly yellowish tint. Vessels generally of large size are seen in it in the greater number of instances; as before explained a capillary plexus is not discernible.

In consistence this tissue varies. It may be extremely delicate and friable, which indicates in the author's estimation that it is itself about to disappear as a further phasis of evolution of the disease: or in other cases it is so hard as to offer the resistance almost of fibrous tissue; which state the writer regards as an evidence of a species of cure, "the sole one possible in the case of lesions so advanced." The size of the meshes, depending as it does upon the amount of absorption both of the nervous substance and of the cellular tissue, supplies a measure of the advancement of the disease; the larger they are, the further has the morbid state advanced.

The fluid filling the meshes (whey-like fluid of Andral, milk of lime of Cruveilhier,) resembles very closely in general appearances the liquid with which it is compared by Cruveilhier. We consider it unnecessary to rehearse the author's reasons for not considering it purulent; as its characters are in every respect opposed to those of matter of that description. The author regards it as the produce of a complete liquefaction of cerebral substance; a notion, he says, confirmed by the results of experiment,—he has found that cerebral matter triturated with water yields a

fluid precisely resembling that now under consideration. He appears to consider that there is no serosity thrown out into the softened part, but that the whole of the fluid arises from simple liquefaction of the brain; this we do not believe; that it should be so, is as incomprehensible as opposed to the general law, well known to pathological anatomists, affecting the progress of things in the brain, whenever a tendency to empty space in the cranium is produced by changes in that viscus.

The author indulges in a very characteristic fling against pathologists in general, for their remarkable ignorance of the condition which he has baptised with the name of "cellular infiltration." Upon this we beg to offer a few words. It is utterly and wholly irreconcileable with fact that the total description given by the author was not known before he wrote. There is not a syllable in the mere anatomical facts he relates, which we did not know as well before we saw his volume, as now after its perusal, and *we* learned what we know from writers all of them occasionally (especially when any of their doctrines can be twisted into food for censure,) quoted by M. Fardel, from Lallemand, Foville, Andral, Cruveilhier, Carswell, and above all the late Dr. Sims, whose admirable papers in the Medico-Chirurgical Transactions contain all that we have yet seen of *positive* information upon this particular state of the brain after softening. As regards the catenation of the different morbid states taught by M. Fardel, and which may be novel somewhat, that is another point. Then in respect of the name he choses to give the condition, we venture to affirm he could with difficulty find two words less capable of accurately expressing, or less suggestive of the state than "cellular infiltration."

We pass over some intermediate matter and take up the pages of our author again, where we find him engaged in the discussion of a very interesting question in morbid anatomy, the distinction between hemorrhagic cavities and those produced by softening. His tendency to self-glorification leads him at the very outset of the inquiry into an obvious mistatement, "the morbid changes I have considered [cellular infiltration and chamois-leather patches] have been *up to the present time* generally considered as traces of former hemorrhages." Now if he had said they were *formerly* so considered, he would have been right; but there is not a single person whose general acquaintance with morbid anatomy is sufficient to place him in the class of well-informed physicians, that does not know sufficient of the researches of the writers named a few lines back, to be alive to the fact that the changes in question are not necessarily the result of hemorrhage, and are sometimes at least that of softening. This is putting the matter in the least dogmatic way; but it may be inquired with M. Fardel, whether the changes in question can at all, under circumstances of a certain kind, be ascribed to hemorrhage? To examine this question requires a preliminary study of the successive periods of cerebral hemorrhage. In this M. Fardel engages; and we shall follow his example. First, however, the author informs his readers to whom is due the gradual establishment of the notion that all cavities found in the brain are not the result of former hemorrhages: the examination is just what we should have expected, the name of Sims carefully omitted, although elsewhere as carefully quoted, where there seemed a possibility to the honest author to connect its mention with error on the part of its owner. But we forget Sims was not a

Frenchman, and upon earth what matters it, then, what he said or did?

One form of hemorrhagic cavity, according to general descriptions, is that in which the cavity is traversed by filamentous bridles in various directions. The existence of these bridles appears to M. Fardel to exclude the idea of hemorrhage, for the following reasons: If blood be absorbed so perfectly as to leave no trace of its presence, it must generally leave a cavity, accurately circumscribed, perfectly free from cerebral substance. If the walls of this cavity approximate, however, they may become united by means of adhesions; but in this case the said adhesions will retain the walls in contact with each other, and instead of a cavity a cicatrix will form, or at the most, a few isolated bridles may stretch from one wall to the other, but this is a very different thing from the abundant supply of cellular tissue in the cavities in question. The notion that the adhesions are the result of organization of the clot itself, the author rejects as altogether unproved, and affirms that the real course of things in the case of effused blood is, as far as observation goes, altogether different. To this latter point we shall presently return; meanwhile we may state that M. Fardel with much justness objects in toto to the manner in which the conclusion has been drawn, that all cavities in the brain are the consequence of previous hemorrhage. The true method of ascertaining the alleged fact would have been to have observed a certain series of recent hemorrhages proving fatal at different periods, and on the condition of the parts in these cases to have founded the evidence for the statements made respecting the transformation of effused blood, and the generation of cavities divided into meshes by a network of new formation. This, according to M. Fardel, has never been done. The sole observations made, he says, have been instituted on the bodies of persons dying a long period after an attack of apoplexy or something of the kind,—for in the greater number of cases the previous history of the individual is exceedingly vague. Now every one knows the frequency with which an apoplectic attack may be the result of acute softening. There is one natural difficulty connected with the subject, and this is, that hemorrhage rarely destroys life, except at the first or at a very distant time from the attack: it rarely proves fatal (and then generally from fortuitous causes) during the intermediate time. With some difficulty, M. Fardel informs us, he has collected a series of cases, with the date of seizure, of death, and the condition of the blood carefully ascertained. The duration of the disease had in these cases, seventeen in number, varied between thirty days and several years: the increase being gradual from the first case of thirty days' duration to the sixteenth of twenty-one months. Now we cannot extract the details; but we are bound to say that the conclusions drawn by M. Fardel from them are justified by the facts. They show it is impossible to affirm at what time effused blood shall cease to present the characters of that fluid; that this time may be influenced by a variety of circumstances, such as the extent of effusion, the nature of the blood, the general constitutional state, the regimen and treatment, possibly too, to which the patient may have been subjected. And it appears certain from the cases collected, that the physical characters of blood may be recognized in effused fluid several months, a year even, after its extravasation. Hence, then, it

would appear altogether unjustifiable to set down simple cavities containing no blood found in the brain a month, six weeks, or two months, &c., after an attack of apoplexy, as proving that that apoplexy was hemorrhagic. In the case of undoubted hemorrhage, sometimes the solid part of the blood separates from the liquid, so that a hard central mass is found separated by serosity from the walls of the cavity containing it; in other cases, on the contrary, the blood is uniformly converted into a thick muddy matter which liquefies and gradually undergoes absorption, without appearing, according to M. Fardel, even susceptible of organization. We regret that we are unable to condense the very close and judicious criticism to which several cases of alleged hemorrhagic cavities published by M. Rochoux are submitted.

c. The third period of chronic softening is represented by the disappearance of the softened substance. In the yellow patches of the convolutions, these parts shrivel or undergo complete alteration of shape; the cortical layer becomes thin, the yellow patches themselves often eventually disappear, and in many cases all that can be detected is a yellowish discoloration of the surface of the medullary substance. In the softened medullary substance the further progress of things beyond the state of "cellular infiltration" last described, is marked by the gradual increase of size of the meshes, and eventually the disappearance of the cellular tissue forming their walls.

Now in the case of the yellow patches of the convolutions, the appearances produced in their ultimate stage now referred to have been known as those of ulceration of the brain. In five cases of the kind related by the author, these ulcerations varied in diameter from that of a shilling to that of a five shilling piece; were of irregularly round form, having well-defined borders cut perpendicularly to the surface, and equalling in depth that of the cortical substance itself. They were generally covered at the fundus by a membrane which was always smooth, sometimes fine and transparent, or thickish and vascular, sometimes of yellowish hue. The pia mater adhered to them but slightly, and neither it nor the arachnoid presented any particular change in the parts corresponding to them. The medullary substance underneath was either healthy or in a state of pulpy softening or "cellular infiltration." The author signifies in a note that the term ulceration of the brain has generally been applied to an alleged acute morbid change, which he has himself never seen. Nor have we. But we would observe that the chronic condition to which he gives the name of ulceration, as above, was excellently well described by Dr. Sims, and its relationship to softening of the convolutions successfully traced: if the *name* be new, the *thing* then is not so, which strikes us as being rather the more important of the two. Nay, further, we altogether doubt the correctness of the term ulceration as applied to the state described; that the removal of the patch is effected by a process analogous to that of common ulceration is anything but proved, and to say the least extremely doubtful.

The conditions of the medullary substance described by M. Fardel as resulting from the progress of softening after the stage of "cellular infiltration," are, we readily grant him, in all probability, really referrible thereto, and we perfectly acknowledge the justness of his censure of those writers who have indistinctly applied the term atrophy to these and

various other more or less dissimilar states. And it must be confessed that morbid states so extensive and complete as are exemplified in the following case could only be traced to their primary cause by a series of observations of the greatest minuteness and fullness of detail: the mere consideration of the single case could never clear up its difficulties. The case we refer to is given by Andral (Clin. Méd. xv. p. 618). A man died at the age of twenty-eight, of peritonitis; at ætat. three he had fallen on his head from the first floor into the street, and became in consequence paralysed on the left side. Some changes, unnecessary to enumerate, occurred in the state of movement subsequently with the progress of time. But he had never suffered from intellectual deficiency in consequence of his fall; his speech was easy, his intelligence that of ordinary men. The meninges on the right side were transparent and fluctuating; when incised, a great quantity of limpid water-like serosity spouted forth; between the membranes and the lateral ventricle no trace of nervous substance was to be detected, a vast cavity occupied the space between them. This condition M. Andral calls atrophy of the brain, but believes that it was secondary to inflammatory changes supervening on the original fall; M. Fardel regards it as the ultimate condition of inflammatory softening, as the last link in the chain of morbid changes produced by encephalitis, as an absorption, not an atrophy. The previous links in the chain have been successively described.

The seat of chronic softening is next considered and compared with that of acute. In 181 cases of softening, the convolutions and central deep parts were affected in the subjoined proposition; one hundred of these cases were observed by the author, the remainder are borrowed:

	Convolutions.	*Corp. Striata, and Opt. Thalami.*
Acute softening	59	 30
Chronic softening	60	 28
	119	58

The almost identity of the numbers of the cases of acute and chronic changes in each of the above divisions of the brain, establishes in the author's belief a strong presumption that the connexion he teaches between acute and chronic softening is real. Again, the number of instances of softening of the corpora striata and optic thalami, appears precisely half that of those occurring in the convolutions, both in the chronic and acute disease; whereas, as is perfectly well known, hemorrhage is vastly more common in the corpora striata and optic thalami than elsewhere. Now almost all the changes attributed formerly to hemorrhage were seated towards the periphery of the brain, an obvious reason for referring them, as the author has done, (and others, as we have seen, before him,) not to that morbid state but to softening. In ninety-five cases of chronic softening:

The posterior lobe was affected	18	times.
middle	51	...
anterior	13	...
posterior and middle	7	...
posterior and anterior	2	...
middle and anterior	2	...
whole convexity of a hemisphere	1	...
middle line	1	...
	95	

Softening of the corpora striata and optic thalami are included in those of the middle lobe.

As regards the frequency of the two different kinds of lesion in one or other or both hemispheres, there appears to be no great difference observable.

	Softening.		*Hemorrhage.*
Left hemisphere	69	...	43
Right ditto	71	...	40
Both ditto	26	...	6
Middle line	3	...	0
	169		89

We consider it the more important to give these numerical statements, as it has been said by Portal, Gendrin, and Morgagni, that hemorrhage was much more common on the right than on the left side.

Symptoms. Passing to the study of the symptoms of softening, M. Fardel introduces the subject by some considerations, very judiciously put, respecting the difficulties of diagnosis of diseases of the brain generally. To these we shall not refer farther. But we find as additional introductory matter, a disquisition upon the nature and reality of two orders of cases, bearing so directly upon the question of the diagnosis of softening that we cannot pass them by. The first order of cases includes those wherein distinctly apoplectic and fatal symptoms have existed, and no appreciable organic lesion been found to explain them. The second order embraces cases in which softening has been found in subjects who during their life exhibited no symptoms giving evidence of such disease. In the first class of cases the cerebral symptoms appear to be sometimes sympathetic of visceral inflammation; in old subjects, of pneumonia for instance. It has been thought by many persons that in these cases (the nervous apoplexy of older authors) cerebral congestion has really always existed. M. Fardel believes, and we are persuaded he is right, that in a great number of cases of the kind, this is the true explanation of what is observed. For it must be recollected that congestion of the brain may occur under two forms, that indicative of increased arterial circulation, and that marked by an engorgement of the venous system with excess of serosity within the cranium, and paleness of the brain. But if it be true that a certain amount of serosity indicates the preexistence of congestion, how is this amount to be determined, when it is well known that the limits of health are extremely variable in this respect? and certainly there are cases in which nothing in the appearance of the cranium or its contents justifies the notion of previous congestion. Is it fair to suppose with M. Gendrin and others, that the signs of that state have disappeared after death? May we say that the brain has absorbed serosity which had been effused, and that congestion has disappeared in the same way as it is known to do in the case of the skin? M. Fardel thinks it next to impossible to admit the post-mortem disappearance of cerebral congestion, because it has been shown by some experimentalists that the phenomenon of removal of redness depends on the direct influence of atmospheric pressure. As regards the absorption of serosity by the cerebral substance, the experiments of M. N. Guillot appear to render it tolerably certain. He finds that the ventricles are filled and even distended with serosity during life; that the quantity of this fluid is found to have decreased in proportion as the examination is made at a

more distant period from death; that this serosity is recoverable in the cerebral substance; that a piece of recent brain will absorb its own weight of water or serosity; that this property varies in intensity with age and other circumstances. Dr. Paterson, in a recent number of a contemporary journal, has described results remarkably similar to these.

Notwithstanding all this, M. Fardel considers it absolutely necessary to admit that a certain modification of cerebral functions may occur perfectly simulating true apoplexy. Now from this fact he would infer that "when after acute apoplectic symptoms, nothing is discovered in the brain, except softening of chronic aspect, it is wisdom to refrain from refuting the symptoms necessarily to the morbid change detected." This would be all admissible, if the notion that the white softening, to which he evidently refers, is a mere chronic form of the red were thoroughly established; until it is, the above statement involves a *petitio principii.* However we shall see, nay, we have seen, that this doctrine is very likely to be the true one.

The author proceeds to consider each particular symptom of the *chronic* malady. In the majority of, but not in all, cases, the power of motility is more or less completely removed from the influence of the will; in some cases simply weakened or abolished; in others modified in various ways, so, for instance, as to exhibit itself in relaxation, *contracture*, trembling, convulsive movements, &c. Relaxation of the limbs is rarely complete in chronic softening; there is almost invariably some remnant of power of motion retained, enabling the patient partially to withdraw the limb when pricked with a pin or otherwise irritated.

Contracture is more common than relaxation; sometimes it appears at the outset, and disappears to be succeeded by relaxation; sometimes, on the contrary, it follows the latter, precisely as in cases of softening following hemorrhage. In some instances, again, it is of intermittent existence; in others persistent from first to last. It occurs with greater certainty when the disease has been developed slowly. Under these circumstances it first affects one or two fingers, and progressively gains the hand, wrist, and elbow. In its most highly-marked state, the leg is seen forcibly flexed on the thigh, &c. The stiffness generally disappears some few hours or even days before death. This condition may exist in the non-paralysed limbs; but this is infinitely rare. M. Fardel gives a very useful enumeration of the various states of movement observed in forty-three cases:

Movement wholly unaffected in	10	cases.
Impaired power of motion, without actual paralysis	6	...
Spasmodic movements, without paralysis	1	...
Simple relaxation, without *contracture*	11	...
Contracture	16	...

Hence it would follow that, contrary to what is observed in the acute disease, *contracture* is more common than relaxation in the chronic malady.

The motions of the tongue did not appear habitually affected, although articulation often did. This we have observed ourselves also; the difficulty of speech M. Fardel considers produced by forgetfulness of words in the greater number of cases.

Modifications of sensibility are among the first conditions which ex-

cite the attention, when the disease commences gradually. Numbness, formication, or pricking sensations, sometimes extremely disagreeable, are experienced in one or both limbs of the side to be subsequently paralysed. They are, M. Fardel conceives he has observed, more marked when eventually to be followed by *contracture* than by simple relaxation. Sometimes these morbid feelings occur on both sides of the body, whereby their development appears traced to general congestion.

Actual pain is rarely felt in the limbs at the outset; almost always at a rather advanced period, and then associated with *contracture.* Sometimes these pains are excessively severe; they are influenced by change of weather.

Whatever be the amount of *paralysis* or contracture, the sensibility may remain perfectly natural. Under all circumstances, complete anesthesia is extremely rare; and M. Fardel is not aware that anesthesia has ever been observed in chronic softening independently of paralysis of movement.

Cephalalgia existed in twenty-four of fifty-three cases observed by the author or MM. Rostan and Andral; it is therefore more frequent now than in the acute stage. It may exist from the commencement, and even, when the disease follows a gradual course, be its first symptom. It may have existed as a premonitory sign and cease when the disease is actually formed. It varies much in intensity and in character; is almost always frontal, sometimes general, rarely limited to one side of the head. Vertigo is in some patients a symptom of very frequent occurrence; varying in amount from a very slight dizziness to complete loss of senses.

Upon the state of the intellectual faculties we do not discover any information requiring us to stop. The intellect may be almost unaffected, but this is very rare.

M. Fardel has, like other observers, met with *peculiar conditions of speech,* which are perfectly inexplicable. A woman invariably added, at the end of every three or four words she spoke, the phrase "*par le commandement ;*" and this for several consecutive years. A woman, aged sixty-eight, uttered only incoherent sounds, always the same, and forming together the word *simona* or *chinona ;* she heard perfectly, understood what was said to her, but answered everything by this single word, curiously varying the inflection of her voice, however, according to the desire she wished to express. Another patient, whose intelligence is not affected, can say nothing but "*madame été*.......... *mon Dieu* .... *est il possible* .... *bon jour, madame.*" These words she pronounces with perfect precision.

M. Fardel refers all cases of chronic softening to the following four forms: 1st. Softening displays itself from the first as an essentially chronic disease. Attended with a variable number of symptoms, it advances slowly or by abrupt increases, but always progressively. 2d. Softening commences suddenly, like cerebral hemorrhage, of which affection it simulates the subsequent progress with great exactness. 3d. The disease commences without determining prominent or, at least, characteristic symptoms; then suddenly gives rise to derangements of function, which run a very rapid course, and quickly put an end to existence. 4th. Softening determines no appreciable symptom; death is the result

of some malady or circumstance altogether unconnected with the cerebral affection. (Latent softening.)

Nothing can be more variable than the *duration* of chronic softening. No limit can be fixed between one or two months and a great number of years. Many so-called atrophies of the brain (of which we have noted an instance) met with in individuals of adult or advanced age, are nothing more than the conditions produced by softening, which had occurred at a very early period of life. The only proposition we find in the author's book, which, in connexion with the question of duration, may aid the observer in forming a prognosis, is that where softening, passing to the chronic state, makes incessant progress to the time of death, and that this appears produced by it directly, the disease generally proves fatal within a year. But, on the other hand, when softening, and this is very common, ceases to exercise any very evident influence on the rest of the organism, and itself makes extremely slow or almost no progress, death is rarely produced directly by it. In reality, too, there is not much useful information conveyed by these statements.

The cases and reflections which here follow, illustrative of the four different forms of the disease above enumerated, want of space obliges us to pass over without further notice; they will amply repay close study.

We reach the chapter on diagnosis. The outset of the disease, being that of the acute form, has already been considered; what the author investigates in the present place is the method of recognizing the malady at an advanced period of its existence, when the observer may often be unable to obtain exact information respecting the outset, or when an error may have been committed at first in determining on its nature, or when no diagnosis may then have been considered warranted by the character of the symptoms.

We have seen the impossibility in certain cases of deciding, during the first days of illness, whether an existing set of symptoms depends upon hemorrhage or upon softening. Is the distinction always easier at a more advanced period? The essential characters of the progress of hemorrhage tending to recovery appear to be a gradual decrease of intensity of symptoms, until the patient's state becomes stationary, the affected faculties having as completely recovered their integrity as we can suppose compatible with the existence of a lesion cured, but not removed. But unfortunately this mode of progress may exist accurately to the very letter in cases of softening. One of the author's cases in particular proves this beyond question. However, taking a number of cases together fit for the purpose under consideration, the author finds that in the event of cure of cerebral hemorrhage, the following three varieties only are observed: 1st. For a certain period before death, there may be no apparent vestige of cerebral disease. 2d. Speech and mobility do not recover their original correctness and power; this is the most common case. 3d. Hemiplegia or loss of speech remain as complete as at the first; this is the rarest case. In almost all these cases the intelligence is restored, or very nearly so. But what is most striking and really characteristic in cases of this kind, is that a certain degree of weakness or impaired power of the faculties originally affected is observed; never are phenomena of a *different* kind, such as cephalalgia, pains in the limbs,

contracture, convulsions, &c. noticed; when such phenomena occur at least, it is in general easy to ascertain that they depend upon some other circumstance than the presence of a cured hemorrhage.

The distinction of tumours of the encephalon from chronic softening is often far from easy. The author found that in three out of seventy-one cases of tumour (tuberculous, cancerous, or other) of the cerebrum, cerebellum, or membranes, there were no symptoms; of all the phenomena observed in the others, cephalalgia appears to have been the most constant and one of the most characteristic; it was noted in sixty-one of the sixty-eight. This cephalalgia, too, is remarkable for its intensity, which is frequently extraordinary, and for its seat; it was generally limited to one side of the head, in fact, in two cases only was it stated to be general. Now, as we have seen, in softening, cephalalgia is rarely limited to one side of the skull. In some cases, cephalalgia was the sole symptom, until a few hours before death, when convulsions, hemiplegia, or coma supervened. In six cases, blindness gradually came on; M. Fardel is not acquainted with a single example of loss of sight as an attendant on softening. The other circumstances (which we can but very briefly indicate) apparently more or less distinctive of tumour are the occurrence of paraplegia; the greater frequency of epileptiform convulsions, which were in twenty cases unaccompanied with paralysis; the last point is very important, as it is singularly rare to observe convulsions without paralysis in chronic softening. We may resume then, with the author, as follows. The prolonged existence of violent cephalalgia, limited to one side of the head, attended or not with vomiting, and blindness or disturbed vision, without paralysis, if it does not indicate a tumour with certainty, at least excludes the idea of softening. If epileptiform attacks are superadded, without paralysis in the intervals, the probabilities are still stronger in favour of the existence of tumour, especially if speech and the intelligence be unaffected. M. Rostan attaches great importance, in the case of cancerous tumour of the brain, to the existence of *lancinating* pains in the paralysed limbs, perfectly different from those occurring in softening. M. Fardel more correctly considers that sharp lancinating pain occurs too frequently in cases of simple softening, to justify the view of their signification taken by that writer.

The curability of softening is very fully discussed by M. Fardel. That an anatomical state is capable of being produced, as a consequence of preexisting softening, which is in itself wholly innocuous, seems supported by a mass of evidence which it is difficult to resist. The reader will not expect us to state this fully, for to the predecessors of the author, and not to himself, we are indebted for it; but we shall briefly review the chief points of the subject.

Pulpy softening may become perfectly quiescent, giving evidence of its existence by no single symptom; but there is no evidence to prove that it can directly undergo anatomical cure, without having previously passed into the more advanced stage of "cellular infiltration."

When softening has arrived at this latter stage, it appears, according to the testimony of various writers, to be capable of cure in three different ways. (*a.*) The cellular tissue surrounding the infiltrated part and that which traverses it undergo a certain amount of induration; in some points it even becomes fibrous, and almost cartilaginous; the spaces

forming the meshes of this tissue, enlarged by commencing absorption and by actual shrivelling of itself, are filled with milk of lime-like fluid or serosity; the surrounding cerebral substance is either healthy, slightly indurated, or slightly softened. (*b.*) The portion of cerebral substance presenting the cellular infiltration is completely absorbed, and a cavity perfectly circumscribed is left behind. The walls of this are white, and more or less indurated, lined or not with a membrane, full of milk of lime-like fluid or serosity, and contained in the interior of the hemispheres or opening outwards. (*c.*) Finally, this cavity, if it be not very large, may contract by a sort of plaiting of its walls, become obliterated, and give rise to a white, stellate, or linear cicatrix.

Dr. Carswell states that he never met with an instance of cicatrization after softening; but he distinctly describes the other species of curative change.

M. Fardel has an interesting chapter on the *mode of death* of patients with softening of the brain. Such is the independence of organic and animal life in subjects of advanced age, such as came under the author's notice, that it is rare to observe a patient fall a victim directly to the progress of the softening itself. Certain complications (developed, however, generally under the influence of the cerebral affection,) prove the immediate cause of death: these are 1, some other cerebral affection; 2, some pulmonary affection; 3, the formation of gangrenous sores of the integuments. Upon each of these causes of death full information will be found in the original work.

The prognosis of softening of the brain is indubitably shown by the researches on the curability of the disease undertaken within the last few years—or rather by the results of these researches, to be not so necessarily hopeless as was at one time supposed. Curiously enough, Rostan, the first describer of the malady, stated twenty years ago his more than suspicion that, being an affection of inflammatory nature in many instances, it might occasionally at least undergo the curative changes proper to inflamed parts. He had no very satisfactory facts to carry out his notion with, though one of his cases certainly exemplifies the mode of cure now admitted. But the strange part of the matter in connexion with Rostan is, that within the last year he has affirmed that the disease is necessarily and infallibly fatal, that the idea of cure is a delusion: twenty years' experience and the researches of English and French observers, appear to have had a very different effect from that which might have been anticipated on the creed of M. Rostan.

There is another point of view in which M. Fardel conceives his own inquiries have demonstrated that the danger of softening is less than commonly supposed. He believes, on evidence which we had not space to lay before the reader, that many cases set down as examples of mere cerebral congestion, and terminating fatally are in reality instances of incipient softening arrested in their first period. This *must* be true, as congestion is in truth the first stage of inflammatory softening, but though this view adds a new principle to prognosis in cerebral affections, it does not alter any previously admitted ones, because former observers, in speaking of the prognosis of softening, certainly made no reference to such cases as are thus adduced by M. Fardel.

The author now considers himself prepared to discuss that much vexed

question, the *nature* of softening of the brain; and though his own opinions have been foreshadowed by much that has gone before, we consider it for the advantage of our readers to examine seriatim the objections raised to one at least of the doctrines held by others, and the precise facts (condensed) on which he supports his own.

The notion most commonly entertained on the essential nature of white softening is that it consists of a gangrenous condition of the brain. Abercrombie appears to have originated this idea, which has subsequently, as is well-known, received most active support from Dr. Carswell, and from various other writers of less distinction. The author opposes this view as might have been anticipated, especially as the alleged gangrene is regarded by those writers as analogous to the spontaneous gangrene of the extremities of old people. He denies that the conditions of the vessels which in the limbs produces senile gangrene (ossification *with* obstructed circulation from coagulation or otherwise) is met with in cases of softening with anything like sufficient frequency to justify its being regarded as the real cause of that diseased state. On the other hand he has himself seen a case in which the arteries of the base of the brain were ossified to such an extent, as to be converted into solid tubes, and yet here was no softening. The author here takes occasion to inquire into the accuracy of the common belief that ossification of the cerebral arteries is extremely frequent in subjects of advanced age, and the main cause of the hemorrhages occurring in their brains. The following numerical statement is really of extreme interest, for we are not aware that any fairer ground than the merest and crudest theory has led to the universal adoption of this notion respecting the generation of cerebral hemorrhage.

	State of the cerebral arteries in			
	32 subjects dying of various diseases: brain healthy.	30 cases of softening.	20 cases of cerebral hemorrhage.	Total in 82 *old* subjects.
Healthy	9	6	3	18
Thickened	21	18	10	49
Ossified	2	6	6	14
Partly cartilaginous	0	0	1	1
	32	30	20	82

In six of the fourteen cases of ossification too the change was in a perfectly incipient state. The present question, however, has less to do with ossification than with any condition capable of arresting the local circulation: thickening then, which was very frequent, is just as important a state. Upon this the author observes that the proportion of cases of healthy arteries and diseased arteries, in the two classes of subjects, (those with healthy and with softened brains,) was almost precisely the same in the series laid before the reader, and that hence it would be perfectly illogical to refer the softening to the accidentally associated morbid state of the vessels.

If obstruction to the cerebral circulation were the cause of softening, we should expect to find that conditions of the heart interfering with freedom of circulation through that organ should secondarily lead to

softening; and that ligature of the carotid artery on either side might possibly be similarly followed by that cerebral lesion. Dr. Law, of Dublin, has printed some cases tending (but no more,) to show that such may be the fact in cases of mitral contraction; and M. Sedillot recently made known a case of ligature of the carotid, in which a soft non-coherent state of the anterior hemisphere of the side on which the vessel had been tied was observed. This condition of the brain was, we are willing to admit with the author, different from true softening; but we do not admire his passing by with a very unsatisfactory remark the curious case of Mr. Vincent, in which the patient died on the seventh day after ligature of the right carotid, with hemiplegia of the left side, and in whom pale softening of the right hemisphere was discovered after death.

The author observes upon Dr. Carswell's statement, "to distinguish softening from obliteration from softening produced by inflammation, it is only necessary to ascertain the presence of the morbid state of the arteries which we have described;" that it is tantamount to saying, the only distinction between softening from arterial obliteration and inflammatory softening is obliteration of the arteries,—a notion altogether deficient in logical precision.

Various other notions of less importance tendered in various quarters respecting the nature of the malady are weighed in M. Fardel's balance and found wanting: we, too, regard them as of "light weight," and accordingly pass to the author's own doctrine. In its main features this doctrine has developed itself in the preceding pages: softening is an inflammation, having nothing special in its nature; essentially the same in the young and the old, and whether produced by local injuries or spontaneously developed; attended with redness in its acute stage, a redness which disappears in the chronic and leaves the part colourless. The "white softening" of authors is thus purely and simply chronic red softening. Such is the doctrine flowing from the 472 preceding pages of the author's volume; but at the 473d he puts the very pertinent question, "Shall we then definitively refuse to admit the existence of *primitive* white softening?" To this the author replies, first, by showing that many cases which have been considered examples of such a state were not so in reality: the evidence in his favour is satisfactory; as, for instance, in cases where œdema of the brain has been confounded with softening; and we consider it unnecessary to be more precise in our notice of this part of the reply. Secondly, he replies by admitting that a certain number of cases have been reported, and to all appearance correctly reported, in which, after an attack indubitably acute, white softening has been detected. Shall we admit that in these cases the disease had been latent for a greater or less length of time? M. Fardel does not yield to the temptaiion thus thrown out; but with more freedom from prejudice than was almost to be expected, admits that such cases are yet open to explanation. May we go further and suggest that they disprove his doctrine as a *general* one, and express our own conviction, derived from previous study as well as from a consideration of all the facts this writer has accumulated, that cerebral softening is not always essentially one and the same lesion.

As some of the details through which we have conducted the reader may have proved somewhat arid, we shall reward him for his diligence with a little amusement. A preposterous chemical theory conceived by

M. Couerbe has M. Magendie—and who more naturally?—for its patron. The brain, according to M. Couerbe, is composed of:

Pulverulent yellow fat	Stearonocote.
Yellow elastic fat	Cephalote.
Reddish yellow oil	Eleencephol.
White fat matter	Cerebrote.
Cholesterine.	

The eleencephol dissolves tolerably well the other matters in the brain which give it consistence; it is isomeric with cerebrote. Now, M. Couerbe thinks, according to the cautious Magendie, that this isomerism may serve to explain a very important "physiological phenomenon," namely softening of the brain; for cephalote having the same composition may under some morbid influence become metamorphosed into eleencephol, dissolve the other solids of the brain, and so diminish its consistence!

The only point, connected with the questions of etiology mooted by the author, which we consider it necessary particularly to refer to, is the condition of the heart. In every case in which softening existed in subjects under fifty years of age, coming under the author's observation, the heart was healthy. As respects the state of this organ in old subjects here are his results:

In old subjects dying of various diseases, brain healthy; hypertrophy of the heart in one fourth of the cases.
In old subjects dying of cerebral softening; hypertrophy of the heart in one fifth of the cases.
In old subjects dying of cerebral hemorrhage; hypertrophy of the heart in a little more than one third of the cases.

That hypertrophy of the heart has no influence in producing softening appears clearly from these facts, nor would theory lead to the belief that it has, but rather to the view held, as already mentioned, by Dr. Law.

Respecting treatment, M. Fardel's work is poverty itself; the disease is inflammatory,—ergo, antiphlogistics constitute the rational means of opposing its progress. In saying this we have in truth said all the author teaches. But the fault is not his; and we must confess we infinitely prefer this simple statement to the long tirade of salines, and alteratives, and tonics, and stimulants, and deobstruents, and refrigerants, and carminatives, and sedatives, with all the other *atives* which, to the bewilderment of the student and the scorn of the philosopher, would have been set down by many writers we have in our eye, had it pleased Heaven to make *them* the inditers of a work on cerebral softening.

And here we take leave of M. Fardel. Occasionally he has ruffled the smoothness of our critical spirit, but on the whole impressed us with a high opinion of his powers of observation, his judgment, and his conscientiousness. He has done better than this; he has added to the stock of sound medical literature unquestionably the most perfect history yet produced of the important disease he has described.

Art. II.

The Climate of the United States and its Endemic Influences, based chiefly on the Records of the Medical Department and Adjutant-General's Office, United States Army. By Samuel Forry, m.d.—*New York*, 1842. 8vo, pp. 378.

The opportunities afforded by the medical returns of our army and navy for demonstrating the influence of climate on disease have, until lately, been completely neglected, or only referred to occasionally by individuals desirous of illustrating the etiology of some particular affection. Yet to those who are acquainted with the principles on which these returns are prepared and organized, it must be evident that no surer basis is ever likely to be obtained for testing the many doubtful theories which have from time to time been put forth by our professional brethren, or for elucidating those important truths on which the preservation of life and the amelioration of suffering frequently depend. The excellence of military medical returns, as compared with the experience of civil practitioners, consists in their containing all the information requisite to enable us to apply the science of numbers to the elucidation of disease; the number of persons over whom the observations extend is always known, which cannot be the case in civil life except in public establishments; there is the same body of men constantly under observation, who have neither the opportunity of concealing disease when it exists, nor the right of indulging their caprice by transferring themselves to the care of another practitioner while the disease is under treatment; the army surgeon consequently enjoys the advantage of watching the progress of his cases from the commencement to their termination, while he is certain that the treatment and diet he prescribes are rigidly adhered to. We may add that he has leisure, except perhaps in seasons of unusual sickness,—nay, that he is obliged, as part of his duty, to keep an accurate record of the cases coming under treatment, for which medical men in private practice often cannot spare time, and for which too many feel but little inclined after the arduous and fatiguing labours of the day. But in addition to the opportunities thus afforded of studying disease throughout all its stages, the army and navy offer the most unexceptionable field for investigating the influence of climate on health. We find in every quarter of the globe, and under every variety of climate, bodies of men of the same average age and standard of health, similarly fed, similarly clothed, and similarly housed, employed in similar duties, with similar excellencies, and tainted with similar vices. They are moreover under the constant superintendence of highly-educated medical men, who periodically furnish returns containing every information regarding the sickness and mortality among them, and whose duty it is to bring under the notice of the authorities anything likely to exert a prejudicial effect on their health. It would be difficult to conceive a more advantageous combination of circumstances for the investigation of disease, and the effects of the various therapeutic agents, or to devise a better means of obtaining accurate information, whereby to estimate the influence of climate on the human frame.

During the last five years much has been done to elucidate this important branch of medical science, and valuable and extensive data have been furnished whereby to test existing theories, and overturn unfounded hypotheses. In 1837 began the series of Military Statistical Reports, extending over all our colonies, and embracing upwards of a million cases of disease recorded during a period of twenty years. This was followed by a similar series on the health of the navy, to which succeeded the valuable reports by the registrar-general, on the mortality in civil life in England. We are now happy to observe that the same spirit of inquiry has extended to other nations, and that our transatlantic neighbours have contributed their share of information to the general fund, in the form of a report on the health of their army. It consists of two parts: the first containing merely a brief sketch of the medical history of each year from 1819 to 1829, during which period the state of the returns did not, as regards the diseases, admit of the numerical method of investigation; the second part gives an analysis of the statistical materials collected from 1829 to 1839, with a view to develop "the laws of climate and the application of these laws to the elucidation of disease."

From the materials contained in this report, and the Meteorological Journal prepared from the same official sources, Dr. Forry has, in the volume now under review, drawn general deductions as to the laws regulating the development and influencing the character of various classes of diseases. We regret that he has omitted to furnish the tables on which his conclusions are founded, because however interesting, these results lose much of their value for the purposes of comparison, when the groundwork on which they have been framed is withheld.

The first part of his work consists of a description of the climate of the United States, with some remarks on the subject of climate in general, to which we shall very briefly advert. The principal constituents of climate may be reduced to heat and moisture, and its diversified character depends chiefly on the operation of two causes, latitude and elevation. There are other circumstances which materially modify climate, but their influence is only partial and limited. To form a correct opinion of any climate it is necessary not only to know the average annual temperature, but also the range of the thermometer throughout the year, and the distribution of heat among the different months, which varies materially in places having the same mean temperature. We find that in climates where the winter cold is excessive, the increase of temperature in spring is great and sudden, and the heat of summer intense, while in those countries where the range is more limited, the seasons glide imperceptibly into each other, and the heat of the tropics is more characterized by its duration than by its great intensity. One of the most important circumstances exerting a local influence, is the vicinity of the sea or a large body of water, tempering the heat of summer, and diminishing the cold of winter, and besides its direct influence, the prevalence of fogs and clouds in such situations, tends to establish an equal temperature. The winds also, as the medium through which the physical character of adjacent countries and seas affects the climate, may be regarded as important agents; witness the influence of the cold polar stream on the east coast of America, and of the warm gulf stream on the western coast

of Europe. The nature of the soil and the electric state of the atmosphere no doubt also contribute their share in modifying climate; but there is a great want of information as to the extent of such influence, and its mode of operation.

Dr. Forry has discussed at considerable length, the question whether the climate of a locality undergoes any permanent changes in a series of years. Our space will not permit us to enter into detail on this subject, but we think the mass of curious and interesting historical testimony which he has brought together, sufficiently justifies the conclusion, that the opinion generally entertained regarding the progressive melioration of climate is far from being sustained by evidence sufficient to enforce conviction.

We cannot in these pages enter into a detailed consideration of the climate of the various stations occupied by the troops throughout the United States, but must content ourselves with a brief notice of their leading features. The extent of the United States is so great, its climate so diverse, and its military force scattered so generally over its surface, that perhaps no more interesting study can be found for the medical statist, than to trace the various diseases which affect the human frame, from the icy shores of Maine, clad during half the year in its snowy mantle, to the flowery regions of the south, basking in perpetual sunshine. To do this with advantage, however, it is necessary to class the military stations in the following general divisions.

I. *Northern*, which has been subdivided into, 1st, the posts on the coast of New England, extending as far south as the harbour of New York; 2d, posts on the northern chain of lakes; 3d, posts remote from the ocean and inland seas.

II. *Middle*, subdivided into, 1st, the Atlantic coast from Delaware Bay to Savannah; 2d, interior or south-western stations.

III. *Southern*, comprehending, 1st, posts on the Lower Mississipi; 2d, those in the peninsula of East Florida.

Of these divisions the northern is characterized by the predominance of a low temperature during winter and spring; the southern by a steady high temperature throughout the year, while the meteorological phenomena of the middle division exhibit a medium between these two extremes. If we compare the climate of the three classes comprised in the northern division, it appears that the mean annual temperature does not vary materially, but the principal difference lies in the unequal distribution of heat among the seasons. Owing to the modifying influence of a large body of water, as already noticed, at the posts on the sea coast or on the large inland ocean-lakes, (which cover an area of 94,000 square miles,) the mean temperature of summer is lower, and that of winter higher than at posts remote from these, and at the latter the mean annual range of the thermometer is considerably greater, the difference of the mean temperature of the warmest and coldest months ranging from 46 to 60 degrees, while at the posts on the ocean and lakes, it only ranges from 39 to 49 degrees. The atmosphere at the interior posts is remarkably dry, and a constant and rapid succession is observed in the seasons, summer succeeding winter so rapidly that there is scarcely any spring; while in the other two classes the air is moist, the number of rainy and

cloudy days much greater, and the changes in the seasons low and uncertain. Along the course of the lakes a strong breeze blows during most of the summer, from 10 A.M. till 4 P.M., which materially modifies the heat of that season.

In the middle division the mean annual temperature is higher, the mean range of the thermometer less, and the seasons more uniform, but the temperature is more variable.

In the southern division the climate is exceedingly mild and uniform, the seasons gliding imperceptibly into each other. Many of the vegetable productions of the tropics are to be found in the peninsula of Florida, and the country may be said to enjoy perpetual spring and summer. The dews are very heavy, and as in tropical climates the air is remarkably humid, evinced by the rapid oxidation of metals, &c., and thunder-storms are of frequent occurrence.

On the subject of barrack and hospital accommodation in the American army, we have little information afforded us. Dr. Forry remarks that the troops are worse quartered than those of any other nation, and assistant-surgeon King states, "my hospital is very bad, and more or less wet at every rain. On the 16th of September, (1821,) the tide rose uncommonly high, which nearly inundated this place and the adjacent country; the water was a foot deep in the hospital; in fact I visited my sick, and went through it in a canoe." Nor is anything specific stated as to the nature of the duty and employment of the troops generally, which, however, appears on some occasions to have been of a very severe kind. The 1st regiment of infantry, for instance, when stationed at Baton Rouge on the Mississipi, were employed in getting timber in the swamps ten miles above that station, and are described as having "in truth become hewers of wood, and drawers of water, much better qualified to shoulder a hod than a musket. All *esprit du corps* being lost, the officer, instead of drilling his men in warlike exercises, expended his military spirit in superintending fatigue-parties operating in dismal swamps. Baton Rouge, or more properly speaking the swamps of the Mississipi, proved literally the grave of this regiment." (p. 201.) The result of this was that during six years (1819-25,) every man on an average must have been in hospital once in every two months and nineteen days, while the mortality during the same period was 21 per cent., a fate more nearly resembling that of our unfortunate countrymen in western Africa, than of any body of troops with whose history we are acquainted. This deficient accommodation and unhealthy nature of the employment of the troops must have tended materially to increase the amount of sickness, of which for the last ten years, we shall now take a brief review.

The following table compiled from the Statistical Report shows the ratio of sickness and mortality among every 1000 men serving at each class of posts.

In framing this table we have omitted the station of West Point in the third class of the northern division, because it is an academy for cadets, and it would be obviously improper to include them in considering the sickness and mortality among soldiers. We have also omitted in the southern division the posts *temporarily* established in the present theatre of military operations.

	Northern Division.								Middle Division.						Southern Division.						Canada.		Nova Scotia and New Brunswick.	
	1. Posts on the Coast of New England.		2. Posts on Northern Chain of Lakes.		3. Posts remote from the Ocean and inland Seas		Total.		1. Coast from Delaware Bay to Savannah.		2. Interior or South Western Station.		Total.		1. Posts on the Lower Mississipi.		Posts in the Peninsula of East Florida.		Total.		British Troops.		British Troops.	
	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.	Adm.	Died.
Intermittent Fever	36		193		202		166		370		747		617		385		687		478		83		1	
Remittent do.	26		33		31		30		181		180		180		196		112		170		5			
Synochal do.	43		16		10		19		27		25		26		60		49		56		125		68	
Typhus do.	5		4		1		3		3		4		4		13		2		10		1			
Total Fevers	110	·8	246	1·	244	1·2	218	1·1	581	5·3	956	7·1	827	6·5	654	14·2	850	8·6	714	12·5	214	2·4	69	1·6
Dis. of the Respiratory Organs	334	4·6	367	2·3	504	2·6	426	2·9	604	8·5	396	9·	468	8·8	276	4·1	294	4·	281	4·1	149	6·7	125	7·1
——— Digestive do.	384	·6	567	·9	528	1·2	512	1·	764	4·1	841	4·4	814	4·3	850	6·8	793	2·	832	5·3	186	1·7	120	1·7
——Brain and Nervous System	36	2·2	32	·5	36	·7	34	·9	32	2·4	39	2·5	37	2·5	55	3·8	96	1·3	68	3·1	14	1·2	11	1·3
Ebriety	59		71	·3	82	·4	74	·3	72	2·4	137	3·	114	2.7	64	2·4	170	·6	97	1·8				
Dropsies	1		3		3		3		3		11		8		13		5		10		2		2	
Rheumatic Affections	110		153		159		147		126		119		123		90		137		105		40		30	
Venereal ,,	102	1·9	57	1·2	60	1·7	68	1·5	109	2·7	46	2·7	67	2·7	68	2·1	28	2·	56	2·	99	2·	83	1·6
Ulcers and Abscesses	42		80		134		98		90		123		111		93		148		110		109		105	
Wounds and Injuries	244		326		336		315		284		417		371		225		340		261		162		148	
All other Diseases	490		283		430		395		238		419		357		474		326		429		122		127	
Deaths in Hospital, causes not stated		4·1		2·		1·8		2·3		3·1		6·7		5·5		9·2		11·2		9·8				
Total	1912	14·2	2185	8·2	2516	9·6	2290	10·	2903	28·5	3504	35·4	3297	33·	2862	42·6	3187	29·7	2963	38·6	1097	14·	829	13·3
Casualties, suicides, and sudden deaths		2·7		·5		1·6		1·5		2·6		1·8		2·		1·8		1·3		1·6		2·8		1·8
Deaths from Epidemic Cholera		1·1		4·5		1·		2·2		2·4		4·		3·5		8·6		. .		5·9		2·1		1·4
Total ratio of Deaths per Adjutant-general's Returns		20·6		16·5		19·5		18·8		36·2		48·8		44·2		60·6		36·7		53·3		20·		18·

The most remarkable feature in this table, is the very high ratio of sickness among the troops, amounting in the southern division to 2963 per 1000 of the strength, in the middle division to 3297, and in the northern to 2290, while among the British troops serving in Canada, where the climate of the military posts nearly resembles the last, the ratio is only 1097 per 1000, or less than half that of the American army in similar situations; and in Nova Scotia and New Brunswick it is still lower, amounting only to 820 per 1000 of the strength. The total mortality, however, in the northern division, very closely agrees with that in Nova Scotia and Canada, being $18\frac{8}{10}$, while that of the British troops is, in the former 18 and in the latter 20 per thousand. It is worthy of remark that the proportion of admissions is lower at the posts on the seacoast than at those in the interior or on the lakes. The same circumstance is observed in Nova Scotia and New Brunswick, where a large proportion of the soldiers are quartered near the sea, as compared with Canada.

No accurate deductions, we fear, can be drawn from the mortality by the different class of diseases in the United States army, as the cause of a very large proportion of the deaths is unknown. For instance, the total number on which the preceding table is framed, amounts as stated in the Adjutant-general's returns to 1480, while the cause of no less than 525 of these is not recorded, and even of 936 entered in the reports of the Surgeon-general, 184 or one fifth of the whole are classed under the head of "causes not specified." In the following pages therefore we shall confine our observations chiefly to the *prevalence* of the different diseases.

Fevers. The prevalence of this class in the northern division strikingly approximates that among the British troops in Canada, being 218 per 1000 on the average of all the stations, while the latter is 214. In the middle and southern divisions however, owing to the greater prevalence of fevers of the intermittent and remittent types, the proportion is from three to four times as high. In the southern division the ratio is lower than in the middle, but we are of opinion that it is stated lower than it ought to be in consequence of the returns of several of the posts being wanting for the third quarter of the year. Although the requisite allowance has been made for this circumstance in the strength, still as the autumnal quarter is that in which diseases of endemic origin are most rife, the want of any returns for that period must materially influence the results. The troops were removed also during the unhealthy season from some of the posts, particularly New Orleans, whence in consequence of its great insalubrity the soldiers were sent, generally in May, to the Bay of St. Louis.

There is a striking difference in the amount of intermittent fever at the different classes of posts comprised in the northern division; at those on the sea-coast the ratio is 36 per 1000, while at each of the other two classes it is nearly six times as high. The same feature has been observed in comparing the diseases of the British troops in Canada with those in Nova Scotia and New Brunswick. Dr. Forry ascribes this exemption to "geological formation and the nature of the soil." "As the region of New England as far as the St. Lawrence, with little exception, has a primitive formation, with a sandy sterile soil, whilst that of the

lakes consists of a secondary formation, having not unfrequently an alluvial superstratum, composed of a rich vegetable mould from three to six feet deep, it is not difficult to deduce the correct inference. In the former the geological structure is destitute of organic remains, and the little contained in the sandy soil does not find enough of moisture to induce the necessary chemical action, while in the latter not only is the geological formation of secondary origin, but the deep rich soil is sufficiently humid when a high temperature acts upon the organic remains with which it abounds, for the development of the morbid poison called malaria." We are by no means satisfied of the correctness of this inference, for though the description applies to all the posts on the coast of New England except Fort Wolcott on Goat Island, it is wholly inapplicable to many of the stations on the western coast of Nova Scotia, which enjoy an equal exemption from fevers of this class, although surrounded by mud and marsh exposed during the summer to the action of a high temperature. The absence of marshy ground in the neighbourhood of the posts in the United States, sufficiently explains the absence of intermittents, except at Fort Wolcott, where, as at most of the military stations in Nova Scotia, the marshes are under the influence of the tide, and being regularly flooded the generation of infectious miasmata may be prevented.

In the middle division the same feature is observed, viz., that intermittents are much more prevalent at the posts in the interior than on the coast, while the other types of fever prevail to the same extent in both. At some of the posts in this division, the troops have been occasionally obliged to encamp, on account of the great prevalence of febrile diseases during the summer.

In the southern division fever is less prevalent at the posts on the Mississipi, than at those in the peninsula of East Florida; but as the troops at the former were always moved to healthy encampments during the sickly season, and especially from New Orleans, it would be improper to infer that the causes of fever were in less active operation there.

The history of Augusta arsenal in this division is interesting, as illustrative of the advantage to be derived from care in the selection of a military post. Prior to 1829 the arsenal was situated on the Savannah, and fever prevailed to so great an extent that the troops were removed always to summer encampments, yet the only advantage of that measure seemed to be that the cases proved less fatal and relapses were less frequent. The arsenal was afterwards removed to the *Sand hills,*[a] three miles from Augusta and two from the Savannah river, where the soil is hard, dry, and sandy, with no marshes in the vicinity, and the ratio of intermittent fever has since then been only 150 and of remittent 156 per 1000 annually. We are unable to state the precise ratio prior to the removal of the post, but so prevalent was it, that in the third quarter of 1825 all the garrison except two men suffered from it. We trust the day is not far distant when our military authorities will deem the opinion of the principal medical officer at a station of as much importance in selecting a site for barracks as that of the commanding officer of engineers; had such always been the case, many thousand lives which have been sacrificed in our colonies, especially the West Indies, might have been

spared, and the heavy expense necessarily occurred in supplying their place might have been saved.

In epidemics of remittent and yellow fever the experience of the United States army fully corroborates the observations made in the British Statistical Reports, of the advantage to be derived from the removal of the troops even to a short distance from the locality in which the disease originated. It seems to have been the practice in the southern division to remove the men from the most unhealthy posts whenever the hot weather set in; but in addition to this, when fever broke out in a severe form at any of the other posts, an encampment was generally formed, which in most instances proved successful in checking the violence of the epidemic.

The advantage of encampment or removal even to a short distance from the spot where the epidemic originated has been sufficiently demonstrated, not only in the various instances referred to in the volume under review, but also by the experience of the garrison of Gibraltar during successive visitations of yellow fever, of the troops in British America on the outbreak of cholera in 1832 and 1834, and in repeated instances in the West Indies on the occasion of severe epidemics of fever. We have been informed that since the publication of the British Army Statistical Report on these colonies, the system has been extensively adopted with highly satisfactory results in every instance, and we trust that the authorities, military and medical, will call the special attention of officers to the subject, and will not be prevented by mistaken notions of economy from authorizing the commanding officer to incur the necessary expense of such removal, whenever the health of the troops affords sufficient grounds for adopting such a measure.

The prevalence of fever of the continued form is apparently much lower in the States than in British America. We say *apparently*, because we believe that most of the cases recorded as "ebriety" in the work before us are of that character, which in the British reports are entered as ephemeral fever. In his remarks on continued fevers Dr. Forry says, "To the majority of American physicians the fact will appear strange, that in the British Islands, probably not above one practitioner in fifty entertains any doubt of the infectious nature of continued fever, comprising synocha, synochus, and typhus." We are not informed on what authority this statement is founded, but we believe it will appear quite as strange to the majority of British as of American practitioners. The opinion of the profession certainly preponderates in favour of the contagious nature of true typhus, but the question of the propagation of any other form of continued fever by contagion in this country is still far from being considered as sufficiently established.

On the much disputed question whether length of residence diminishes the susceptibility of the constitution to the operation of morbific agencies in tropical climates, Dr. Forry remarks,

"As the regular troops in Florida were almost wholly from the northern regions, those that escaped the first summer, instead of gaining an immunity from disease by exposure to the climate, acquired an increased susceptibility of the system to it in a less violent form. The power of resisting morbific agents inherent in the animal organization is so much diminished every suc-

ceeding summer, that the ratio constantly sick in each regiment, more especially as regards intermittent fever, bears a close relation to the period of service in the territory."

Diseases of the respiratory organs. On a former occasion we reviewed at some length the author's opinions on the influence of the climate of the United States, and more particularly of the peninsula of East Florida, on this class of diseases. We shall not at present, therefore, enter into a detailed consideration of the subject, but merely state the conclusions which seem fairly deducible from the data contained in this and the British Statistical Reports. From both these sources of information it seems clearly established that phthisis is nearly twice as prevalent in the southern as in the northern regions of the western hemisphere, and though the mortality in Florida by this disease is less than at the other southern or the middle stations, it is almost exactly the same as among the troops occupying the northern posts. "A cold temperature therefore is not essentially *per se* favorable to the development of phthisis pulmonalis." With these results before us we must differ with our author on the advantages to be gained by a winter residence in East Florida in cases of pulmonary consumption, and more especially as the proportion admitted into hospital among the troops during the winter season is actually higher in Florida than at the posts in the northern regions during the same season. There is, moreover, another very serious objection to a residence in this climate, in the greater prevalence of fever of the intermittent type. Formerly a marshy country was supposed to be beneficial in consumption, but this notion is now happily exploded. Sir James Clark says that "an attack of ague is much more likely to favour the occurrence of consumption than to prevent it;" and Dr. Forry himself remarks that he has frequently witnessed in the southern States the supervention of rapid pulmonary consumption in constitutions deteriorated by malarial diseases, and adds, "Malaria holds a prominent place among the causes productive of the cachectic condition of the system which precedes the formation of tubercle." Entertaining such opinions, we think our author should be very cautious in recommending a person predisposed to consumption or already labouring under it in an incipient form, to remove from a climate where intermittent fever is so rare as on the coast of New England, to one where the sources of malaria exist so abundantly as in Florida.

But while we thus differ as to the advantage to be derived from this change of climate in phthisis, we entirely accord with Dr. Forry's views of its efficacy in bronchial affections. We find that wherever the seasons are strongly contrasted, catarrhal diseases are most prevalent, and that the proportion decreases as the range of temperature diminishes. In chronic bronchitis, therefore, much advantage might be expected from exchanging the inclement winter of the northern region for the mild and equable climate of the south.

The prevalence of diseases of the respiratory organs in the southern division is nearly twice, and in the middle and northern three times as great as among our troops in America.

Diseases of the digestive organs. As we proceed southwards, we find the prevalence and intensity of this class of diseases to augment with

the increase of temperature. There is a slight exemption in favour of East Florida as compared with the south-western stations, and the posts on the Lower Mississipi, but even there the ratio is more than one half higher than the average of the northern division, and more than four times that of the British troops in Canada; while it bears a still greater disproportion to that of Nova Scotia and New Brunswick.

Among the diseases of this class it may be observed as an interesting circumstance that in 1830 and the following year, a number of cases of colica pictonum occurred, some of which terminated in paralysis of the hand and arm, and were traced to have arisen from the use of whitelead by the men for cleaning their gloves and belts, in consequence of which it became necessary to abandon its use, substituting pipe-clay.

Epidemic cholera. This disease prevailed in the United States in 1832-3-4 and 5, but from the absence of several quarterly returns, and from a number of the cases having occurred among a body of troops engaged in the campaign against Black Hawk in 1832, from which no official reports were received, the information regarding it is very meager. Throughout the whole army, 686 cases are reported, whereof 191 proved fatal or one in $3\frac{3}{5}$. Of these, 302 cases and 103 deaths occurred in the northern, and 384 cases and 88 deaths in the middle and southern divisions. As in British America, it seemed to spread along the great water-courses, but appeared with such irregularity at different places, breaking out at stations remote from each other, while intermediate spots remained unaffected, as to leave its contagious nature still a disputed question.

Diseases of the brain and nervous system. The ratio of diseases of this class is very high in all the divisions but particularly in the southern. Among them are a considerable number of cases of epilepsy, which we agree with Dr. Forry in attributing among soldiers to the abuse of spirituous lipuors, and in this, as in all other diseases of the same class, the mortality is proportionately higher in regions where a high temperature predominates.

Ebriety. Under this head a considerable number of cases are reported, but there seems to have been a want of uniformity in the nomenclature in this respect, as at some stations there is not a single case entered. In the British reports, we believe, they are mostly included with ephemeral fever. Though well aware of the pernicious effects of habitual intoxication, we cannot quite agree with our author when he attributes to this vice more than half the deaths among the soldiers. We not unfrequently meet with the same laxity of expression in the writings of medical officers of our own army; it is not uncommon, for instance, to find it stated, at stations where the mortality rarely exceeds 13 or 14 per thousand, that one half or two thirds of the deaths originated from drunkenness, without considering that under no circumstances is the mortality among any body of men likely to be so low as 6 or 7 per thousand, however temperate they may be. There is no doubt that a continued indulgence in intemperance tends to undermine the constitution, and produce premature old age, rendering a man unequal to the active duties and habits required of a soldier, and diminishing the power of resisting disease; but we are of opinion that the *fatal* effects of this vice are to be looked for, not among the young, but among the prema-

turely old soldiers, whose unfitness for military duty has been probably induced, or at least aggravated by their too frequent potations. Let it not be supposed that we underrate the moral evils arising from this vice, and the debasing effects produced by indulgence in it; but we feel it our duty to state the true conclusions which seem warranted by the various data before us, nor can we lend ourselves to the pious fraud of exaggerating the description of the physical consequences, even with the hope of diminishing thereby the more lamentable train of moral evils to which it gives birth. We are happy to observe that in 1830, the absurd and pernicious regulation by which a soldier was compelled to drink a quantity of spirits daily as part of his duty, was abolished, and this government normal school of intemperance suppressed, we trust never again to be established. We do not despair of seeing a more efficient and comprehensive system of education introduced into the army, as the best check to intemperance; when the soldier will be taught not only to read but to understand what he reads, and when his leisure hours may be more pleasantly spent in the library than in the canteen.

With regard to rheumatic affections, the results corroborate those arrived at in the British Reports, that these affections are but little influenced by climate. Thus while the ratio is lowest at the posts on the Lower Mississipi, the next lowest is on the damp and foggy coast of New England. We are rather surprised to find Dr. Forry recommend Florida as a winter residence in rheumatism, for, exclusive of the temporary posts the ratio of admissions amounts to 137 per 1000, which is above the average of the United States generally, and considerably higher than in some of the other subdivisions. In estimating the influence of a climate it is obviously necessary to draw our conclusions from the results of the permanent posts, excluding those temporarily established for military operations, as it is a remarkable fact that during the progress of active warfare troops are little subject to the influence of disease. The prevalence of diseases of this class is three times as great as among our troops in Canada. Dr. Forry supposes this may be partly accounted for by the omission among the latter of cases treated in quarters,—an explanation, however, which will not suffice, as it happens very rarely indeed that a soldier is treated in his quarters, and when such an occurrence does take place, his case is invariably included in the returns.

Venereal affections are less prevalent than among the troops in Canada; but as we are not informed whether there are regular health inspections in the United States army as in the British, the comparison may perhaps be unfair.

Scorbutus. The only other disease we shall notice at present is scurvy. From 1829 to 1838 inclusive, 152 cases of this disease occurred, most of them among the troops in Florida, but in 1820 it prevailed to a very great extent in the 6th infantry and the rifle regiment. These corps were sent in the autumn of 1819 to Council Bluffs, on the banks of the Missouri, to form a permanent station there. On their arrival in October, they were obliged to build barracks for themselves, of which they took possession about the beginning of January. Previous to this they had suffered considerable hardships in conveying their stores up the river in boats, and in procuring timber for building the barracks, and had in consequence been more sickly than usual. In January the weather became

exceedingly severe, and fresh provisions were so scarce, that their issue was confined to patients in hospital. That we may not be accused of exaggeration we describe the state of the provisions issued to the rest of the troops in the words of one of the medical officers :

"The important articles of beans, peas, and vinegar, contemplated to have formed important parts of the ration failed altogether. Salted pork and beef, bacon, flour and Indian corn, constituted the substantial part of the ration. By far the greater part of the meat was decidedly in a putrescent state, and absolutely unfit for issue; the smell and taste both rejected it with disgust. The flour, though less exceptionable than the meat, and originally of fine quality, had become musty previously to its issue. The corn, which was furnished in the proportion of two pints to every six rations was soon thrown aside as a drug. Deprived of vegetables and the usual condiments of the table, the repast of the soldier was at the same time deficient in nutriment, unpalatable, and unwholesome." (p. 337.)

Under these circumstances it is not astonishing that scurvy in a very aggravated form broke out and raged with great severity till the weather moderated, and fresh provisions with an abundant supply of vegetables cold be procured as spring advanced. The total number of cases at Council Bluffs and Fort Snelling in that year was 734, of whom 190 died. In 1821, there were 86 cases, and 5 deaths, but they were not confined to one station; they arose from deficient diet, and particularly from want of a supply of vegetables.

The results showing the influence of season on the prevalence of disease, completely bear out the observations made by the medical officers of the British army, the greatest proportion of admissions taking place in summer, when the heat and moisture are greatest; and this is much more strongly marked in the middle and southern divisions, where a large proportion of the diseases are of malarial origin. The following table exhibits the proportion admitted into hospital in each quarter, out of every 1000 men serving in each division :

	Northern Division.			Middle Division.		South. Division.	
	1st Class.	2d Class.	3d Class.	1st Class.	2d Class.	1st Class.	2d Class.
1st Quarter .	410	475	566	574	716	719	705
2d Quarter . .	491	548	622	699	802	707	764
3d Quarter . .	553	676	756	907	1153	898	892
4th Quarter .	463	499	578	704	806	629	835

These results correspond with those obtained from observations both in the British and French infantry, showing the greater prevalence of disease in the third quarter of the year among the military.

Having now concluded our observations on the sickness and mortality in the United States army, we feel ourselves called upon to express a most favorable opinion of the industry and talent which Dr. Forry has displayed in the work before us. That there are many deficiencies in the statistical portion of the work we admit; but these are more to be attributed to the defective state of the returns, than to neglect on the part of our author; and those who have had any experience in compiling returns made by numerous individuals, few of whom take the trouble to adhere strictly to prescribed forms, will be able to appreciate the difficul-

ties he has experienced, and the able manner in which he has discharged his task.

There is one part of the work, however, on which we think a little more labour might have been very advantageously bestowed. Had an appendix been added to part second, containing the figures on which the results have been founded, condensed into abstracts similar to those of the meteorological observations in the appendix to part first, the value of the book would have been much increased as a work of reference, and for comparison with others of a similar description.

Before concluding, we must briefly animadvert on the manner in which these statistics of the United States army have been laid before the public. The report on the health of the troops, we learn, was printed for the use of the medical officers of the army, and although a few copies have been otherwise distributed, still it has not been published for general circulation. On what principle the surgeon-general permitted this work to be printed for private circulation only,—a work so essentially national in its origin and objects, containing so much relating to military hygiene, such a mass of information on the climate of the United States, and so many facts illustrating the general laws of medical science, while the results were to be made public first through the medium of the medical journals, and finally of the work now under consideration,—we cannot divine. It is but an act of justice to the medical officers of the army, of whose labours this work may be considered a condensed record, that it should be published in an unmutilated form under the sanction of the surgeon-general. Should this be done, we trust he will cause to be appended to it detailed abstracts of the diseases by which the sickness and mortality have been occasioned, and thus place it on a footing, as a work of national importance, with those which have emanated from the medical departments of the army and navy in this country, while he will at the same time afford the profession an opportunity of submitting medical opinions to the searching ordeal of numbers.

ART. III.

Geschichtliche Darstellung der grösseren chirurgischen Operationen mit besonderer Rucksicht auf Edlen von Wattmann's Operations-Methoden. Von Dr. FERDINAND HEBRA.—*Wien,* 1842. 8vo, pp. 434.

An Historical Account of the more important Operations in Surgery, with especial reference to the mode employed by Dr. Von Wattmann. By FERDINAND HEBRA, M.D.—*Vienna,* 1842.

THIS work is written as a manual for the use of the students at Vienna and the German universities, and contains a series of articles devoted to the history and progress of operative surgery, with a review of the present practice on the continent and in this country. To the end of the various articles is added an especial account of the practice of Dr. Von Wattmann, for the convenience of the students of Vienna, as well as on account of the respect of the author for the skill and kindness of his teacher.

The work is written with great care, and evidently with great attention to the works of the old writers. The work of Sprengel is taken as the

basis of the historical part, whilst the standard of German surgery is deduced from the labours of Rust, Chelius, Dieffenbach, and Langenbeck. In France and this country the works of the writers during the last thirty years have been carefully studied, and their contents arranged with those collected from the German works, so as to form a complete and very honest summary of the present condition of operative surgery.

The history of surgery is more generally studied in Germany than in this country, and appears from Dr. Hebra's account to form a part of the examination at Vienna. The medical literature of this country is deficient in any good history of the progress of surgery. To the few who wish to study it, the works of Portal* in France, with Sprengel† and Zang‡ in Germany, afford a basis for further study. To the majority of readers, each surgical operation is only regarded for its own peculiar merit, without reference to anything else; and in the writings of most authors on surgery, the operations of Cheselden and Dupuytren, Taliacotius and Dieffenbach, are all classed together, without the slightest respect for their antiquity or any mention of their authors; the operation itself and not its author being the point always kept in view.

The present article contains an abstract of the work of Dr. Hebra, with a more complete summary of the practice in this country, as deduced from observation and a study of our surgical works. The principal points in the historical part, which are connected with the present mode of operating, are touched on, as being of sufficient interest in themselves, as well as deserving of mention, from respect to the originators of them.

1. *Trephining.* Perforation of the skull for the evacuation of blood, or for the relief of insensibility or vomiting after injuries of the head, appears to have been performed as early as 400 B.C., by Hippocrates himself, or one of the same family. This operation, however, was either performed with an instrument resembling a large gimlet, or with a circular saw, like the modern trephine, worked with a wimble or brace, and requiring the use of both hands. With the former instrument a simple perforation was made, whilst with the latter a circular portion of bone was removed. The danger of wounding the brain or membranes being great when the simple borer was used, the perforation was not generally made quite through the skull, but only partially; a thin portion being left to separate by exfoliation, and the relief being thus necessarily postponed till this event occurred. When simple fissures or cracks existed, a black fluid was rubbed on the bone, which sinking into the fissure, enabled the operator to trace it; this part and the bone round it were then removed with a kind of rasp, if the fissure was only shallow; if it extended deep, the trepan was employed. The trepanning instruments at this time consisted of the perforator, the trepan worked by the brace, and the rasp, to which we find added in the time of Celsus, or about the first century of the Christian era, the hammer and chisel, to remove portions of bone between two or more perforations; and the *meningophylax*, an instrument resembling a small mason's trowel, the metallic

* Portal. Histoire de l'Anatomie et de la Chirurgie, 1770. Seven Vols. 8vo.

† Sprengel. Geschichte der Chirurgie. 2 Bande, 8vo.—Halle, 1805, 1819.

‡ Zang. Darstellung blutiger heilkunstlerischer Operationen als Leitfaden, 1825. Five Vols. 8vo.

part of which was passed between the skull and membranes, to prevent injury to the latter from the slipping of the instruments.

The trepan worked with the brace appears occasionally to have injured the brain, and produced serious results; to avoid this result, we find that about this period a ring was added at a short distance above the cutting edge, or that three or four metallic knobs were here placed. These instruments were called by Galen (131 A.D.) *abaptista*, and held by him in great contempt; the chief instruments used by him being a pair of strong forceps to extract the loose portions of bone, and a strong cutting chisel or scoop to remove other portions.

From the time of Galen to 1363, the trepanning instruments were rarely used, the knowledge of injuries of the head decreased, and the medical plan of treatment was chiefly practised. Amongst the Arabians, the perforator and chisel were occasionally used; whilst in the west of Europe, the monks, who had become the surgeons of the age, treated these injuries with salves and powders. Little, however, as these monks benefited surgery, they appear not to have under-estimated their own labours, for they established a regular scale of payment in cases of injury to the head. The sum paid was proportionate to the size of the exfoliated portion of bone, and the noise produced by dropping it into an empty copper vessel. The knowledge of injuries of the head had now grievously decreased; thus Roger of Parma, (1206,) considered the non-passage of air through fissures of the skull, when the patient held his breath, as a means of diagnosis; whilst Lanfranchi of Paris, (1295,) considered the existence of fracture of the skull as detectible by the resonance of the head, when struck with a stick, and the inability of the patient to chew his food. Amidst this mass of ignorance we, however, find Guy de Chauliac, physician to Pope Clement VI., who introduced the trepan and added the central pin to it; he also employed instruments more cautiously than his predecessors, and limited their application to the more severe forms of injury of the head, and to those parts of the skull which are destitute of sutures.

A brighter era now, however, commenced in the operative and other departments of surgery, and the treatment of injuries of the head began to assume those features which it has since retained. Within a short period we find Andreas da Croce at Vienna, Ambrose Paré, with his indefatigable pupil, Jacque Guillemeau, at Paris, and Fabricius ab Acquapendente at Padua, all labouring at the improvement of practical surgery, the perfecting of their instruments, and the instruction of others by their well-known works. Up to this period, namely about 1550-1600, the trepan or circular saw worked with a wimble had been used. Fabricius ab Acquapendente now introduced the modern trephine, or circular saw worked with a handle, which required only the use of one hand. This instrument was, however, armed with prominences, till Andreas da Croce improved it, and described the trephine as used at the present day, without prominences or guard, but depending alone for its safety on the skill of the operator. Amongst the instruments used at the end of the sixteenth century, we may therefore enumerate the trepan, or circular saw worked with a wimble, the trephine, or circular saw worked with one hand, with or without central pin, elevators, chisels, the forceps, rasps, and strong chisels, acting on their lateral edges, with various instruments

for smoothing the edges of the cut and cracked bone. In addition to these, the Hey's saw deserves especial mention, as it is so frequently used in this country, in comparison with the various forms of round saws. This instrument, consisting of a small flat saw, with the teeth either in a straight or convex line, and fixed on a small handle, has been ascribed to Scultetus of Ulm, and is figured by him in the Armamentarium Chirurgicum. Scultetus, however, describes this instrument as used by others, and lays no claim to its invention, but only to the introduction of the "serrula versatilis," which is a complicated instrument, with a double set of wheels, and resembling in principle the modern "scie à molette" of Charriere at Paris. The Hey's saw is an instrument of much greater antiquity, and appears, from the account of Andreas da Croce, to have originated with Avicenna (974 A.D.), although some slight doubt exists whether Hippocrates himself was not to a certain degree acquainted with it.

The present surgical instruments for injuries of the head thus existed at a comparatively early period, but they form a part only of the number, as many surgeons added to and altered the instruments in a useless and frequently ridiculous manner; and it was not till the beginning of the eighteenth century that Mr. Cheselden, of St. Thomas's Hospital, and Mr. Sharp, of Guy's Hospital, simplified the instruments, and brought the trephine into more general use, which latter instrument Mr. Pott improved by substituting a wooden for an iron handle. Mr. Sharp practised the incision of the dura mater, when blood was effused under it, an operation which appears to have been practised as early as 1619 A.D., by Glandorp, surgeon at Bremen on the Weser. The operation of trephining over the sinuses appears for many years to have been dreaded as dangerous, till Hoffmann, of Altorf, (1653 A.D.) and subsequently, amongst others, Mr. Warner, of Guy's Hospital, trephined over the lambdoid suture, whilst Lassus, of Paris, (1783,) opened the longitudinal sinus without any bad result.

It is remarkable that the trephine should be still used in England, whilst the trepan or saw worked by the wimble is the instrument chiefly employed in Paris and Vienna. In the great majority of cases, however, in England, as the operation is performed for injuries accompanied with depression of bone, the Hey's saw is principally used. The chief advantage of the trepan is that it can be used by the most clumsy person, and requires very little delicacy of hand. The trephine, however, in the hands of a dexterous surgeon can be used with the greatest safety, whilst to a delicate hand a more complete sensation of the nature of the parts cut through is communicated to the hand, and not unfrequently, by the slight pressure of the hand the internal table is cracked and not sawn, by which means the dura mater is less exposed to injury.

In this country, the exposed dura mater is treated as mildly as possible with a soft poultice or water dressing. In the account of the practice of the Vienna school, Dr. Hebra mentions that unless circumstances exist to contraindicate it, the portion of bone, which has been sawn out, is inserted again in its old position in the opening made by the trephine, and covered again with the integuments and other parts, which are united by suture or plaster, except at a small spot, which is left open for the evacuation of pus or other fluids. If, however, in a few days the replaced

portion shows no inclination to unite with the neighbouring bone, and assumes an unhealthy colour, it is removed, and proper dressings are applied.

2. *Harelip.* It is remarkable that so common a deformity as harelip was not relieved by operation till a comparatively late period. The operation for the removal of harelip is described by Celsus; but even the remarks made by this author are less full than might be expected, and simply to the effect, that the best mode of operating in cases of simple fissure consisted in making an incision on the convex edges of the fissure, whilst in the double fissure he appears to have removed the central portion, and inserted a piece of skin from the neighbouring parts. Abul Kasem, an Arabian physician, resident in Spain, (1122,) adopted the practice of applying the actual cautery to the edges, and uniting them with plaster, which operation appears to have been the general practice, till Ambrose Paré (1509) adopted a modification of the plan now generally used. This experienced surgeon, in cases of harelip and wounds resembling it, pared the edges of the wound, and then united them, by passing needles through the lip, which were more firmly fixed by thread twisted round the needles in the figure of eight, the end of the thread, however, being passed through the eye of the needle. This mode of fastening the thread was copied from the manner in which the ladies and tailors of the day wound the thread round the needle, and thus carried both safely in their cuffs or caps. This operation of Ambrose Paré was improved by Fabricius ab Acquapendente, (1557,) who suggested the advantages derived from making an incision of the mucous membrane, which united the lip to the gum. The generation of surgeons succeeding these two appear to have displayed their ingenuity in trifling alterations; some used metal pins with the ends cut off, whilst others put pads of lint under their extremities; some used triangular needles, whilst others used very small sewing needles, so small, indeed, that Heister bestowed on the profession a needle-holder. At last Petit, of the Academy of Surgery, (1674,) introduced the silver pins with moveable steel points, which were not only more convenient to use, but did not rust or stick in the wound.

The mode of making the incision, the best form of needle, and the mode of applying the thread being thus settled, the next change in the operation was caused by the disputes concerning the relative advantages of sutures and needles, as compared with plaster; the majority, however, continued to use needles, employing frequently bandages and plaster in addition. The general object of these plasters was to press the parts of the face forwards, and render retraction of the sides less likely to occur. By some, bandages and plasters were carried round the back of the neck and over the lip, whilst others only applied them, as is frequently now done, to the cheeks. The instrument for holding the lip consisted of two blades working parallel, and of such relative dimensions, that the blade applied on the under side of the lip afforded a broad surface, made of wood or horn, for cutting the lip accurately upon; it appears to have been first introduced about 1801, by A. von Beinl, a military surgeon at Vienna.

The operation for harelip, as performed on the continent and in this country, differs little in the mode of incision and manner of bringing the parts into apposition. By some the double harelip is treated at once,

according to the plan of Desault, whilst others prefer operating on one side first, generally commencing with the worst, and then treating the second side at a subsequent period. The choice, however, of this plan must depend on the nature of the fissure, as in cases of great separation the attempt to restore both sides at once would be very difficult. The knife-edged scissors are free from the objection, urged against common scissors, of crushing the lip, and are used by many of the principal surgeons at Paris as well as in London. The simple knife is, however, more generally used by the surgeons of Edinburgh, whilst at Vienna the lip is cut with a knife on a piece of wood or plate of horn placed under it. The harelip pins of Petit, although still frequently used, are not by some preferred to strong common needles or pins, united, however, generally with the twisted thread of Ambrose Paré, whose simple copy of the tailors' needle has not been yet improved on.

The practice of Dr. Von Wattmann does not appear to correspond in two very important points with that employed most frequently in this country, in reference to the proper periods for removing the pins, or the manner of feeding the child. The needles, according to Dr. Hebra, are not removed till the seventh or eighth day, whilst in this country the second, third, or fourth days are frequently chosen as the proper time for applying plaster or other means in the place of the needles; thereby lessening the chance of cicatrized dots in a part where any unnecessary blemish is much to be deprecated. The child is also recommended to be fed on artificial food, and by no means to suck; experience, however, points out the impropriety of such advice. If the pins are short and neatly arranged, the child may be allowed to take the breast from the first, and thus be kept quite quiet, thereby preventing the derangement of the health as well as the separation of the parts from crying.

3. *Tracheotomy and laryngotomy.* The operation of making an opening into the trachea appears to have been performed as early as 100 B.C., by Asklepiades, of Bithynia, and to have been followed with success. Aretæus, however, brought the operation into disuse, by stating that inflammation and spasm of the part, as well as the sense of suffocation, were increased by it, and that the wound did not heal up. In spite of these objections, Antyllus, a surgeon in Adrian's army, revived this operation in cases of difficult breathing from pressure of the parts in the fauces and the presence of a foreign body in the trachea, forbidding, however, its use where the lungs were affected. The operation performed by Antyllus consisted in an incision down to the trachea, of which he removed three or four rings. The fear of fistulous openings remaining was removed by Abul Kasem and Ebn Zohr, in Spain, the former of whom cited an instance where a wound of the trachea made by a maniac on herself had healed up, whilst the latter removed a portion of the trachea of a goat, and found that the opening closed readily.

The next improvement, introduced by Fabricius ab Acquapendente, consisted in reaching the trachea by a series of incisions, and inserting a tube into the opening which he had made. This tube was originally straight, but was curved by Julius Casserius (1545). The chief variations in the operations introduced by various writers consisted in the length of the external incision, and the oblique or vertical direction of the incision in the trachea. Frederick Dekker of Amsterdam, in 1694,

introduced the plan of puncturing the trachea at once with a trocar and canula, and after withdrawing the trocar, leaving the canula in the opening. This operation was revived within the last few years by Mr. Wood, in a long and very laborious paper in the Medico-Chirurgical Transactions, who suggested the curved canula instead of Dekker's straight tube, armed with a jointed trocar or knife accurately fitting the canula. Desault is generally said to have first performed the operation of laryngotomy, and is certainly deserving of the credit of having first set the operation well before the profession; but he does not describe this operation as quite new, or claim the discovery of it. In common cases of difficulty of breathing, this able surgeon cut the crico-thyroid membrane, but when a foreign body existed in the trachea, he divided the anterior edge of the thyroid cartilage. This operation was modified by Jourdain, who cut the crico-thyroid membrane, and then divided the anterior part of the cricoid cartilage and the upper rings of the trachea. Since his period, surgeons have employed the incision of the crico-thyroid membrane extended upwards or downwards, so as to avoid the thyroid gland on the one hand, and the chordæ vocales on the other.

The operation of opening the larynx or trachea does not afford either room for much variety or generally much time for thought; the difficulties are great and variable, especially when the urgency of the case affords no time for delay. The trocars of Dekker and Wood are rarely used, and do not appear devoid of danger. If the trachea is exposed, these instruments might be used with benefit and safety, but if the trachea be punctured at once, when the parts are deep and the trachea small, this tube might even miss the trachea or wound its posterior wall, whilst any vessel, usual or unusual, from the thyroid veins to the arteria innominata, must run its chance. The knife is the especial instrument in the hands of the surgeon in this as in most other operations. The danger of blood flowing into the trachea has been variously estimated; by some it has been supposed not to be dangerous in moderate quantity, whilst by others it has been considered as the chief danger of the operation. It appears that in many cases no bad symptom has occurred from even considerable bleeding into the trachea; but the case of M. Roux, (Andral, Clinique Médicale, tome iv. p. 209,) with its excellent and ingenious treatment, affords such an instance of the danger from this occurrence as to serve as a warning for ever, and to justify direct reference to it here. M. Roux was performing the operation of tracheotomy. The trachea was just opened, when a small quantity of blood entered into it with the first rush of air, and prevented its entrance into the bronchial tubes. The patient became insensible, and the arteries ceased to pulsate, the face assumed the whiteness of death, and the pulsations of the heart were no longer detectible. Without the least delay, M. Roux introduced a gum elastic tube into the trachea, and by sucking the air through it drew out part of the blood. In a short time respiration was established, and the heart began to beat, consciousness, however, returned very slowly, and it was not till after two days that the patient first began to recognize the persons standing round her bed.

The old tubes are still frequently used; cases, however, often occur, in which so much irritation is produced by them, as to render

them inapplicable Under these circumstances the removal of a portion of the trachea has been practised; sometimes also the separation of the sides of the wound with a curved wire or other means has been found to be the most convenient course of proceeding.

4. *Œsophagotomy.* Although the possibility of this operation was mentioned by Verduc of Paris as early as 1611, it does not appear to have been performed till 1783, when Goursaud of Limousin extracted a bone from the œsophagus by cutting on the left side of the neck; this operation was successful, and was performed a second time by Roland shortly after.

Carlo Guattani, in 1772, wrote a very valuable paper in the Memoirs of the French Academy of Surgery, describing some experiments on dogs to show that wounds of this part healed readily, and that it could be easily reached in man by cutting between the trachea and left sterno-thyroid muscles. In 1779, Eckholdt of Leipsic suggested the propriety of reaching the œsophagus between the two origins of the sterno-mastoid. These two operations were so difficult that, till 1820, the attention of surgeons was chiefly directed to the removal of foreign bodies in the œsophagus by vomiting and instruments, and to this period we are chiefly indebted for the information on this subject which we possess. Vacca Berlinghieri of Florence, in 1780, suggested the best plan of reaching the œsophagus, by making an incision between the sterno-thyroid and sterno-hyoid muscles in front, and the sterno-mastoid behind, the omo-hyoid being cut across, and the œsophagus rendered prominent by the introduction of a bulbed steel catheter. The œsophagus was divided upon this and the obstacle drawn out with the forceps. The chief varieties in the operation have consisted more in the instruments used than in the situation; in the occasional cases which occur the situation selected by Vacca Berlinghieri being generally preferred. In this country the scalpel and forceps, with the occasional use of the bulbed sound or catheter to render the œsophagus prominent, have been generally used with a hook to hold the œsophagus and forceps to extract the obstruction; such also appears to be the practice at Vienna. Considerable variety, however, exists as to the instruments used for this operation at Paris. Thus M. Lisfranc introduces a curved canula, with a forked stylet, on which the œsophagus is divided, whilst MM. Roux and Velpeau prefer a canula with a spring blade, with which the œsophagus is divided from without inwards. The chief peculiarity of M. Begin's operation consists in cutting down to the œsophagus midway between the trachea and sterno-mastoid, and opening it without the use of any instrument except the simple scalpel.

5. *Paracentesis thoracis.* This operation appears to have been performed at a very early period, and is said to have been originally suggested by the recovery of a man after a wound of the chest, who, on the want of any relief to a disease of the lungs by medicine, had gone to battle in hopes of being killed. Such may or may not have been the case; but there is no doubt that this operation was performed at a very early period, as it is described very fully by Hippocrates, and some means of diagnosis given and indications for the place of puncture. The patient is directed to be shaken to make the fluid rattle, whilst a wetted cloth is recommended to be wrapt round the chest, and the spot which becomes

dry first is to be chosen as the place of operation. If the case was one of hydrothorax, Hippocrates recommends that the rib should be perforated and the fluid partially evacuated, after which a plug is to be inserted and the fluid gradually drained off during the succeeding thirteen days; In empyema, however, the intercostal space was punctured with a lancet, and a certain portion let out, whilst the remainder was evacuated twice in the day for nine days, after which the opening was either allowed to close gradually or injections were made into the chest, according to the nature of the matter. Galen introduced the plan of trepanning the sternum in hydrothorax, and drawing out the fluid with a syringe; whilst the chief Arabian and Grecian surgeons used either the lancet-shaped instruments for perforating the intercostal muscles, or applied cauteries to the prominent part of the swelling, and practised the operation of perforating the rib very rarely. The experience of Ambrose Paré induced him to recommend this operation in cases of wounds of the chest and fractures of the ribs, as well as in empyema; whilst Fabricius ab Acquapendente also included cases of inflammation of the pleura, suppuration of the lung, internal abscesses, and wounds. The incision of the walls of the chest was practised by Fabricius ab Acquapendente, Diemerbroeck, and Solingen till 1694, when Vincent Drouin introduced the trocar for this operation.

The two operations of cutting down to the pleura and opening the chest with a knife or trocar have been employed by Delpech, Morand, Boyer, and Van Swieten, whilst the puncture of the chest with the trocar pushed right through has been employed by Sharp, Sir Charles Bell, and others in England; by Laennec, &c. in Paris; and by Schuh of Vienna; both operations being still practised and supported by strong authorities. By drawing down the skin a valvular opening may be made, either with the knife or trocar indifferently, and an equally free opening afforded for the canula; when, however, the least admission of air is objectionable, the introduction of the small trocar and canula affords an advantage over the knife, as, in addition to the valvular opening, the sides of the wound fit the canula more accurately, the flow of fluid can be graduated and intermitted at pleasure. If the wound is to be left open and the admission of air is immaterial, the opening made by the knife is more free than that made by the trocar, as the former is one clean cut, whilst the latter is formed by three small radiated incisions. The most prominent part of the chest will often point out the best situation for puncture; there are, however, some situations in which the opening can in general be more conveniently made than in others. The lower edge of the rib is to be avoided, on account of the intercostal artery; whilst the junction of the serratus magnus and obliquus externus abdominis is also objectionable on account of the small arteries and nerves which are usually situated there. Professor Leber stated that a convenient spot might be found, by passing a line horizontally round one side of the chest, from the ensiform cartilage to the spine, and choosing the spot at the junction of the middle and posterior thirds for the seat of puncture. This line would fall between the seventh and eighth ribs, and is the situation preferred now by Von Wattmann of Vienna, and formerly by Walther of Halle. Now, although this situation is supported by such good authority, and even a lower situation has been recommended by Pelletan, Sabatier, Larrey,

and Desault, and is still practised in France, yet a higher situation is generally preferred in this country on account of the danger of wounding the diaphragm, lung, or liver. The space between the fifth and sixth ribs has been recommended by Hey and Laennec, and that between the sixth and seventh by Sharp, Bromfield, Sir Charles Bell, S. Cooper, Callisen, Rust, and Von Graefe, either of which is indifferently preferred by the principal surgeons of this country. Before leaving this subject it is right to mention the fine grooved needle, which has been introduced for puncturing the chest in obscure cases, and for the gradual removal of the fluid, by the late Dr. Davies of the London Hospital, but which has been brought more fully before the profession by Dr. G. Babington of Guy's Hospital; to which may also be added the double trocar of Dr. Skoda and Dr. Schuh of Vienna, by which the entrance of air into the cavity of the chest is effectually prevented.

6. *Paracentesis abdominis.* This operation was performed as early as 1100 B.C., and appears to have been remarkably fatal till the time of Ambrose Paré, who recommended the slow evacuation of the fluid, the careful bandaging of the patient, and the accurate closure of the wound. This operation was generally performed either with a pointed instrument, knife, or caustic, until 1626, when Sanctorius Sanctorinus introduced the trocar and canula into general practice, which had really been invented by Girault, a French surgeon some time before. The trocar of Sanctorius was a round pointed instrument encased in a canula, with which he punctured the umbilicus in men and the vagina in women, under the bedclothes and out of the sight of the attendants. This canula of Sanctorius was altered and modified in various ways without much improvement by many; thus L. Petit added a fissure in the canula, which allowed the edge of a knife to be passed through to enlarge the opening, whilst Andrée introduced the flat trocar with the lancet point, to which a canula, made of two semicircles of tube screwed together, was fixed. This canula was flattened by the trocar extending it laterally, but on the withdrawal of the trocar assumed an oval or round shape. This trocar was subsequently altered for a round one by Mr. Wilson of Glasgow. The lancet-pointed trocar, with a flat or round stem, as well as the simple round trocar, with a round point, had now been all introduced, when John Aug. Ehrlich of Leipzic, in 1760, introduced the trocar with an enlarged extremity and canula, the latter accurately fitting the part behind the enlargement of the trocar, and divided into three parts at its end, to allow the easy withdrawal of the trocar. This is the instrument now generally used, whilst the same modification has been applied to the flat trocar. Injections of fluid into the abdomen were tried at various periods and continued by some down to the end of the last century. Thus Mr. Warrick of Truro, in 1744, after drawing off twenty pints of fluid from the abdomen of a woman of fifty years of age labouring under ascites, injected several pints of a mixture of claret and Bristol water without any very bad effect, and thus cured the dropsy. Since this period, injections have been recommended by some and practised by others; those of aromatics by Heuermann of Copenhagen; of lime-water by Weitz; and barley-water by Chopart, Desault, and Bertrandi. Two cases are recorded by Mr. S. Cooper, where ovarian cysts were injected by Mr. Ramsden with a fatal result.

The abdomen has been punctured from the rectum, vagina, and scrotum by various surgeons, but the parietes of the abdomen have become now the common situation of puncture. The practice at Vienna is to puncture the umbilicus, or the lower part of the abdomen, on the right or left side, at a point midway between the anterior superior spine of the ilium and umbilicus. The umbilicus has been recommended by Ollivier and Bigot, of Angiers, whilst M. Velpeau recommends the lateral part of the left side. The puncture of the umbilicus has become very rare in England, and is not practised, rather from want of any reason for it than from any objection against it; whilst the puncture midway between the anterior superior spine of the ilium and the umbilicus is not practised on account of the danger of wounding the epigastric artery, which is occasionally carried out to one side with the flattened rectus, whilst at other times it retains its natural situation. On these accounts the puncture of the abdomen is generally made in or close to the median line, by which means all arteries are avoided, and only in very rare cases a vein of any size is exposed to injury.

7. *Catheterism* Catheters appear to have been used frequently at the time of Celsus, of a curved form, and from nine to twelve or fifteen inches long for men, whilst somewhat similar instruments, but from six to nine inches long, were employed for women. Amongst the ruins of Pompeii, some long straight iron tubes were found, which appeared decidedly to be instruments for passing into the bladder. Catheters appear to have been regularly used from their first introduction, and amongst a rich people addicted to rich living and sensual pleasures must have been often required; the passing of the catheter at last became an art, and when the itinerant lithotomists travelled about the country, assumed a supposed degree of perfection. These men treated difficult cases with a boldness which could only be based on some experience of good, and tried to extol the difficulties of everything that was comparatively easy: they passed the catheter with a facility which was acquired by constant experience, and with a dexterity and rapidity of motion, which astonished the spectators, and received the well-known name of "tour de maître."

At the end of the sixteenth century flexible catheters of leather and horn were introduced, which were convenient, but far inferior to the modern elastic catheter. It is not clear to whom the merit of originating the modern elastic catheters is due. Theden, Pichel of Wurzbourg, and Bernard of Paris, are mentioned amongst the introducers of them. The elastic catheters for a considerable period were made of silver wire, twisted in a spiral form and covered with elastic web; these were found very liable to corrode and break, and the introduction of the common elastic gum catheter as at present used was a great addition to surgery. These were imported for a very considerable period from Paris, but are now made to a considerable extent in this country, of the most varied sizes and forms. In affections of the prostate gland as well as in cases where the catheter is required to be left in the bladder, these instruments are of the greatest use, whilst their employment has increased latterly in cases of stricture since their recommendation as well as improvement in the adaptation of suitable handles by Sir B. Brodie.

The utility of straight catheters was pointed out by Montagu, Lieutand and Gruithuisen, but did not excite much notice in the medical profes-

sion till Amussat and Civiale devoted their attention to this subject. On the continent and especially at Paris, this plan has attracted more notice than in this country, where the plans of our forefathers meet with more respect, and any new remedy must possess some superiority before we lay aside our hereditary practice. Catheters undergo less change than any other instruments. The most convenient metal is found to be silver, and as the urethra possesses a definite length and breadth, the powers of invention are limited. The long prostate catheter of Sir Astley Cooper, the short sharply-curved catheters of Sir Benjamin Brodie with their solid firm handles, and the catheters suited to the natural curve of the urethra introduced for the operation of lithotrity by Baron Heurteloup, and for common use by Mr. Stanley, are the principal instruments used in this country in addition to the silver catheters of the ordinary curve, the real excellence of which it has been universally agreed, even by the inventors, exists only when used by the good anatomist as well as experienced surgeons.

8. *Hernia.* The operation for the relief of strangulated hernia was evidently known to Celsus in an imperfect manner, but appears to have become to a certain degree obsolete after his death till the time of Ambrose Paré and Franco, the former of whom appears to have relieved cases of inguinal hernia, by opening the sac and cutting out a small portion at its neck. It does not appear, however, to have been decided till the end of the sixteenth century, in what situation inguinal hernia was generally strangulated, when Abr. Cyprian in Holland, laid down the fact, that the strangulation was generally caused by the parts at the abdominal ring. To this fact very great additions were made by Haller, Mery, Pott, and the two Hunters, who described more accurately the anatomy and pathology of inguinal hernia, as well as the peculiar nature of the congenital rupture.

Louis Petit appears to have practised the operation of dividing the stricture outside the sac as early as 1720, which operation was supported and performed by Monro Primus, about fifty years later. This operation has been variously estimated by the writers and surgeons of the present day, especially Mr. Key, who has been its zealous supporter; whilst its value has been discussed in all the modern works on this subject, more especially those of Professor Richter and Mr. Lawrence, as well as illustrated by cases from the practice of Mr. Luke, Mr. Lloyd, and Mr. Liston. No subject in the whole of surgery has been rendered more complete than that of hernia within the few last years, whether we consider the anatomy of the parts, the operation itself, or the medical treatment. In each country of Europe, where medicine has been regularly studied, this subject has attracted the attention of the best men in the profession. In Italy Scarpa, in Germany Richter and Langenbeck, in France Dupuytren and Jules Cloquet, in England Percival Pott, Sir Astley Cooper, and Mr. Lawrence, have all made it the subject of exclusive and laborious essays and works. The important parts of this operation have become thus so definitely settled, and the reasons for them so evident, that the history of this operation presents matters of less general interest than that of any surgical operation, if we consider its gradual progress; it is, however, more remarkable than any other for the early

period at which many of the most important points of its treatment were decided.

9. *Lithotomy.* The operation for stone appears to have been performed as early as 318 B.C. at least, and to have consisted in cutting down on the stone through the perineum, which was rendered prominent and tense by the pressure of the stone, which was pressed against the lower part of the bladder by the introduction of the finger into the rectum. This operation was called the operation by cutting on the gripe or the lesser apparatus, and was performed by Celsus, the surgeons of Alexandria, Guy de Chauliac, and several itinerant lithotomists down to the sixteenth century. Lithotomy in women was performed by Celsus, by making an incision between the urethra and pubis, whilst in virgins the incision was made on the outer and lower part of the left labium. In 1525, Johannes de Romanis of Cremona introduced the plan of making an incision into the bulb on a sound, this incision being then continued into the membranous portion of the urethra only for a short distance. The urethra and neck of the bladder were then dilated with two "conductors" fitting into each other, on the withdrawal of which, the "forceps" and "latera" were introduced, and the stone removed. This operation was continued more or less till 1697, and was practised by the principal continental surgeons. The operation from its complexity was named the "apparatus major," as well as the Marian operation, from a wonderful account of it published by Marianus Santo da Barletta, who described the instruments and operation in such flowing and eloquent language, that his name has been transmitted to posterity with the operation.

It is said that Hippocrates bound his pupils by an oath not to perform lithotomy, and if we refer to the accounts of the Marian operation, we might be tempted to agree in the propriety of his decision: whether we consider the parts divided, or the extreme dilatation of the membranes and prostatic portions of the urethra, the operation has nothing to recommend it, and is far inferior in excellence even to the old Celsian method or operation by the gripe. With all the skill of the first surgeons in Paris, the deaths from this operation amounted to more than 50 per cent., whilst imperfect cures and fistulous passages also occasionally occurred after the operation. This operation was unfortunately perpetuated by an accidental circumstance. Lawrence Collot learnt it from one of the pupils of Romanis, and practising it at Paris was made Lithotomist Royal, an office which continued in the same family for about 150 years, whilst the very worst part of the operation, the extreme dilatation, was held forth by Collot himself as the whole secret of the proceeding.

We now come to one of the most remarkable circumstances in the whole history of surgery, whether we regard the complete change in the mode of operation, its mode of origin, or the conduct of the originator himself.

In 1697, Jaques Beaulieu, commonly called Frère Jacques, appeared at Paris, stating that he was inspired from heaven to cut for stone, fistula, and rupture. He claimed neither money nor reward for his operation; (which he had secretly learnt from an itinerant lithotomist of the name of Paulani;) he lived a life of the greatest practical piety and self-denial, and only asked them to allow him enough to obtain the common necessaries of life, and to keep his instruments clean. His operation consisted

in passing a long dagger-shaped knife from the left side of the perineum by the tuber ischii right into the bladder, which was rendered fixed by a round large sound with a short curve passed through the urethra. The bladder was thus wounded in its lower and back part, and this wound was continued forwards and enlarged by cutting towards him. He then withdrew his instruments, and guiding his forceps with a conductor or finger, pulled out the stone. Thus this uneducated friar changed the operation for stone from a dilating to a cutting operation, and without even a pretence of scientific knowledge saved as many, perhaps even more patients, than the best operators of the day. This man made, however, but one friend, and was at last obliged to leave Paris in 1698, to travel in Germany and Holland; but in 1700 his only friend, Fagon, the royal physician, became a sufferer from stone, and recalled him to Versailles, where he studied anatomy under Du Verney, and learnt his new operation. This new operation consisted in passing a grooved staff into the urethra, upon the prominent part of which an incision was made on the left side of the perineum through the transversus perinæi and levator ani muscles; he then carried his knife, an ordinary scalpel, along the groove through the prostate and neck of the bladder, and extracted the stone with the forceps. This operation with the improvements collected during 140 years, we now retain as the best lateral operation; yet it benefited Frère Jacques but little. He practised at Paris, and the greatest capitals of Europe, taught Marèchal and Raw how to perform his operation, and died in comparative poverty at his native village about the year 1714. The little money which he had acquired he gave to his relations; the medal bearing the motto "Ob cives servatos," presented to him by the senate of Amsterdam, was never found; and the golden sounds which had been also presented to him at the Hague, were well known to have been melted down before his death.

On the retirement of Frère Jacques, Raw acquired that great fame, which has transmitted his name to posterity. This famous lithotomist had seen Frère Jacques operate both before and after his improvement, and was supplied with bodies at Amsterdam for his anatomical lectures, whilst Frère Jacques was operating there. There appears to be no doubt that his secret was the poor friar's operation, by which this shrewd surgeon made a large fortune, and which he practised with the greatest success. He explained the operation to nobody, and even advised them not to believe what he said, and only gave them a slight hope of the revelation of his secret on his death. This secret never transpired, but what was more unfortunate a wrong idea of his operation became prevalent after his death, and misled amongst others Cheselden himself, who believed the accounts which Albinus, the pupil of Raw, gave of his patron's operation.

Cheselden, then surgeon at St. Thomas's Hospital, was willing to practise anything, which he could learn to be of use for the relief of others, even though it was unintelligible to himself; he therefore cut deep into the body of the bladder according to Raw's supposed plan, and met with the greatest disappointment. He then began to think for himself, and planned the lateral operation, closely resembling the operation of Frère Jacques, and in all probability the real operation of Raw.

The real operation of Cheselden has generally been copied from Dr.

Douglass' statement, and a wrong conception of its nature thus been formed. The latest form of operation, though certainly different from Dr. Douglass' description, is not very clear even in Cheselden's own words. In 1730, Cheselden, in an appendix to the fourth edition of his Anatomy, described the latter part of the operation thus: "I then feel for the staff, and cut upon it the length of the prostate gland, straight on to the bladder, holding down the gut all the while with one or two fingers of my left hand." This accords with the account of Le Dran and Morand, the former publishing in 1730, the latter in 1731; the former, however, is more particular, and mentions that after the point of the knife was inserted in the groove of the staff, that it was carried on in the groove to the end of the staff. The operation of Cheselden may thus be stated, on the authority of Cheselden's Appendix of 1730, and Morand's account in the memoirs of the Royal Academy of Sciences in 1731, and Ledran's account in his Paralèlles des Tailles in 1730, to have consisted in cutting between the accelerator urinæ and erector penis and by the side of the intestinum rectum; the staff was then cut upon in the membranous part of the urethra, and the point of the knife inserted in the groove, which knife was then carried on through the prostate into the bladder, all the way running in the groove. The gorget was then passed along the groove of the staff and the forceps over it.

But here comes a question: Dr. Douglass, in his appendix to the History of the Lateral Operation, states that Mr. Cheselden's knife first entered the groove through the sides of the bladder immediately above the prostate, and afterwards the point of it continuing to run in the same groove in a direction downwards or towards himself, the prostate and muscular part of the urethra were divided. This account of Dr. Douglass appeared in 1731, he also adds the following words: "When he first began to practise this method, Cheselden cut the very same parts the contrary way; that is, his knife entered first the muscular part of the urethra, which he divided laterally from the pendulous part of its bulb to the apex or first point of the prostate gland, and from thence directed his knife upward and backward all the way into the bladder, as we may read in the appendix he lately published to the fourth edition of his Anatomy. But some time after he observed that in that manner of cutting, the bulb of the urethra lay too much in the way; the groove of the staff was not so easily found, and the intestinum rectum was more in danger of being wounded." In short Dr. Douglass states that Cheselden subsequent to 1730 pierced the bladder behind the prostate, and cut from behind forwards. The point therefore remaining to be decided is, whether Cheselden describes a different operation in his work subsequent to 1730.

There are two editions published during Cheselden's life subsequent to 1730; one in 1740, and another in 1750, containing an account of this operation, and both these describe the operation in the very same way as the appendix of 1730, but with an important alteration in a few words. The words "cut upon it (the staff) in that part of the urethra which lies beyond the corpora cavernosa urethra and in the prostate gland, cutting from below upwards to avoid wounding the gut," are substituted in the editions of 1740 and 1750 for the words "cut upon it (the staff) the length of the prostate gland straight on to the bladder,"

which are the words of the edition of 1730. This alteration "from below upwards" agrees with the account given by Dr. Douglass of the direction of the incision made by Cheselden latterly, the great mistake of Dr. Douglass appearing to consist in the point at which he supposed the incision to have been begun. The parts divided in Cheselden's operations appearing both before and subsequent to 1730 to have been the same, and the only difference in the operation to have consisted in the fact, that Cheselden divided the prostate down on to the staff, instead of cutting it from without inwards towards the rectum.

Claude Nicolas Le Cat had seen Morand perform Cheselden's operation, but subsequently introduced a modification of it, which was performed with a multiplicity of instruments, and had the disadvantage of making an opening of very uncertain size into the bladder, whilst Frère Côme attained to the same uncertain end with his lithotome caché, an instrument with a spring blade capable of making a very extensive incision.

The gorget with a cutting edge was introduced by Sir Cæsar Hawkins of St. George's Hospital in 1753, and has been modified chiefly by Scarpa and Mr. Abernethy, whilst its use has been recommended and practised by successive surgeons of the British metropolis. At the present day there are few hospital surgeons, who use it as the regular instrument, but by most its use is recommended under particular circumstances, and in some cases, as when the perineum is deep, the prostate much enlarged, or the parts in the perineum much diseased; whilst in some cases where the perineum is deep and large, the double cutting gorget is still used. It may, however be said, that the knife or simple large scalpel is the instrument generally used for lithotomy by the chief surgeons. Mr. Blizard's knife is also used at some institutions. The blunt gorget is still used as the most efficient dilator of the prostate, whilst the cutting gorget is still sufficiently often used to justify its insertion in the operating cases, but known to be an instrument of so great good or evil, that its recommendation is always accompanied with allusion to the accidents, too frequently fatal, occasionally arising from its use. In different countries, and even in different hospitals of the same town, various knives and staffs of different sizes and curves have been introduced, but the principle of operation has varied much less; the operation of Cheselden, with slight variation, is performed by the principal surgeons in this country, by Lisfranc in France, and by Dieffenbach and Von Wattmann in Germany. The lithotome of Dupuytren, recommended by his practice, and left as a legacy to Sanson on his death, still retains its place in France, though modified somewhat, and occasionally employed in conjunction with the knife. It would be difficult to mention all the modes in which the bladder has been reached by high incisions as well as by the rectum, but the perfection to which these have been brought would not be very interesting in a country, where the unanimous voice of the profession has rejected them, and when each successive year limits the modes of performance as well as the frequency of lithotomy.

Within the last few years the attention of surgeons has been chiefly directed to the study of the various conditions of health existing in persons, who are unfortunate enough to require lithotomy, whilst the operation of removing the stone without incision has become more gene-

rally practised by the profession. This is remarkably contrasted with the studies of the older writers on the subject of lithotomy, to whom the peculiar instruments as well as the mode of operation formed the chief attraction. The frequency of incontinence of urine in women who have been cut for stone, has induced surgeons to try this as the last resource, and even to practise, when all dilating means have failed, the incision of the mucous lining of the urethra alone.

10. *Aneurism.* The operation for the cure of aneurism was not attempted till 97 A.D., when Rufus of Ephesus tied an artery wounded in venesection, and thus producing aneurism; the artery was, however, divided after the ligature had been applied. Burning, and excision of swellings connected with vessels, were introduced by Celsus, and appear to have been practised till 340 A.D., when Antyllus again introduced the practice of applying ligatures to aneurisms. By this surgeon the artery was laid bare above and below the swelling, and tied in both situations, whilst the aneurismal sac was cleared out, and filled with various substances to produce suppuration. This operation appears to have continued some time, with this exception that after the application of the ligatures the sac was occasionally removed in toto, or destroyed by caustic or actual cautery. The introduction of the tourniquet by Morel, in 1674, was found to be a great advantage in the operation for aneurism, which even at the period of Dionis, Le Dran, and Bertrandi, consisted in laying open the sac and applying a ligature above and below. The plan of opening the sac, and tying the artery above and below, continued even down to the time of John Hunter, with one exception, which does not, however, appear to have attracted much notice. Anel, in 1740, in a case of aneurism at the bend of the arm, cut carefully down to the artery, and after separating the nerve, tied the artery above the sac without opening this, and cured his patient. This operation met with little favour, and was not much noticed; it however may justly be considered as the greatest improvement in the operation on aneurism down to the time of Hunter. In the imperfect condition of surgery the introduction of Anel's method was a great improvement, but in reality the value of this improvement depends more on the bad condition of surgery at the time, than the excellence of the operation itself. In cases of aneurism from disease of the artery, the application of a ligature according to Anel's method would be attended with danger, and often inapplicable, from the diseased condition of the artery, and its benefit would be confined chiefly to aneurism arising from wounds, and principally to those in which the effusion of blood had attained a circumscribed form, and had not been loosely extravasated. The chief improvement in the operation for aneurism is certainly the application of the ligature according to the plan of Mr. Hunter, which consists in tying the artery at a distance from the aneurism. This great discovery, the value of which has been appreciated more and more ever since it was made, is grounded on the three facts, that by this operation the arteries will maintain the life of the limb, that the aneurism will not continue to increase, and that after certain changes it will be removed; but its value is increased by its enabling the surgeon to tie the artery in a convenient position and healthy situation, as well as to avoid all the dangers of the old operation. The merit of this operation belongs in all respects to Hunter, without diminution from any quarter.

In the middle of the eighteenth century Brasdor introduced the plan of tying the artery on the distal side of the aneurism; this operation is however best known in this country by the labours of Mr. Wardrop, as well as by the principal modern surgical works. This operation presents itself as a last resource, when, from the situation of the aneurism, the artery cannot be tied in the common situation; but even in such cases the chances of success are but slight, as in the majority the aneurisms to which it is applicable are of the most incurable kind, and in the large vessels near the heart.

The attention of surgeons in this country and abroad has been directed especially to the subject of aneurism, with the most decided benefit, and with a remarkable absence of all controversy. The knowledge of the capabilities of small arteries to maintain the circulation of the limb has reduced many arteries to the domain of surgery which were supposed to be as far beyond reach by operation as the aorta itself. The knowledge of the action of the ligature, as first completely and clearly shown by Dr. Jones, and since illustrated by Manec, Lawrence, and Travers, has rendered the application of the ligature more certain, and its action better understood, whilst the application of the discoveries of Laennec, and the improvements of them introduced by successive physicians in this country and abroad, has placed a means of accurate and minute diagnosis in our hands, of which our forefathers had not the least conception.

Within the last few years the torsion of arteries has been introduced by Amussat of Paris; and this plan, consisting in separating the internal membrane of the artery from the middle coat and squeezing it back into the artery, whilst the part on the distal side of this portion of retracted membrane contracts forcibly, has been extensively practised on the continent. In this country it has however met with little encouragement, partly because we are always inclined to follow any safe plan of proceeding, to which we have been educated, as well as because this plan is just as difficult as the application of the ligature, without possessing its certainty.

11. *Amputation.* Removal of the limbs by operation was practised at a very early period, and appears till the time of Ambrose Paré to have been a very fatal proceeding. The danger of bleeding appears justly to have occupied all their thoughts, so that very little attention was paid to the formation of the stump. The bleeding was checked by hot irons, compresses, and the most varied means, and yet with all this apparatus, so small was the success of the operation, that one can well agree with the quaint yet mournful maxim of Guy de Chauliac, that it was better to let the limb drop off than to cut it off, for even if the patient gets well after the amputation, he thinks that the limb might have been saved. The process followed by this surgeon consisted in passing pitch-plasters very tight round the joint, and thus causing the limb to mortify; whilst Botalli, a few years afterwards, fell into the opposite extreme, and invented a kind of guillotine for cutting off limbs.

The whole practice of surgery was, however, destined to undergo a marked change by Ambrose Paré, and in no respect more than in the treatment of vessels wounded in amputation. To this great man we are indebted for the application of the ligature as a means of arresting he-

morrhage, it might almost be said the discovery of its application, but the honesty of Ambrose Paré induced him to show that, although in practice he might be the inventor, yet that the suggestions for its use were contained in the works of others. The ligature was applied by Paré to wounds of arteries and veins with the best results; yet the value of it was not estimated by many of his own day, and especially the professors of Paris. These revilers, however, were answered in a manner, which they little expected, by Ambrose Paré, who added an apology to his works, in which he detailed a series of successful cases, when the ligature had been employed by him, and pointed out to them suggestions in Avicenna, Vesalius, Johannes de Vigo, Andreas da Croce, and others, of the value of that means which they had abused. Since this time the ligature has gained general use, and its action been completely illustrated by the labours of Morand and Petit in France, and more especially by our own countryman, Dr. Jones. The tourniquet of Morel appears to have consisted of a cord tightened by a stick, whilst a modification of the present common tourniquet was introduced by Petit, which has been gradually improved by the instrument makers to its present condition.

Within the last few years the attention of surgeons has been directed chiefly to the compression of arteries in difficult situations and without exerting pressure on the neighbouring parts. For this purpose, M. Malapert introduced an instrument, which having a fixed point at the back of the neck and curved parts passing round one side of the neck, would enable the surgeon to compress the carotid artery; whilst an instrument having a fixed point at the back of the shoulder was not long since presented to the French Academy for compressing the subclavian artery. The ingenuity and neatness of these instruments are admirable, but the occasions for their use are rare, and even then it may be doubted, whether the thumb of the surgeon is not at least a more useful, and certainly a more simple instrument. The tourniquets of Dupuytren and Signoroni are however well deserving of notice. That of Dupuytren consists of two steel arcs passing on each other so as to form a semicircle, but capable of forming a larger part of a circle, if required; to one extremity a pad is fixed, whilst to the other a pad elevated and depressed by a screw, is applied and capable of being pressed on the artery. The instrument of Signoroni is formed of two steel arcs, moved like a pair of compasses at their junction with a screw, whilst to each end a pad is fixed. Both these instruments are extremely useful, both avoid the surrounding parts and exercise firm pressure on the artery, but whilst the instrument of Dupuytren is capable of the most accurate adjustment, that of Signoroni is less liable to any accidental derangement.

The introduction of the flap amputation by Lowdham, together with the use of the ligature immediately after amputation, as well as the recommendation of the union by the first intention at the end of the seventeenth century, do not appear to have produced much effect, and to have been soon forgotten. The simple flaps of Verdouin and others, were rather made as a preventive against hemorrhage by being firmly fixed against the face of the stump, than as a means of forming a good basis of support, and the flap amputation appears to have been chiefly of the double flap kind at this early period, when the formation of the flaps had reference to the formation of the stump. The proceedings of

Vermale and Ravaton resembled to a certain degree the modern double-flap amputation, but whilst those of the former were rather short, being only about three inches long, those of the latter were unnecessarily bulky, as the amputation was performed by a circular incision down to the bone, and then by cutting a front and back flap by piercing the limb close to the bone, and cutting at right angles to the first incision. The flap amputation arrived very slowly at any degree of perfection, and was indeed perfected at a much later period than the circular mode. The circular mode of Louis which consisted in first dividing the skin and superficial muscles, followed by a higher incision of the deep muscles of the part, as well as the circular mode of dividing the skin and subjacent cellular tissue, and then dividing the muscles introduced by Cheselden and Pott, enabled them to gain a good covering to the bone, and to make a good stump in many cases; these plans were not, however, so likely to attain the perfect circular stump, which Mr. Alanson and Mr. Hey in England, and Richter in Germany, all strove to obtain by different means.

The peculiarity of Mr. Alanson's operation consisted in the division of the skin by a circular incision, followed by the incision of the muscles in a conical form, the apex of the cone being formed by the cut bone and situated at least three inches higher than the incision of the skin.

Mr. Hey and Professor Richter employed the circular incision of the integuments followed by an incision of the superficial muscles, and subsequently by an incision of the muscles situated near the bone.

In these two modes, and more especially in that of Mr. Alanson, the bone was divided high up, and well covered subsequently by the skin, and would produce a good stump. Great, however, as is the merit of Mr. Alanson's mode of amputation, his fame is not that only of a great operator but of an excellent surgeon; the mode of amputation is but a part of his treatment of these cases to which the accurate union of the stump, and the most careful subsequent treatment were also added, equal to the most improved surgery of the present day, and when it is not very well practised, immeasurably superior to it.

The flap amputations, though practised more or less for many years, have been chiefly introduced into London from the continent and Scotland. Although Benjamin Bell and Sir Charles Bell usually performed the circular operation, the double-flap operation has been practised by the officers of the Royal Infirmary of Edinburgh for some years, and has met with the approbation of Sir George Ballingall, Messrs. Syme and Lizars, and has been practised extensively by Professors Liston and Fergusson. Limbs have been cut off in the most varied manner now for many hundred years, yet it still remains undecided whether the flap or circular amputation is the best; but the experience of civil and military surgeons has decided that a good end may frequently be obtained in either way. The excellence of the stump is by all allowed to consist in a complete absence of pain and a perfect use of the remaining portion of limb, whilst the bone is free and covered with a good cushion marked by a narrow cicatrix; and such excellence the experience of Alanson and of the surgeons of Edinburgh have shown to be attainable in either way. But to men with daily experience in hospitals much is comparatively easy, which to the great majority must from limited expe-

rience be extremely difficult. The increase of the flap amputations within the last few years is to a certain degree an evidence of their success, as well as of the want of any objection against it, whilst the facility of performance, though not very great, is not so difficult to acquire as to form any just objection against them. One objection alone, the occurrence of secondary hemorrhage, has been urged against them; but the daily experience of surgeons has now removed this imaginary fear. Although the circular operation in good hands is capable of the greatest perfection, yet its perfection is not entirely dependent on the surgeon. When all proceeds well, the surgeon has nothing to regret; but let the stump inflame and suppurate, and a little bare piece of bone remain raw on the face of the stump, and then the subsequent misery of the patient is excessive, and may even justify a second operation. To all, even the most experienced, the amputation of a limb is a matter requiring great accuracy; but to one whose opportunities are limited, it is a matter of the most serious consideration: the difficulties on either side are great, but the tendency during the last few years has been to extend the flap operation, and apparently with an increase of credit to the surgeon and with relief and subsequent comfort to the patient.

12. *Subcutaneous division of muscles and tendons.* This operation does not appear to have been performed till the end of the seventeenth century, when Roonhuysen and Meeckren of Amsterdam divided the sterno-mastoid muscle in two cases of crooked neck, by exposing it and cutting it with a knife or scissors; this operation was also repeated by Gerrhard Ten Haaf of Rotterdam, and Sharp and Cheselden in this country: but previously to the year 1784, the only other muscle divided by exposure and incision was the scalenus of the left side, which was exposed by caustic, and then divided with the knife by one Minnius, probably of Amsterdam, about the year 1738. In the year 1784 the tendo achillis was divided in a case of varus, under the directions of Thilenius of Lauterbach, with success; this operation was repeated by Sartorius of Hachenburg in 1806, on a case of club-foot, with very indifferent success, both operators making a considerable wound in the skin, and the latter also using extreme violence to bring the foot to a proper position.

The incision of the tendons of the knee, fingers, and feet was practised by Michaelis, professor at Marburg, in 1809, with imperfect success; the incision of the tendon being intended to be only partial and being accompanied with exposure.

We now come to the labours of Delpech on this subject, and find that he published in 1823 a case of a boy cured of pes equinus by division of the tendo achillis as early as 1816; this case was, however, attended with very serious illness, and appears to have been the only one operated on by Delpech; but yet the attention of this eminent surgeon was drawn forcibly to this subject, as is shown by his later work.

Delpech, in his "Orthomorphie," published at Paris in 1828, speaks of the division of the tendo achillis as an operation not yet submitted to regular rules or methodically set forth, but as a useful resource; whilst he recommends as rules for the division of the tendon, that it should not be exposed but reached circuitously; that extension should not be tried at first; that extension of the means of union should be subsequently made, and the limb fixed in the new position, until the new tissue is firm. Such

are the rules given by Delpech, and which form even now the basis of treatment, whilst the veterinary surgeons operated with the greatest success in France and Germany on the deformed feet of horses and mules, and even prefaced their remarks in one paper by saying that they wished to render an operation generally known, which might perhaps be introduced into human surgery.*

Thus as early as 1828 the remedial means of curing many distortions had been shown in animals and in some few men, whilst the principles of the operation had been well laid down by Delpech; yet no general notice was taken of it; and even in 1835 (one year after the publication of Stromeyer's cases), Dr. Little at Leyden, Leipsic, Dresden, and Berlin, "in answer to his inquiries respecting the operation of division of the tendo achillis, experienced only disappointment." The skill of Stromeyer, however, enabled him to add the case of Dr. Little to his cures, who returning to Berlin, conveyed to Dieffenbach a certain proof of the ability of the present professor of Erlangen.

To Stromeyer is due the great merit of having first set the division of tendons by subcutaneous incision in a practical point of view before the profession; others had performed operations and detailed proceedings which collectively amounted to the same result; but his operation includes all the best points of his predecessors, with the separation of the dangers and the recommendation of a proper plan of treatment. In a short period after the introduction of the operation by him, we may enumerate Dieffenbach at Berlin and Dr. Little in London, who together treated by divisions of tendons the various forms of distortion of the feet, fingers, and neck, as well as contractions of joints, whilst Dieffenbach extended the operation to the cure of old dislocations, curvature of spine, and neck.

Although some may attribute the introduction of the operation for the cure of strabismus to an itinerant surgeon of Rouen, or an unknown surgeon in Italy, or find some slight priority in the operations of others, to Dieffenbach belongs the credit of making the division of the ocular muscles a regular surgical operation, the date of his first operation being December, 1839, just two months subsequent to the first operation of Cunier in France. Since this period the division of the ocular muscles has been practised and recommended by the most eminent surgeons in this country and on the continent. In Germany we may name Stromeyer and Dieffenbach, the introducers of this plan of operation, with Von Ammon, Fricke, Von Wattmann, Chelius, and Carus, whilst in France amongst the supporters of this operation are Velpeau, Jules Guerin, Bouvier, Roux, Duval, and the real original operator Cunier. In England the works of Mr. Lawrence and Mr. Lucas contain a complete summary of this operation, whilst the essays and cases of Messrs. Liston, Lizars, Mackenzie, &c. have contributed to its extension and improvement. The capitals of Russia and Sweden have had supporters of this new operation, and Dr. Hebra speaks with enthusiasm of the success of Diffenbach's pupils on the banks of the Ohio and Mississippi. The division of the recti muscles has been performed in cases of deficient vision and amaurosis, by Mr. Phillips of Brussels, for which latter affection Mr. Adams has also employed the same means; the success of this operation has, however,

* Journal pratique de Médecine Vétérinaire, 1826, p. 202.

not been sufficiently great or marked to induce the repetition of it by others, or at least to lead them to publish their results.

The division of the various muscles of the tongue was suggested soon after the introduction of "tenotomy" as a remedy for stammering, and practised at one time rather extensively; this operation is now rarely heard of, and appears either to have ceased to be a novelty or to have been merely at best a temporary means of relief; probably *at best* the latter. The incisions made by Velpeau, Amussat, Phillips, Baudens, and Bonnet appear to have chiefly acted on the genio-glossus, whilst those of Dieffenbach removed a large part of the tongue, and divided the lingual nerve and ranine artery. If these operations are even free from danger, they require for their performance some proof that the obstacle is in the tongue and remediable by this operation; but as they are not all free from danger and do not necessarily improve the patient, their rash performance is inexcusable in the highest degree.

Art. IV.

Handbuch der gerichtlichen Medicin, &c. für Aerzte und Criminalisten. Von Dr. G. H. Nicolai.—*Berlin*, 1841, pp. 556.

A Manual of Medical Jurisprudence, for Physicians and Jurists. By Dr. G. H. Nicolai.—*Berlin*, 1841.

The volume of Dr. Nicolai professes to be a manual for the guidance of medical men and lawyers in the examination of all medico-legal investigations. The subjects are treated in a somewhat condensed form, but nevertheless we are inclined to regard this work as a good addition to this branch of medical literature. In the first part the author treats of the questions connected with pregnancy, delivery, and criminal abortion. The peculiarities of the fœtus at all stages of gestation are then fully described, more minutely than appears to us necessary, in a work especially intended for practice. Thus we cannot see the necessity for a medical jurist being informed of the exact measurements in length and breadth of all the bones of the child; for it is not by such trivial and uncertain characters as these that he would proceed to determine the age of a child in a case of abortion or child murder. (p. 48.) The pages devoted to these details, might have been well spared for some other subjects of practical importance. The causes of the death of the child before, during, and after birth (p. 54) are very well described; but in other respects, the questions relative to frequency and parturition do not call for particular notice.

The subject of infanticide is somewhat briefly treated, at least as far as concerns that *vexata questio*, the evidence of the child having lived after birth, from the state of its lungs. It is well known that lungs may have undergone the process of respiration, and yet sink in water.

"Lungs will not float in water, when only a small part of their structure has been filled with air, when they are incompletely developed, or only expansible in certain insulated portions. It is a well-known fact that children may live and breathe a short time, provided only a very small portion of the lungs receives air; and they have been known to cry several times, when, after death it has been found that only a few detached parts of the lungs had undergone re-

spiration. The lungs of children which are immature, are very likely to be found in this condition, since these subjects may commence but have not the power to continue the process of respiration, or to give to the chest its full degree of expansion. This state is what has been termed *atelectases* by Jorg. Two cases have occurred to me in which the lungs of one side of the chest floated, and those taken from the other side sunk. These latter were of a dark-red colour, resembling the liver in consistency; they were congested with blood, and on compressing them, a bloody purulent matter oozed out. Both children had lived, the one two days, the other three weeks." (p. 80.)

The author states upon the authority of Mende, that in children which have survived birth, but have subsequently died from cold, the lungs lose their buoyancy in water. This statement appears to us to require confirmation. Nothing is added to our knowledge on the subject of artificial inflation; but Dr. Nicolai joins most medical jurists in the opinion, that the operation of inflating the lungs is of too difficult a nature to allow this to be made a serious objection to the employment of the hydrostatic test in cases of infanticide. He contends further, that there is this difference between respired and inflated lungs, that where the air has been received by respiration, "the lungs contain more blood; on cutting them, the blood streams out abundantly, and the pulmonary artery with its branches is filled with dark-coloured blood. On the other hand, when the lungs have received air by artificial inflation, they appear like a distended bladder or membrane without blood." (p. 81.)

Desirable as it would be in these cases, to find some good physical distinction between respired and inflated lungs, we must decidedly object to the reception of any such criterion as that here laid down by Dr. Nicolai. In the repeated examination of lungs under both conditions, we have not met with this difference which he describes; and we believe that it would be most unsafe to rely upon it as any evidence of the origin of the air contained in the pulmonary cells. Admitting that the lungs of children after respiration are more congested with blood than before the establishment of that process, many observers have met with cases of congested lungs in still-born children, that had not breathed. The notion that the presence or absence of blood in the pulmonary vessels can assist a medical practitioner in this important inquiry is now very properly exploded. All we can say is, that while Dr. Nicolai's conclusions are theoretically true, they are practically erroneous; and we cannot, therefore, agree with him in the assertion that a safe reliance may be placed on the quantity of blood in the lungs, to distinguish inflation from respiration.

The author next enters into a review of the various other tests for determining the question of respiration in new-born children. These rather belong to the history of the science, and therefore do not here require to be adverted to. The tests of Ploucquet and Daniel he considers unsafe.

The conclusions of the author on the subject of the hydrostatic and other tests, indicate the degree of uncertainty which attends the employment of them. Thus the buoyancy of the lungs in water is not a decided proof of respiration, nor is there absolute weight, unless taken with other circumstances. When the signs of respiration are entirely wanting, it cannot be affirmed with certainty, that the child has not respired. Respiration may have been performed, but imperfectly, and no physical signs of the process remain in the lungs. It is well known that a child may live some time after its birth without respiring; but there is not one

of the tests which can establish this condition. The sinking of the lungs and the absence of the other phenomena of respiration do not show that the child died *before* birth, and lastly, the fact whether the child breathed *before*, *during*, or *after* its birth, cannot be determined by these tests under any circumstances whatever. (p. 94.)

We fully agree with him, that because these difficult points cannot be settled by the hydrostatic test, this is no objection to its employment in cases of infanticide. Questions of this nature are entirely beyond the reach of its application; and it would be just as reasonable to reject the tests for arsenic, because they are unfitted to detect the presence of corrosive sublimate, as to reject this test on the ground above alluded to.

In dismissing this part of the subject we must here call the attention of English medical jurists to a point which is becoming of considerable importance, in relation to the medical evidence given in cases of child-murder. The difficulty in the present day, is not so much to determine whether or not a child has breathed, as to establish whether the act of respiration was performed *before* or *after* the *entire birth* of its body. It will be observed that at every assizes, where a charge of infanticide is tried, and such cases are now very numerous, the medical evidence may be clear as to the proof of respiration; but unless the witness is prepared to swear that the process was set up or continued *after* the body of the child was entirely in the world, this evidence is not held to be sufficient. The judge commonly stops the case by remarking that the proof of live birth has failed. We fully concur in the opinion of Dr. Nicolai, that in the present state of science, there is no certain sign whatever to indicate where the act of respiration took place in children that have lived but a short time. It is beyond the reach of any medical evidence to show whether the act was performed during the protrusion of the head from the outlet, or after the entire delivery of its body. There can obviously be no difference in the state of the lungs; for we are supposing, as it happens in most cases of alleged childmurder, that the child lives but a very short time. It is, therefore, improper to charge the hydrostatic test with a deficiency which cannot be fairly imputed to it. The point insisted upon as a necessary part of the evidence by our judges, requires some new proof or test which at present it appears to be beyond the reach of science to afford. In the mean time, the definition of child-murder should be changed; it is not the wilful destruction of a living child which the evidence is required to prove, but the destruction of a child which is proved to have breathed after every portion of its body was in the world. In the absence of ocular evidence, no proof of this kind can be possibly afforded; and as the benefit of the presumption is always given to the prisoner, so we do not see how with an advocate who thoroughly understands the difficulties of the subject, any prisoner, however guilty of the death, can in future be convicted of the crime of destroying her offspring.

The characters of infancy and puberty are next set forth; and we must here particularly point out the account of the changes that take place in the skeleton of the child. These are very well detailed, and they are well adapted to assist the practitioner in determining the age of a skeleton that may have been discovered buried under suspicious circumstances.

There are many questions connected with the bones of the adult after various periods of interment, which medical practitioners often find considerable difficulty in answering. This is owing to the subject of osteology never having been considered by them in its relations to legal medicine.

"Injuries to bones produced some time before death are known by the softer consistency and enlarged appearance of the parts involved. Traces of callus may also be found where there has been fracture. In determining for how long a period bones may have remained interred in the ground, it is necessary to consider the age of the individual; for this materially affects the rapidity of decomposition, as well as the kind of soil in which they have been interred. The bones of subjects abounding in liquid matter, of young persons and children, undergo decay with greater rapidity than those of old and emaciated subjects. The firm and hard bones resist decomposition longer than those which are soft and cartilaginous. Soils formed of loam or compact clay retard these changes in the bones: but in light sandy or calcareous soils decomposition is accelerated. The more freely bones are exposed to air, the more readily do they become decomposed. In general, the soft putrefaction of bone ceases in about twenty or thirty years from the time of interment. After this period, all the soft parts are destroyed: they are deprived of gelatin, and will be found hard and porous. The cartilages disappeared with the marrow and the fatty parts. If in examining bones, any of the soft parts remain, if they are oily and moist, and still contain within them some marrow, it may be inferred that they have lain within the earth from ten to fifteen years. If the bone readily crumbles under pressure; is rough and porous throughout its whole substance, it may be presumed that it has been in the ground forty, fifty, or even one hundred years." (p. 174.)

Although Dr. Nicolai's observations may be generally true, yet it must be remembered that there is a great dearth of facts in relation to the changes of bone within a given period. We have found gelatin and oil in bones that had been interred in chalk for a period of thirty-four years; and animal matter in bones that had lain in loose clay for upwards of half a century. Indeed it is a well-known fact, that bones may retain a portion of their animal matter, even after an interment of three or four centuries. There are so many influences that affect the rapidity of decomposition in bone, that it is difficult to lay down any general rules for determining the probable period of interment in an unknown case. Each case must be judged of by itself; at any rate we must be careful that we do not pronounce for a short period of interment, merely from the fact that there are still traces of animal matter in the substance of the bone.

The author entirely passes over one important question connected with the exhumation of bones, namely the mode of distinguishing the sex. There are many cases on record, where this evidence has been material, and it is easy to perceive that this is a question of as much importance as that which relates to the period of interment. In the early periods of life the means of distinguishing the bones of the sexes are very imperfect, and in consequence mistakes have frequently been made. Even in the adult subject, unless the whole of the skeleton has been disinterred, there may be some difficulty in pronouncing a positive opinion. The pelvis is well known to furnish the best mark of distinction between the skeletons of the two sexes; but the medical practitioner may merely have some of the bones of the upper or lower extremity, or a few of the ribs to guide his judgment. In such a case a decision will be difficult. The bones of the male are commonly stouter, heavier, and longer than

those of the female; and the processes and ridges are more strongly marked upon them, but it is undoubted that there are many male skeletons in which these characters are not very apparent; and other female skeletons the characters of which closely resemble those of the male.

The section on wounds does not satisfy us. The author has followed that bad artificial arrangement, which is adopted by most of his countrymen, and which takes its character from the singular state of jurisprudence existing in Germany in relation to local injuries. Thus we have absolutely mortal, conditionally mortal, and collaterally mortal wounds! although the injuries themselves afford no ground for such an arbitrary arrangement. This system of judging of the criminality of prisoners, not according to the intention displayed by the act of wounding, or the obvious results of their violence, but according to certain arbitrary rules serves no other purpose than to perplex medical witnesses, and to interfere with the proper administration of justice. Thus if a man is stabbed by an assailant, and he dies from hemorrhage, it can signify but little in relation to the wrong committed, whether the wound is in the brachial or in the common carotid artery; and yet according to this system of jurisprudence, there is a difference in the complexion of the offence; the one being a conditionally, and the other an absolutely mortal wound. In England the main inquiry would be whether death was caused by the wound; the prisoner would not derive any benefit from its anatomical situation, nor would it be necessary to speculate upon whether, had a surgeon happened to have been present, the deceased might have been saved in one case, although not in the other. Cases are of daily occurrence which show the absurdity of laying down or acting upon such capricious rules in relation to the mortality of wounds. The whole of this section is, in a practical view, without any interest to the English medical jurist; for the facts are stated by the author simply with a reference to the practice of the German courts. There is also an entire want of illustrative cases; and we know of no department of the science where teaching by examples could be made so profitable as in this. There are several faults of omission. Thus the subject of ecchymosis, involving numerous and highly important medico-legal questions, is entirely overlooked; nor do we find any rules mentioned for distinguishing between wounds inflicted on the living and the dead; and yet there is no doubt that as a matter of practice, this is a question of far greater moment than the settlement of the relative degrees of mortality in wounds of different parts of the body.

The subject of *malapraxis* in relation to the various branches of medicine has been but little examined by English medical jurists; although it must be admitted to be one of great interest to the profession. Incompetency in a regular, or ignorance in an irregular practitioner, appears to become a frequent matter of public investigation in Germany; but it seldom extends beyond a coroner's inquest in England. When trials for malapraxis have occurred, they have been in general ill reported, so that it was not easy to find out the truth of the case, or to make a profitable use of the report. Indeed such cases are confined to the daily journals: they are seldom met with in medical periodicals, and thus they are lost to the profession for all the purposes of medico-legal

practice. The author treats concisely of malapraxis in relation to surgeons, accoucheurs, and apothecaries; but it is easy to see that his attention has been principally given to obstetric practice; and it is well known that more faults are here committed by irregular or ignorant practitioners, than in any other department of medicine. The victim of mistreatment is sent to the grave, and nothing further is heard of the case. The only remedy for such a state of things must be a rigorous examination, and a special licence to practise. The malapraxis of chemists, or rather of dispensing druggists, is a matter which should likewise receive the attention of the legislature. It must be a matter of astonishment to all who reflect on this subject, that an individual should be allowed to dispense medicine without having given any test of his capacity to undertake so responsible an office; and yet this is at present the anomalous state of medical government in England. In this respect we form an exception to all other countries in Europe.

Dr. Nicolai devotes a long chapter to feigned and simulated diseases; but we do not consider this a subject involving many practical difficulties. Medical men are not often consulted in such matters in civil life; and when they are, they frequently do not show greater aptitude in discovering tricks and impositions than the civil authorities. This chapter is more especially adapted for army and navy surgeons, or those who have the medical superintendence of prisons.

In the chapter on suicide, we find the following remarks on the means of determining the period at which a gun or pistol has been discharged. The time may be divided into four periods:

"The first period lasts about two hours, and is known by the dark blue colour or efflorescence left on the piece by the discharge of the powder, by the absence of crystals of red oxide of iron (?) or of any salt of iron: by the slight turbidness of the solution derived from washing the piece, and the presence of sulphuretted hydrogen gas.

"Ferrocyanate of potash, (?) acetate of lead and tincture of galls will show whether sulphuretted hydrogen gas be present or not. As additional tests, we may observe in the first instance, the absence of any salt of iron, afterwards its presence, and finally its partial disappearance.

"The second period lasts twenty-four hours. The efflorescence on the weapon is not of so deep a colour: the solution is clear, there is no trace of sulphuretted hydrogen, or of red oxide of iron, and some salt of iron is present in the liquid.

"The third period lasts thirty days. Small crystals appear about the pan and touchhole; and these are long in proportion to the length of time since the discharge. Many traces of red oxide of iron may be found about the weapon, the presence of which may be easily shown by using the tincture of galls and the ferrocyanate of potash.

"The fourth period lasts about fifty days, and is distinguished from the third by there being less of the salt of iron, and more of the red oxide of that metal." (p. 293.)

Imperfect as this method of determining the period of the discharge of fire-arms must appear, we have quoted it because it displays a novel kind of research, and because we believe it to embrace all that is known relative to the subject. We do not think evidence of this kind would be received without opposition in an English court of law, but the practitioner should be aware of the attempts that have been made by continental medical jurists to furnish answers to these intricate inquiries. No one can doubt that great importance must often be attached to the

inquiry as to the probable period at which a particular weapon was discharged: for it may make all the difference between murder and suicide; but many may doubt the propriety of basing the answer upon such loose statements as these above given. In the first instance, the saline residue on the piece consists almost exclusively of sulphuret of potassium. It is alkaline, and is immediately affected by a salt of lead. In the course of from half an hour to an hour, we have found this sulphuret transformed into sulphate, where the air had free access to the weapon; and the solution lost its alkalinity and no longer affected a salt of lead. We never found any traces of iron where the discharge had taken place several hours previously. All we are entitled to say is, that the presence of an alkaline sulphuret indicates a recent discharge; but it is, we believe, impossible to specify the exact time at which it actually took place. In speaking of death from lightning the author observes:

"I have met with cases where a large portion of the surface of the body had been burnt by the action of the electric fluid, and the skin from the head downwards to the lower part of the trunk had become completely excoriated. The clothes were torn in the same direction, and in some parts were burnt or singed. The healing of these wounds was as difficult and painful as in cases of severe burns." (p. 315.)

It is the opinion of medical jurists, that the electric fluid does not produce burns on the body, unless some part of the clothing be ignited; and then of course the burning is an indirect result. We are inclined to think that this was the origin of the burns and excoriations in the instances referred to by Dr. Nicolai. In cases that have been carefully observed, where the dress of the individual has escaped combustion, the wounds produced by the electric fluid which had penetrated beyond the surface presented merely the characters of lacerated punctures, exactly resembling stabs with a blunt dagger.

The author is inclined to admit the doctrine of *spontaneous combustion* in the human body; but as he adduces no new cases to show that this alleged phenomenon ever began in the interior of the body, or even on the exterior without the contact or vicinity of fire, we do not see how the occurrence of this condition is placed, as he asserts it is, beyond the possibility of doubt. We are surprised that he should not have seen the necessity for better evidence to support his opinion than that which he adduces. He admits that all of these subjects have not been spirit drinkers; consequently a separate hypothesis must be invented to account for the spontaneous combustion of those whose bodies have not been impregnated with alcohol. Whatever opinion may be adopted, the subject has very little interest for the medical jurist; since the marks of design or accident in burning are commonly so clearly distinguishable, that not even a medical or scientific opinion is required, to satisfy a court of justice of the real mode of death.

The examination of blood-stains found on the dress of suspected persons has of late become an important part of the duty of a medical jurist. Dr. Nicolai does not attach much importance to inquiries of this sort, if we are to judge by the few lines in which the subject is treated:

"'A very simple method of recognizing suspected blood-stains in medico-legal investigations, is, besides the observation of the colouring particles, to examine

them by the light of a candle, in which case the slightest spots will appear of a clear red colour.

"Marks of blood on carpets, wood, or furniture generally, are not easily discovered by the light of day, but by the use of a candle, either at night or in the day time, spots no larger than half a line in diameter (1-24th of an inch) may be easily detected; after this they may be removed and examined microscopically or chemically. When dissolved in water, the red particles may be readily perceived." (?) (p. 321.)

This is all that the author says on the subject.* The methods for distinguishing other stains resembling those of blood, and easily to be confounded with them, are omitted, although this is assuredly a matter highly important in a practical view. There is no objection to the use of the lighted candle in the manner suggested; but a chemical analysis of the suspected stain should never be neglected. The best characters to distinguish blood are the ready solubility of the stain in water, the entire destruction of the red colour by coagulation on boiling, and the fact of the red solution not being turned to a crimson or green colour on the addition of a few drops of ammonia.

The subject of *insanity* is treated at considerable length; but as the points to which the author refers his readers chiefly relate to the rules and ordinances of Prussian law, they are without interest to an English practitioner. The medical history of the subject presents no novelty.

The last section of the work is devoted to *toxicology*. Dr. Nicolai defines a poison to be a substance which, when taken in small quantity, produces dangerous effects upon the system. This we know is a very common definition of a poison; but in our view it is erroneous and highly objectionable in its application to legal medicine. It is clear that if this definition were correct, the class of poisons would embrace but very few substances. Oxalic acid is a powerful poison, and yet compared with arsenic, it requires to be taken in very large doses in order to produce fatal effects. It cannot be doubted that carbonate of lead and tartarized antimony are poisons, but, compared with strychnia it requires a very large dose of either substance to affect the body dangerously or to cause death. Barristers, in defending prisoners on charges of poisoning, have taken advantage of the error in this definition, and have contended that no conviction could take place upon such a charge, because the substance administered was not a poison according to this usual medical definition. We repeat then, that a medical jurist must look to the effects of the substance upon the body, to characterize a poison: the term cannot be with any propriety restricted to that class of substances only which operate powerfully in *small* doses.

Dr. Nicolai has treated very fully the subject of arsenical poisoning, and the chemical processes for the detection of arsenic with the recent improvements are very well described. In his preface, the author acknowledges himself to be indebted to Dr. F. Simon, for an account of the chemical analysis of poisons, and we may observe that this gentleman has given a very able summary of what is at present known on the subject. While the chemical processes are often more simple, the action

* There is a paper inserted as an appendix at the end of the volume, which, however, appears to be a literal translation of Lassaigne's essay on the subject published some years since in the Révue Médicale.

of the tests is more clearly given than in most French and English works on medical jurisprudence.

As we might have expected, much space is given to an account of Marsh's test, and to the cautions which ought to be observed in its employment. The purity of the zinc and sulphuric acid having been determined by previous experiment, the operator must take care not to employ the same zinc a second time; for as the author observes, a portion of arsenic is liable to be precipitated on the zinc, which cannot be removed from it by merely washing with acid water. It is worthy of the attention of those who employ this test, that the arsenical crust may be deposited, but subsequently volatilized by holding the plate of glass or porcelain too long within the flame. Thus, should the quantity of arsenic be small, it may be erroneously supposed that none was present. The brightest and most metallic-looking stains, are obtained by merely depressing the point of the flame with the glass plate. It is recommended, however, when the quantity of arsenic is minute not to burn the gas; but to pass it through a small glass tube cemented into the apparatus, and to apply a strong heat by a spirit-lamp to one part of this tube. When the glass is heated nearly to redness, the gas is decomposed, the hydrogen escapes, and the metallic arsenic is deposited in a ring at a short distance from the heated spot. (p. 465.)

In distinguishing the antimonial from the arsenical deposit, the author refers to a difference which we have described in a former Number in reviewing Orfila's researches, namely, that when the crust is thin, the stain of arsenic is brown, that of antimony is of a dull gray or smoky black colour. There is, however, a new character which, he says, was first pointed out by Bischoff, namely, that the arsenical stains are all soluble in a solution of chloride of soda, (Labarraque's bleaching liquid,) the thin stain immediately, the black deposits more slowly. In decomposing this solution by muriatic acid, and gently warming it to expel chlorine, we obtain a liquid from which sesquisulphuret of arsenic is easily thrown down on passing into it a stream of sulphuretted hydrogen gas. The stains produced by antimony from Marsh's apparatus are wholly unaffected by a solution of chloride of soda. (p. 466.)

Supposing this to be confirmed by further observations, chemists will have here a good practical ground of distinction, between arsenic and antimony, as well as an easy method of separation when they happen to be deposited in a mixed state upon glass. Dr. Simon does not appear to have met with an account of the simple plan for distinguishing the stains lately proposed by Marsh himself, namely, to burn the gas, and allow the solid product of combustion to fall upon a few drops of ammonio-nitrate of silver in a saucer. If the flame contain arsenic, yellow arsenite of silver is formed; if antimony, silver is reduced and there is a black stain. It has been lately attempted to impugn the value of this mode of testing by the assertion, that if a phosphuret were present in the apparatus yellow phosphate of silver would be formed, resembling the arsenite in colour, and thus a phosphuret would be mistaken for arsenic. This is an objection more ingenious than sound. Mr. Marsh's correcting test answers the purpose intended, it clearly distinguishes arsenic from antimony: and supposing that an alkaline

phosphuret finds its way into a liquid for analysis, although we do not see how this can easily happen, it requires no great chemical skill to distinguish such a substance from arsenic. At any rate one who was unable to make this distinction could not safely be trusted with the use of Marsh's apparatus under any circumstances. We do not wish to underrate the difficulties attendant upon the use of this test. They are many: but we object to this wholesale introduction of speculative objections, many of which could only have been concocted in a closet.

Dr. Simon occupies some space in detailing the means for separating arsenic from antimony. (p. 468.) It is well-known that the hydrosulphuret of ammonia precipitates antimony but not arsenic. When the sesquisulphurets require to be distinguished, the better way is to boil the powder in a few drops of strong muriatic acid. The sulphuret of antimony entirely dissolves with evolution of sulphuretted hydrogen, and forms a colourless solution, which, provided the sulphuretted hydrogen be entirely expelled, gives a white subchloride on adding it to a large quantity of water. Sesquisulphuret of arsenic is insoluble in muriatic acid.

The researches of Orfila and Devergie on the detection of arsenic, absorbed into the body, and the experiments of the former on the presence of normal arsenic in the bones and muscles, as a natural constituent, are next adverted to, but Dr. Simon is not aware that Orfila has made a recantation of these views, and that it is now admitted there must have been some mistake in his processes. Arsenic is not present as a natural constituent in any part of the human body. It is unfortunate that Dr. Nicolai's work is thus made a means of diffusing a serious error; but it is a clear proof that medico-legal inferences of so extraordinary a character should be very slowly adopted, and only after a full examination of the facts by many experimentalists. It happened in this case that Orfila stood alone; but his authority was such as to lead readily to the diffusion of a statement as an ascertained fact, before there had been time for others to examine into its correctness, by a repetition of his experiments.

Poisoning by corrosive sublimate is very briefly treated: the pathological effects of this poison are dismissed in a few lines. The chemical analysis is more full: it goes to the detection of mercury only in organic mixtures, by the usual process of immersing in the liquid a slip of gold, having round it a coil of zinc. Dr. Simon condemns the use of tin recommended by Devergie; since, he observes, this often contains mercury, the presence of which, if unsuspected, might lead to a serious error. (483.) Whether this be the case or not, it is certain that the use of gold with zinc is more effectual than that of tin. For the detection of antimony in tartar emetic, sulphuretted hydrogen is recommended as the best test; but the following important fallacy is pointed out by the author:

"The presence of organic substances renders the reaction of this test occasionally obscure. I have remarked that a solution of tartar emetic, mixed with albumen, is not precipitated of a brick-red colour by sulphuretted hydrogen, but of a colour closely resembling the sesquisulphuret of arsenic! The ambiguity is removed by the addition of a few drops of hydrosulphuret of ammonia, which produce the usual orange-red colour." (p. 506.)

In testing for antimony, it must be remembered that the same reagents

act very differently on the two principal poisons of that metal, namely, tartarized antimony and chloride of antimony.

"Caustic potash gives with the first a faint turbidness, with the second an abundant white precipitate. Nitric and muriatic acids do not affect the solution of chloride: but they produce white precipitates in the solution of tartar emetic. Ferrocyanate of potash does not precipitate the latter, but it throws down the former, white. Both yield an orange-red precipitate with sulphuretted hydrogen and hydrosulphuret of ammonia; but the precipitate in both cases is redissolved by an excess of the last-mentioned reagent." (p. 505.)

As a test for oxalic acid, when in a pure state, the addition of chloride of gold with the subsequent application of warmth to the liquids is recommended. The gold, as it is well known, is under these circumstances reduced and precipitated in a metallic state. (p. 522.)

It is commonly imagined that free sulphuric acid is one of the easiest of the poisons to detect; but it is proper to observe that there are some difficulties connected with this subject, which must not be overlooked. For example, any acid, vegetable, or mineral, mixed with a solution of a common sulphate, might be erroneously pronounced to be sulphuric acid. Dr. Christison has fully pointed out this source of error; but the means which he recommends for the avoidance of it, are somewhat complicated. It may happen that sulphuric acid may be mixed with a sulphate in a liquid submitted to analysis. In such a case, carbonate of barytes may be added to the liquid warmed, and the whole of the free sulphuric acid will thus be thrown down as sulphate of barytes. Dr. Simon's method of separation is as follows:

"As free sulphuric acid is soluble in pure alcohol, and the sulphates are insoluble in that menstruum, the liquid should, after filtration, be evaporated at a gentle heat to a small bulk, and then digested with absolute alcohol. This alcoholic solution should be set aside for twenty-four hours, again evaporated to a syrupy consistency, and the residue again treated with pure alcohol. On filtration, the alcohol may be separated from the sulphuric acid mixed with it by evaporation, and the acid then discovered by any barytic salt." (p. 527.)

This appears to us to be a better process than any yet suggested by toxicologists for separating free sulphuric acid from mixed sulphates. Dr. Simon makes no further application of it; but we think it might be made serviceable for the separation of any acid capable of combining with alcohol, (as the acetic) from sulphates, and render unnecessary the process of distillation recommended by Dr. Christison.

The process for hydrocyanic acid is not very satisfactory, at which we must express our surprise, considering the great simplicity of the analysis. Nitrate of silver is recommended as a test; but it is said that the cyanide of silver in drying becomes almost black, a statement opposed to the most common observation; for it is one of the least changeable of the salts of silver, and this has been assigned to it as one of its chemical characters by Mr. Barry. We agree in the statement that it differs from the chloride, in being easily decomposed by heat; but the author does not say that when heated it yields cyanogen, which burns with a peculiar flame, and thus renders the salt easily distinguishable from every other compound of silver. (p. 553.)

To prevent the loss of this poison in separating it by distillation from any organic mixture it is recommended to place a small quantity of al-

cohol in the receiver, with a few drops of caustic potash. It is customary to employ, under these circumstances, nitrate of silver, a solution of which is placed in the receiver; but, as Dr. Simon observes, it is possible that muriatic acid may be present and be distilled over, thus giving rise to ambiguity. To prevent this, the free acid in the liquid should be first neutralized by potash, and then, before distillation, it may be slightly acidulated with acetic (tartaric) acid. (p. 537.)

The author prefers, however, to use the ammonio-nitrate of silver in the receiver, by which he thinks the vapour of the poison is more likely to be absorbed; diluted nitric acid will afterwards throw down the cyanide. He omits to mention a very important chemical distinction between the cyanide and chloride of silver, namely, that the former is entirely soluble in caustic potash, while the latter is insoluble. He recommends as an additional test, the addition of sesquichloride of iron to the solution of cyanide in ammonia, in order to produce Prussian blue. The production of Prussian blue from the solution of cyanide of silver in potash, is an excellent addition to the tests for prussic acid, and has for some time been known in this country. It is not, however, noticed in works on toxicology. The author evidently imagines, that this modification of applying two good tests to one single portion of suspected liquid is original; but with the exception of the substitution of ammonia for potash as the solvent, by which no advantage is gained, it has been long practised here.

The chapter on opium is defective, both pathologically and chemically. The symptoms are dismissed in five lines, and the post-mortem appearances in two. Indeed, if we except the chemical analysis of poisons, we must look upon the author's mode of treating toxicology rather as furnishing a synopsis than a finished essay of the subject. The doses of the poisons required to prove fatal, the time within which death takes place, the antidotal treatment, and numerous other interesting questions relative to the practical part of the subject, are not even referred to.

It is not common to find the chemical processes for detecting strychnia and brucia in works on toxicology; and we shall, therefore, extract a few of Dr. Simon's remarks on this difficult subject. Some years since, it was scarcely possible to obtain a specimen of strychnia which did not contain traces of brucia; hence it was difficult to distinguish these two bodies from each other, which otherwise bear a close resemblance. Strychnia may now, however, be procured quite pure, and the following are the tests for its presence:

"Concentrated nitric acid dropped upon strychnia or its salts gives to it a lemon-yellow colour; if the strychnia contain any brucia, the colour is first of a light rose, passing afterwards into a deep red. Brucia is first changed by nitric acid to a rose red, and afterwards to a deep blood-red tint. A solution containing 1-100th of strychnia, is scarcely affected by concentrated nitric acid; if any brucia be present, an orange colour is produced. A solution of brucia, of equal strength with that just mentioned, is turned by a few drops of strong nitric acid, to a dark Malaga wine colour. Pure strychnia, treated in the same way with concentrated sulphuric acid, acquires a yellowish colour; if it contain brucia, it is at first rose-red, and afterwards of a brown-red. Pure brucia becomes of a deep brown-red, and finally passes to an olive-green. Sulpho-cyanate of potash causes in a solution of strychnia containing 1-100th, a turbidness soon followed by a white precipitate. If less concentrated, fine needle-shaped crystals

form in the liquid after some time, which gradually increase in size. In a similar solution of brucia, no turbidness or precipitate occurs on adding the sulphocyanate, and it is only after some hours that a horny crystalline deposit attaches itself to the sides of the vessel. Both solutions are precipitated by infusion of galls, or any liquid containing tannin." (p. 546.)

As a poison, it is necessary to remember that impure strychnia is much more likely to be seen than that which is absolutely pure. We have seldom met with a specimen of strychnia which has not been turned of a red or orange colour by nitric acid; and this colour in the course of an hour commonly passes to a dark brownish green, a character which strikingly distinguishes it from morphia similarly treated. This is at first of a rich red, and slowly passes by exposure to a light yellow tint.

In order to separate strychnia or brucia from the contents of the stomach, the following process may be pursued:

"We must separate the solid from the liquid portions, digest the solid parts repeatedly in acetic acid, then mix the resulting liquid, and evaporate to dryness in a water-bath. This residue must be treated with alcohol, and to the concentrated alcoholic liquid, ammonia may be added to precipitate the alkaloids. The precipitate should be redissolved in acetic acid, and decolorized by boiling with animal charcoal. On evaporating this mixture, and digesting in alcohol, the acetate of strychnia or brucia may be obtained in a crystalline form, by spontaneous evaporation, or, as it sometimes happens, a very bitter extract remains, which is affected, like strychnia or brucia, by the addition of nitric acid." (p. 547.)

Perhaps this is as simple a method of obtaining these alkalies from the stomach in cases of poisoning, as any that can be suggested; but it will be seen that there is a good deal of difficulty and trouble attending the process; nor can the operator always hope to succeed unless he has had some experience in extracting these alkaloids from the substances in which they exist.

With regard to nux vomica, this can only be detected by seeking for the strychnia which it contains, having first separated as much as possible all foreign matters from the residue of the poison found in the stomach. In this case, we must boil the substance repeatedly in alcohol of 60 or 70 per cent., filter, concentrate and boil this alcohol extract, with calcined magnesia and water. This causes a precipitation of the strychnia, which may be afterwards separated from any extraneous matter by boiling alcohol. (p. 545.)

We here conclude our notice of Dr. Nicolai's work. Our remarks on chemical toxicology have been full; because it is now some years since a new edition of Christison's treatise has appeared; and there is no other English work on the subject that we can recommend. In the mean time, many improvements have been made in the processes for detecting poisons. We consider this, on the whole, to be a useful manual of medical jurisprudence. For the benefit of those, who wish to study the subject more deeply, the author has attached to each section the names of the authorities whom it would be advisable to consult. A work of this description is much wanted in the English language.

ART. V.

1. *On the different forms of Insanity in relation to Jurisprudence.* By JAMES COWLES PRICHARD, M.D.—*London*, 1842. 8vo, pp. 243.
2. *The Plea of Insanity in Criminal Cases.* By FORBES WINSLOW, M.R.C.S.—*London*, 1843. 8vo, pp. 78.
3. *Criminal Jurisprudence considered in relation to Cerebral Organization.* By M. B. SAMPSON.—*London*, 1843. 8vo, pp. 147.
4. *Commentaries on some Doctrines of a dangerous tendency in Medicine.* (Comm. III.—*On some Important Questions relating to Insanity, both in a Medical and Legal point of view.*) By SIR ALEXANDER CRICHTON, M.D. F.R.S.—*London*, 1842.
5. *Report of the Trial of Daniel M'Naughten for the wilful Murder of Edward Drummond, Esq.* By R. M. BOUSFIELD and RICHARD MERRETT.—*London*, 1843. 8vo, pp. 78.
6. *M'Naughten. A Letter to the Lord Chancellor upon Insanity.* By J. Q. RUMBALL, Esq. M.R.C.S.—*London*, 1843. 8vo, pp. 35.
7. *On the Amendment of the Law of Lunacy; a Letter to Lord Brougham.* By a Phrenologist.—*London*, 1843. 8vo, pp. 39.

THE great interest which is taken by the profession and the public in the medical jurisprudence of insanity is we think sufficiently evinced by the number of publications which have lately appeared on the subject. Some heinous crimes have been of late years perpetrated, which have called especial attention to the subject of homicidal insanity, and the most recent case, namely that of M'Naughten, has in some respects modified the old legal doctrine of responsibility. Several of the works placed at the head of this article have been called forth by the medical evidence given on this occasion, as to the irresponsibility of the insane. Before noticing these we shall first proceed to examine a work of more general and permanent interest.

I. The small volume by Dr. Prichard may be regarded as an abridgment of the treatise on Insanity, by the same author, which we reviewed in our Thirteenth Number. It is written in a more popular style, and is evidently intended for the use of commissioners and juries, engaged in the investigation of questions relative to the sanity or insanity of individuals. This, indeed, the author professes to be his object in the preface. A very large portion of the treatise is occupied with a description of the characters peculiar to the various forms of insanity, and of the means of distinguishing one form from another. Every point relative to treatment and the medical management of the insane is of course avoided, as not being within the scope of the work. We pass over the author's analysis of the conflicting opinions of lawyers on mental unsoundness, this subject having been already fully treated in a previous number, (No. XIX, p. 120.) It will be our object to comment in this place on those parts of the treatise only, the subjects of which were but slightly noticed in our former article on the medical jurisprudence of insanity.

Dr. Prichard believes it to be the settled doctrine of the English courts at present, that there cannot be insanity without delusion, or, as it is otherwise expressed by physicians, without illusion or hallucination, that is, without some particular erroneous conviction impressed upon the understanding, the affected person being otherwise in possession of the full and undisturbed use of his mental faculties. This, he observes, is the doctrine of partial insanity, so that a man is supposed to be mad upon one point, and sane on every other particular, a state in itself most incredible. The only admissible view of partial insanity is that which was taken by the present Lord Chancellor (Lyndhurst.) "It is that the mind is unsound, not unsound on one point only, and sound in all other respects; but that this unsoundness manifests itself principally with reference to some particular object or person." (p. 16.) The author further remarks that mental derangement in almost every case not only involves a disordered exercise of the intellectual faculties, but extends even further than the understanding, and implicates more remarkably the moral affections, the temper, the feelings and propensities; that it affects in reality the moral character even more than the understanding.

The author devotes considerable space to the subject of moral insanity, by which we are to understand a disorder affecting only the feelings and affections, or what are termed the moral powers of the mind, in contradistinction to the powers of the understanding or intellect. In our notice of Dr. Prichard's former work, (No. XIII, p. 6,) we expressed the opinion that there is some difficulty in admitting the existence of any form of moral insanity, disjoined from some degree of mental infirmity, less perceptible in the slighter, but manifest in the more severe cases. Esquirol is also opposed to this view of Dr. Prichard's; he does not consider it possible that the intellect can be in a state of integrity in what is called moral insanity. The truth probably is, that the understanding is disturbed in all the cases, only more in some and less in others. Our opinion is not shaken by the cases cited by Dr. Prichard. We may find it in some instances difficult to detect the evidence of any "fixed notion" which influences a man's conduct on the existence of any "delusive ideas;" but still we believe that there is in every case of true insanity some latent disorder of the intellectual powers.

In a subsequent chapter, when treating of the irresponsibility of madmen, the author complains, "That the attention of those who have hitherto investigated cases of insanity has been too much directed to the particular error which clouds the understanding, or to the disordered state of the intellect, or judging and reasoning powers; whereas, in reality, it is of the moral state, the disposition and habits of the individual concerned, that the principal account ought to be taken." (p. 175.)

We admit that there is some truth in this observation, and that time is often wasted by a physician in endeavouring to detect some absurd delusion; but, nevertheless, this is often the only way by which we can arrive at a knowledge of the course on which the perversion of the moral feelings and affections depends. If all physicians admitted, like Dr. Prichard, that moral insanity was perfectly independent of and distinct from what is called by him intellectual insanity, then there might be some reason for censuring this limited method of inquiry into the state of the intellect; but as the common opinion is rather opposed to this view, we

conceive that a man who looked only to the habits and disposition of a party would often be led into error.

We have already seen that the author is himself a strong opponent of the doctrine of partial insanity, as applied to the intellectual powers. He believes that in all such cases there is more or less disturbance of the whole of the intellectual faculties. But we doubt whether the facts adduced in support of this view be any stronger than those which tend to show that there is no form of moral insanity without some trace of perversion of the intellect. We are glad to perceive that the author, notwithstanding his belief in the nonexistence of partial insanity, as it is commonly understood, is opposed to the general practice of placing such persons under restraint on the most trivial pretexts.

"A man who, like Count Swedenborg, labours under a harmless hallucination, or fancies that he converses with angels and meets Moses in Cheapside, ought not to be confined in a lunatic asylum. But it will generally be found on inquiry, that persons who have illusions are otherwise incapable of exercising civil rights." (p. 67.)

All we can say is, if such persons are shown to be incapable of performing their civil duties, let them be placed under proper restraint or interdiction; but it has happened in many cases, and in our view most unjustly, that the incompetency to manage affairs has been a hasty inference from the proof of the existence of some foolish notion in the person's mind. How far this notion affected his actions was not so much a matter of inquiry as the mere proof of its existence. We have known an instance where a lady was interdicted in consequence of her entertaining the belief, in which she persisted before the commission, that the Duke of Wellington was in love with her, and was about to marry her. We could not ascertain from the evidence that this delusion had operated injuriously upon her general conduct, or that it had thrown her affairs into disorder.

In some cases of this kind the object of an inquiry has been almost defeated by the extreme difficulty which existed in detecting any delusion in the mind of the alleged lunatic. When this delusion was ultimately laid bare by the ingenuity of a physician, after a cross-examination of some hours, the result has been looked upon as a matter of triumph and justification; but, as Dr. Conolly has remarked, in speaking of cases of this kind, one point seems to have been wholly lost sight of; namely, what injury could have resulted to the patient or his friends from the presence of a delusion in his mind, over which he had such control, that it required many hours and much ingenious questioning even to detect its existence? Of course we admit that these cases require close watching, since the existence of any kind of delusion indicates more or less of a disordered state of the mental faculties.

In treating of homicidal insanity, and of the means of distinguishing the insane homicide from the sane murderer, Dr. Prichard observes:

"The act of homicidal insanity is different in its nature and moral causes from that of the murderer. Men never commit crimes without some motive; the inducement which leads them to an atrocious act is of a kind which other men can appreciate and understand, (?) though they do not sympathise with them. Jealousy, hatred, and revenge excite some; others are moved by the desire of plunder, of getting possession of money or property. The act of a madman is

for the most part without motive, and even contrary to all the imaginable influence of motives. .... There have been some instances in which the perpetrator of insane homicide has been excited by a real monomaniacal delusion, a firm and strong belief that he acted under a divine command." (p. 127.)

In our former article, the subject of homicidal insanity was examined chiefly as to one of its characters, namely the existence of *delusion*. We said little about *motive;* and it is therefore now our purpose to make some remarks on this assumed criterion of sanity in the commission of crime. The question is the more important, because since the article referred to was written this very criterion has been made a prominent subject of discussion in relation to the attempts made on the life of the sovereign, as well as in respect to certain other atrocious crimes committed on private individuals. From the above paragraph, Dr. Prichard evidently assumes that sane men never commit a crime without an apparent motive; and that an insane person never has a motive, or one of a delusive nature only, in the perpetration of a criminal act. If these positions were true, we apprehend that there would be no difficulty whatever in distinguishing a sane from an insane criminal; but in practice the rule is only applicable to a very limited extent, and is often liable to mislead.

The fallacy of trusting to such a criterion must, we think, be apparent by examining the question, whether the responsibility of a criminal is to depend *on the discovery or non-discovery of a motive* for the act of which he has been guilty. We put the question in this, the plainest, light, because, in point of law, the non-discovery would be taken as synonymous with the non-existence of motive; and the prisoner in such a case would receive the benefit of the doubt incurred by deciding on his responsibility according to this false criterion. "De non existentibus et non apparentibus eadem est ratio" is a well-known rule of law.

Georget and others who, with Dr. Prichard, have advocated this view, have forgotten that there is a very wide distinction between the non-existence and the non-discovery of a reasonable motive for the commission of a crime. It is undoubted that a motive may exist for the most atrocious criminal act without our being able to discover it. This is abundantly proved by the numerous recorded confessions of criminals before execution, in cases where, until these confessions had been made, no possible motive for the perpetration of the crimes had appeared even to the acutest minds. Two persons are seen together on friendly terms; one is afterwards found murdered, and circumstances inculpate the other. No motive appears for the crime, for he makes no confession, and no eye witnessed the transaction. Are we, therefore, to acquit the accused and pronounce him irresponsible? In the case of Courvoisier, who was convicted of the murder of Lord William Russell in June 1840, it was the reliance upon this fallacious criterion, before the secret proofs of guilt accidentally came out, that led many to believe he could not have committed the crime; and the absence of motive was urged by his counsel in defence as the strongest proof of the man's innocence. It was ingeniously contended: "that the most trifling action of human life had its spring from some *motive* or other." The sophistry of this position is very evident. Allowing that all the actions of men proceed from motives, it is not always in our power to discover them; and therefore it

would be in the highest degree absurd to rest the innocence or responsibility of an accused person on so feeble a ground.

On the trial of Francis for shooting at the queen, one main ground of defence urged in proof of the prisoner's irresponsibility by his counsel, was that the prisoner had *no motive* for his act. In the language of Dr. Prichard, it was argued, that "*men never commit crimes without some motive.*" It is thus we see the danger of giving circulation to unsound medico-legal axioms. We shall here quote the observations of the present solicitor-general on the occasion of this trial, in reference to this question of motive, since they strike at the root of the false principle involved in this assumption :

"This doctrine about motive is of a most dangerous character, and must be very guardedly received. It is very difficult for you (the jury), very difficult for any well-regulated minds, not accustomed to contemplate the workings of iniquity, to discover the motives for crime. What motive instigated the execrable assassin in Paris, who shot at his king and deluged the streets with blood by means of his infernal machine? Did any one ever hear propounded in a court of justice a doctrine that would lead to such dangerous consequences to society as that you must ascertain the motive before you convict of the crime?"

It is an undoubted fact, that crimes are sometimes committed without any apparent motive by sane individuals who were at the time perfectly aware of the criminality of their conduct. Some years ago there were, in this metropolis, several miscreants who went about stabbing females with knives or dirks in the dusk of the evening, inflicting thereby dangerous wounds in the groin and thighs. The females were perfect strangers to them; there was no attempt at robbery, no demand for money, and in short no sort of motive could be discovered, on their apprehension, for this most wantonly malicious conduct. That they were fully aware of the crime was proved by their endeavouring to escape immediately after its perpetration. No evidence of insanity or delusion could be discovered about them. They had nothing to say in their defence. The crime was soon checked by very properly making them responsible, notwithstanding the absence of motive, and by inflicting on them severe punishment. No case can more strongly show the impropriety of resting the question of responsibility on general rules of this kind.

We are willing to admit, that an absence of motive may, in some instances, favour the view of irresponsibility for crime. These cases are, where there are other strong evidences of insanity; but what we strongly protest against is that insanity and irresponsibility should be inferred to exist whenever a motive for a crime cannot be detected.

But on the other hand—Does the discovery of what may be called a rational motive for a crime like murder, such as revenge for real injuries received, necessarily indicate that the party is sane and that he should be held responsible? Dr. Prichard does not exactly touch upon this question, although his language evidently implies that the existence of such a motive should render the party responsible. It may, however, be fairly objected to the exclusive application of this doctrine, that attendants have been on several occasions murdered by lunatics, where the latter have experienced violence or ill-treatment at their hands. A case of this kind is related by Dr. Haslam, and will be given hereafter. In answering such a question we are bound to consider, whether in insanity

all those passions and feelings are annihilated which so often supply motives for crime to some men. It is, we believe, an undisputed fact that the insane are susceptible of revenge; but they are never known to commit crime for the sake of robbery or for the concealment of another crime.

Thus then, admitting as a general principle, that real motives for crime are only imputable to the sane, we must contend that the absence of all apparent motive is neither to be taken as absolute evidence of insanity nor as a plea for irresponsibility. And again, the occasional discovery of what appears a rational motive for crime, such as revenge or hatred, must not lead us to suppose that the individual is necessarily sane and responsible for his act. We believe that great mischief is done both in and out of the profession, by laying down such criteria for the distinction of homicidal insanity. It would be far better to admit that there are no well-founded means of diagnosis. Each case must be judged of by itself: it cannot be determined by any legal or medical rules derived from a few cases, or founded on a narrow circle of observations. If the crime has proceeded from the usual incentives, if the criminal is aware of the real effect of a plea of insanity, and exerts himself to create a belief in others that he is really insane, there can be no pretence whatever for exculpating him, even although we may be ignorant of his real motive in committing it, or the act itself may be "contrary to all the imaginary influence of motives."

We agree with Dr. Prichard in considering "that *suicide* is not always the result of insanity. (p. 134.) We need not enter into his reasons for this opinion, as the subject has been already sufficiently discussed in previous articles.

In regard to *lucid intervals* the author thinks that we ought never to convict for a crime committed during this state, "because there is every probability that the individual was under the influence of that cerebral irritation which makes a man insane." (p. 201.) We consider that there is great truth in this remark, and we do not now find that juries are so ready to convict a prisoner who is admitted to have been insane within a short period of time before the perpetration of the criminal act with which he is charged.

In treating of imbecility, the author adopts the artificial degrees of this state proposed by Hoffbauer; although he substantially admits that the application of any such divisions to practice would be less successful than the method at present pursued by English lawyers. To this chapter there is attached but a very slight and imperfect notice of the celebrated case of Miss Bagster, of which we have elsewhere spoken. (No. XIX, p. 155.) We much doubt whether the author can have read the excellent report of this case, published in the Medical Gazette, (vol. x. p. 519;) for we find that he completely mistakes the evidence of Dr. Morison and Dr. Haslam in saying: "Drs. Morison and Haslam had both visited her, and were disposed to consider her imbecile or idiotic." (p. 227.) What Dr. Morison did say on this occasion is as follows: "Her (Miss Bagster's) incompetency to manage her affairs, arises not from unsoundness of mind, but from ignorance," and Dr. Haslam's evidence stands thus in the report: "She (Miss Bagster) is not a lunatic, she is not an idiot, nor is she of unsound mind." We feel disposed to think that our English

author has taken his account from the American writer, Dr. Ray. It is to be regretted that this work should thus have been made the means of circulating an error with respect to one of the most important medico-legal cases which have occurred in modern times.

In concluding our notice of this volume, we must repeat a complaint which we made in reviewing the treatise by the same author a few years since. His references are almost invariably made to foreign documents. It is true that some of the best works on insanity are in the French and German languages; but we do not see why, if these were consulted as authorities, his illustrations should not have been derived from English cases. There is no want of cases in England to illustrate every department of the medical jurisprudence of insanity. Not a single year passes without many curious and intricate questions relative to the civil and criminal liabilities of lunatics coming before our courts of law. Dr. Prichard entirely neglects these, and takes up the old and well-known cases of Esquirol and others. The important case of Oxford, tried for shooting at the queen, is not even noticed; that of Francis for the same offence probably had not commenced until the greater part of the work had been written.

On the whole, however, we do not hesitate to say that this will be a useful manual to those whose avocations do not allow them sufficient time to go deeply into the subject of insanity. It may be considered as containing the essence of the author's former work, the reputation of which is sufficiently established to ensure a wide circulation to the present treatise.

II. The essay of Mr. Winslow is chiefly confined to the subject of the irresponsibility of the insane for criminal offences. While the author presents us with nothing new, he gives a very fair popular outline of the modern doctrine of homicidal insanity. The dicta of different judges, from Lord Hale downwards, are cited to show that the views of lawyers are wholly unsettled on this subject. The only conclusion to be derived from an examination of these authorities, is that, in cases of insanity, "the law looks to the capability of distinguishing between right and wrong; of the person knowing that the crime of which he stands accused is an offence against the laws of God and man." (p. 9.) In plain language, the conclusion amounts to this: if a man knows that what he is doing is contrary to law, he is responsible for the act, otherwise not: right and wrong must then here stand, as Lord Brougham has suggested, for legality and illegality. The difficulty lies, however, in applying such a test. How are we to discover what a man's views are of the legality or illegality of the act which he is perpetrating? We cannot take it from his confession; and if we take it from circumstances, we are very liable to be deceived. Bellingham did not admit that he had done wrong in shooting Mr. Perceval; and there was every reason to believe that he was insane: he was however convicted and executed. Martin, the incendiary, admitted that he knew he was doing wrong, according to the law of man, when he set fire to York Cathedral; he knew that the act was illegal, but he said he had the command of God to do it. There was no doubt that it was perpetrated under a delusion, and he was acquitted. Thus, then, it appears from this case that a man may have a

full conviction that the act which he is perpetrating is illegal, and yet be held irresponsible. Some homicidal monomaniacs have committed murder, in order that they might suffer death according to law, considering that they were forbidden to destroy themselves. They must, in these cases, have been fully aware that the crime which they were about to perpetrate was contrary to law, and have actually looked forward to the punishment which they conceived would deservedly follow the offence. For a remarkable instance of this kind, see No. XIX. of this Journal, p. 144. The case of Hadfield, who was tried for shooting at George III., furnishes another striking example of the existence of insane delusion, coupled with a knowledge of the consequences of the act which he was about to commit. He knew that in firing at the king he was doing what was contrary to law, and that the punishment of death was attached to the crime of assassination; but the motive for the crime was that he might be put to death by others; he would not take his own life. The legal test, then, here falls short of what is necessary for justice. A consciousness that the act committed is contrary to the law of God and man may exist, and yet the person be held irresponsible. Hence this mode of testing criminal responsibility, withou taking into consideration numerous other circumstances, is incorrect. Cases occur in which it is impossible to act upon it.

"Many able writers on jurisprudence," says Mr. Winslow, "have maintained that in no instance where insanity is established ought the person so unhappily affected to be considered a responsible agent. So little, it is said, is known of the condition of the mind, even in cases of decided monomania, or delusion upon one point only, that it is impossible to form anything like a correct notion of the condition of the other mental faculties, or to ascertain with anything like precision to what degree they may be occasionally implicated in the disorder, and thus drive the person to the commission of capital offences." (p. 12.)

While he thus fairly represents his views of this difficult question, Mr. Winslow is inclined to the opinion, that the existence of any one predominant delusion, and that delusion not in the slightest degree associated with the criminal act, should be occasionally considered as a sufficient exculpation in criminal cases. The author, however, expresses himself so cautiously on this point, that we have but little difficulty in agreeing with him. The mere fact that a delusion exists in the mind is sufficient to create a suspicion of general mental perversion, and therefore to make out a primâ facie case of irresponsibility. Nor would we rest the exculpation of the accused entirely on the connexion between the delusion and the act being established by the evidence; because 1, this connexion may exist but will not admit of proof; and 2, it may not exist, and yet there may be such a degree of mental disorder as to justify a plea of irresponsibility. It is, however, a very different matter to assert that the mere proof of a delusion existing or having existed in the mind should render a party in all cases irresponsible for his act. It may be a strong ground for suspecting insanity, but it is nothing more; we should not be justified in inferring irresponsibility until all the circumstances of the case had been fully investigated. It might turn out, notwithstanding the existence of delusion, that the crime had every mark of sanity about it, just as we find in the execution of wills and deeds that their validity is occasionally admitted in spite of the obvious existence of delusion.

The reasonableness of the act and the mode in which it is performed are in all such cases closely scrutinized; and a civil act is not necessarily held to be invalid, unless it bear about it obvious marks of the individual having been influenced by his delusion in drawing it up.

Mr. Winslow mentions some facts which prove beyond all doubt that persons confined as lunatics are capable of committing offences with the full knowledge that they would escape punishment by insanity being pleaded for them; in short, they are aware of the effect of the plea, a state of mind which some medical writers have held to be a well-marked criterion between one who is an impostor and another who is homicidally mad. It is certainly a fair matter for consideration, whether, when the mind is thus capable of drawing a perfectly sane inference as to the consequence, the individual should not be held as much responsible for his act as if no delusion were proved to exist. This of course involves the question of how far punishment would be attended with a good effect, a subject which we shall reserve for consideration hereafter. As far as the law is concerned, it is pretty certain that where so much knowledge of the consequences of an act was displayed, the individual perpetrating it would be held responsible. A man would not be allowed to forge a will or a deed, and escape upon the plea that being a lunatic, or labouring under some delusion, he should avoid the punishment due to his act, when, in the very perpetration of it, he contemplated an acquittal on the ground of insanity. If a man has sufficient knowledge to calculate upon an escape from punishment, it is reasonable to suppose that he has a sufficient power of self-control to prevent him from perpetrating the crime for which such punishment is commonly inflicted.

At p. 17 the author quotes a very instructive case, in which, as he observes, if the murder perpetrated had occurred *out of a madhouse*, with all its attendant circumstances, there would have been no doubt as to the justness of the penalty had the individual been condemned to expiate his crime on the gallows. A man confined in a lunatic asylum had been subjected to very cruel treatment, and he killed the person who had the care of him. For the act itself there was the strong motive for revenge; the man had threatened the life of his keeper; and in the perpetration of the murder, there were displayed cool premeditation, precaution, and concealment of the means, which are commonly considered as characteristic of the sane assassin. The man, known to be insane, was justly acquitted; but had he been at large when the act was committed, it might be questionable whether his execution might not have been justifiable. A mind which was thus able to display all the usual marks of sanity in the perpetration of a heinous crime, would be influenced by the example of punishment; and others, like this man, might be thereby so deterred from the perpetration of murder under similar circumstances. Unless some such rule as this be adopted, it is difficult to understand how we should ever be justified in inflicting punishment for any offence. Exculpation cannot depend merely upon the fact of a man being confined in an asylum; for, while a responsible sane person may be thus unjustly confined, another who might with propriety be considered irresponsible may be enjoying his liberty.

The case of Earl Ferrers, who was executed for the murder of his steward about the middle of the last century, has given rise to great difference of opinion among jurists and physicians. Lord Erskine thought

that the Earl was properly executed for the crime of murder; because his resentment was not founded on any illusion (delusion?); but it depended upon *actual circumstances and real facts.* Dr. A. Combe considers that the Earl was unquestionably of unsound mind, because he had been long beset with unfounded suspicions of plots and conspiracies, unconnected ravings, sudden starts of fury, and a strange caprice of temper; it was proved that insanity was an hereditary disease in the family, that several of his relatives had suffered from madness, and at one time his nearest relations had actually debated on the expediency of taking out a commission of lunacy against him. Mr. Winslow does not concur in this view; because the quarrel which the Earl had with the deceased did not originate "in any morbid images which had fastened themselves on his imagination, but it was founded upon existing facts." (p. 27.)

It appears to us that Mr. Winslow is here guilty of some inconsistency. The very strong presumptions in favour of the existence of insanity in Lord Ferrers's case weigh as nothing in his judgment; the question of responsibility is here made to rest upon whether the act was or was not founded on existing facts. We do not exactly perceive how he can reconcile this opinion with the conclusion to which he comes respecting the lunatic who murdered his keeper, and who was acquitted because he happened to be the inmate of an asylum. "We know so little of the workings of the human mind, either in its healthy or morbid state, that it is a point of great difficulty, in fact almost an impossibility, to detect the line of demarcation between responsibility or irresponsibility, or where one commences and the other terminates." (p. 18.) We do not see why this rule should not be equally applied to the case of Earl Ferrers: the lunatic had the same motive—revenge, and the murder was premeditated; in short, the crime was "founded upon existing facts." The lunatic had been ill treated, and knew of no other way of punishing his keeper than by killing him. It is a question whether the violent resentment which Lord Ferrers had imbibed against his steward, without a sufficiently reasonable cause, was not in itself a strong proof of his mind being disordered. We cannot, therefore, understand how in the one case the crime is to be referred to passion and in the other to insanity. We consider this test of looking for "existing facts" as decidedly fallacious. Lunatics are certainly capable of committing crimes under the influence of what may be called sane motives, as for example from revenge or passionate excitement; the quarrel preceding the crime may not have originated in any morbid images, but have been founded upon existing facts; and if this is to be made the test of responsibility, many would be condemned to suffer whom the very advocates of the test would pronounce to be irresponsible for their acts. The recent decisions in the cases of Oxford and M'Naughten, contrasted with those of Lord Ferrers and Bellingham, attest the uncertainty thrown around the medical jurisprudence of insanity. In the two former cases the proofs of insanity in the general conduct of the parties were feeble, compared with those which existed in the latter; and it is impossible to come to any other conclusion than that if Oxford and M'Naughten were properly acquitted on the ground of insanity, Lord Ferrers and Bellingham were most unjustly convicted. We shall reserve any further remarks on this subject

until we have occasion to examine the evidence given on the trial of M'Naughten.

On the subject of moral insanity, we do not find anything that is new in Mr. Winslow's book: this part of the essay is mostly made up from cases and observations extracted from the works of well-known authors. The same may be said of the section on homicidal insanity; the author thinks that "in the majority of these cases the intellectual faculties, as contradistinguished from the moral perceptions and powers, give no evidence of disease," (p. 60,) although he is inclined to believe that these horrible destructive impulses will generally be found, when the case is properly examined, to be connected with some hallucination or perversion of the mental faculties.

The characters which are supposed to distinguish the homicidal monomaniac from the sane criminal are quoted from the work of Dr. Prichard. Upon these we have little further to remark. We admit that in homicidal monomania we commonly find the crime to have been preceded by certain peculiarities of conduct; sometimes by a total change of character in the perpetrator, and there have often been previous attempts at suicide. These supposed criteria have, however, been repeatedly and very properly rejected, when tendered as evidence of insanity in courts of law. They are of too vague a nature, and apply as much to cases of moral depravity as of actual insanity; in short, if these were admitted as proofs, they would serve as a convenient shelter from punishment for most criminals. The third point is absence of motive, upon which some remarks have already been made in our notice of Dr. Prichard's work. It appears to us that Mr. Winslow is more inclined to adopt the opinions of others than to examine them with the care which the importance of this question demands. He must be aware that if, before inquiring into the perpetration of a crime, we were to search for the motives, and rest the responsibility of an accused upon the accidental discovery of what we might deem reasonable motives, many most atrocious criminals would necessarily go unpunished. We have already adverted to the case of Courvoisier; but this is not a solitary instance of the extreme fallibility of such a criterion. In the atrocious murders and mutilations perpetrated by Greenacre and Good within the last few years, we have additional proofs of the utter futility of trusting to this test for homicidal monomania. No reasonable motive was ever discovered in either of these instances; and yet the parties were very properly made responsible for the crimes. We could here enumerate many similar cases; but we have elsewhere sufficiently stated our views of this subject. The absence or rather the non-discovery of a motive for a crime like murder, can only be admitted, in support of a plea of irresponsibility, when the existence of insanity is clearly established by other facts. Unfortunately, however, medical witnesses rely upon this as one of the strongest proofs of insanity; although cases are continually occurring which show that many atrocious offences are perpetrated without any *apparent* motive. It is not the office of a human tribunal to search into motives; this is a matter which lies between man and his Maker; and it is obvious that if this were considered a necessary preliminary proof on trials, and the accused were to be convicted or acquitted according to whether a motive for a crime was or was not discovered, it would be a complete sacrifice of

the safety of society to false metaphysical doctrines. Another character assigned as a ground of distinction is, that the murderer has generally accomplices in vice and crime. It must be apparent, however, that this is far from true; some of the most atrocious murders committed in modern times have been the acts of solitary individuals, who had neither accomplices nor any assignable inducements leading to the perpetration of the crimes. The cases of Courvoisier, Greenacre, Good, Bolam, and Cook, the murderer of Mr. Paas, sufficiently bear out this statement; and in the absence of motive, of accomplices, and the atrocious nature of the crimes themselves, these cases certainly fall under the usual definition of homicidal monomania. But the criminals were properly held responsible and were punished; a clear proof of the insufficiency of such arbitrary rules, to enable us to distinguish between the homicidal monomaniac and the sane murderer.

There is one other criterion which carries with it some force: "The subsequent conduct of the unfortunate individual is generally characteristic of his state. He seeks no escape or flight, delivers himself up to justice, acknowledges the crime laid to his charge, describes the state of mind which led to its perpetration; or he remains stupefied and overcome by a horrible consciousness of having been the agent in an atrocious deed." From an examination of many cases of homicidal monomania, we are bound to state that this is a character which is almost uniformly met with. The monomaniac commits the crime openly, and when charged with it, he neither attempts to deny or to conceal it. By the sane criminal, however, every attempt is made to conceal all traces of the crime, and he denies it to the last: examples of which are furnished to us in the cases of Courvoisier, Greenacre, Bolam, and others. But let us not be misunderstood; we would not say that this should be made a test of homicidal monomania. Persons who destroy the lives of others through revenge or anger, often perpetrate murder openly, and do not attempt to conceal the crime when they know that concealment is hopeless. Besides, a morbid love of notoriety will often induce sane criminals to attempt to commit assassination under circumstances where the attempt must be witnessed by hundreds, and there can be no possibility of escape. The English law has, however, more than once rejected this as a test; and proceeded to punish the individual in the same way as if he had attempted secret assassination, and had endeavoured to conceal his attempt afterwards. We may remind our readers that the attempt made by Francis on the life of the sovereign was followed by conviction and punishment, although it was publicly made and the assassin did not attempt to escape. Indeed, it is obvious that the notoriety attending the offence would be the only conceivable inducement to its perpetration. What possible motive, we would ask, could ever exist in the mind of a subject for attempting to assassinate the sovereign of this country? And if, according to Dr. Prichard and Mr. Winslow, we are to take the absence of motive, the want of accomplices, and the fact of a man attempting assassination openly, and using no effort to escape, as tests of homicidal monomania, and sufficient for a plea of irresponsibility, it is clear that under no circumstances should the assassination of the sovereign be held to be a crime. It would be better at once to declare that this detestable act of treason should be deemed sufficient evidence of in-

sanity,—that the perpetrator should be regarded as "an unfortunate" person, and exempted from all responsibility for his act. We do not say that these authors would adopt this extreme view, but their arguments lead directly to its adoption. It is sufficient for us to state, that this revolting doctrine has been negatived by the law in the case of Francis; and we believe that in this case the verdict met with the full concurrence of society. Notwithstanding the evidence of peculiarity of conduct in the accused, of the act being without motive, of its being perpetrated openly, of the individual seeking no escape or flight, but delivering himself up to justice, of there being no accomplices in vice or crime, the law has, and we think justly, pronounced that these facts did not exempt him from responsibility for the act. It therefore appears to us, that these alleged criteria of homicidal monomania must either be abandoned as insufficient and dangerous of application, or those who advocate them must be prepared to say, that atrocious crimes involving the safety and well-being of society may be perpetrated with comparative impunity.

It may be said:—if these are not to be received as characters of homicidal monomania, what other distinctions can be drawn? We reply that if we are not yet in a condition to draw a line of demarcation between responsibility and irresponsibility, it is something to have shown that the rules at present advocated by certain authors are insufficient, and liable to become dangerous in their unrestricted application.

With some of Mr. Winslow's sentiments we fully agree. Thus when he says, "There is no doubt of the occurrence of this form of insanity (homicidal monomania), and when its presence is clearly established the person so unhappily afflicted ought not to be considered as a responsible agent," (p. 67,) he states no more than would be immediately assented to by most persons who have reflected on this important subject. We do not, however, find that he has furnished us with any tests by which its presence can be *clearly* established. He has relied too much upon the vague rules given by others, without sufficiently examining their tendency or actual value.

Again; he justly complains of the test applied by the law to these cases, namely, whether the person committing the crime could at the time distinguish between right and wrong, good and evil—terms which, as we have elsewhere said, only tend to confuse the minds of juries. He says, "A person may be perfectly competent to draw a correct distinction between right and wrong, and yet labour under a form of insanity, which ought unquestionably to protect him from legal or moral responsibility." (p. 74.) This is undoubtedly true; and we think that it is fully borne out by the cases of Martin, Hadfield, and Greensmith. (No. XIX. p. 144.) The knowledge of the legality or illegality of an act at the time it is perpetrated is not always a safe criterion; and the only apology that can be offered for its adoption is this: that it is perhaps the sole criterion by which the conduct of a criminal can be tested before legal tribunals, as they are at present constituted, and that the test is never insisted upon by our law authorities where other facts plainly tend to show the insanity of the accused. Thus, then, it is not true that the test is rigidly applied or that "the unfortunate maniac has no chance of escaping." The cases of Martin, Hadfield, and others plainly refute

this notion. The author must be aware that no direct rule of criminal law can be so framed as to meet all cases of supposed insanity; and that with an erroneous test substantial justice may be done, if the rule be relaxed in those instances where other circumstances would clearly render its application harsh. It is an essential feature of the *sane* criminal that he always shows by his conduct a knowledge of the *illegality* of the act which he is perpetrating; it is this which leads him to the adoption of secret and tortuous means of destruction, and also to the subsequent concealment of the crime. Thus, then, this rule, although it falls short of what may be required, is perhaps as well fitted as any other to guide the uninstructed minds of a jury.

The following extract is replete with good sense, and we here quote it, as it conveys an important caution to medical witnesses:

"He (a medical witness) should, when called upon to give evidence in cases of insanity, never forget that he has nothing to do with the legal definition of the term. No medical man is competent in every case to say whether the party supposed to be deranged is or is not competent to draw a line of distinction between good and evil, right and wrong. The legitimate point which the medical witness has to decide is this: Had the alleged lunatic, at the time he committed the offence, sufficient control over his actions? It is absurd to believe that any amount of medical information or metaphysical knowledge, which the witness may be in possession of, will enable him to form anything like an accurate notion of the lunatic's capability of distinguishing between right and wrong. Above all things, the medical man should avoid defining insanity. Counsel knowing the obscurity of the subject, and conscious of the difficulties with which medical men have to contend in arriving at a correct opinion, most unfairly, in their examinations, endeavour to tie them down to definitions; and then by showing their fallacy weaken the whole effect of their testimony. There is nothing so easily seized upon as a definition, therefore the witness should be cautious in committing himself by attempting to define insanity. It will be better and wiser for him at once to acknowledge his incapacity to do so, than by a vain and ostentatious display of metaphysical lore to peril the life of a fellow-creature." (p. 76.)

In admitting the justice of these remarks, it must not be forgotten that there are two sides to this question; and that as yet more "metaphysical lore" has been displayed in endeavouring to prove criminals insane than in establishing sanity on the part of criminal lunatics. It is only fair to the author to state, that he entirely disconnects himself from those who think that the presence of a disordered mind ought invariably to shield a person from responsibility. The degree and character of the mental derangement should be looked to; the existence of waywardness of character, idle fancies, and absurd delusions, should not of themselves screen a person from the penalty awarded by the laws for a criminal offence. The doctrine that such persons should be exonerated from all responsibility is, in his view, as unphilosophical as it is opposed to the safety of society.

In dismissing Mr. Winslow's book, we have to remark, that in our judgment the subject might in a future edition be better arranged. The treatise would advantageously admit of a division into chapters, and some parts of the subject require to be more fully treated. In other respects, there is much in the work to suggest matter for deep reflection to those engaged in inquiries of this nature.

III. We have now to examine the work of Mr. Sampson, in which we find an entirely new view taken of the medical jurisprudence of insanity. The great object of the author in this treatise appears to be to establish three points: 1, that the test of insanity is obedience to the laws; 2, that the commission of crime is ipso facto a proof of insanity; 3, that capital punishment, whether for treason or murder, is barbarous, inexpedient, and unjust. With regard to the first point the author remarks:

"Although it cannot be maintained that there exists any human mind in a state of perfection, yet we may consider, as perfect for all social purposes, that mind which comes up to the average state (?) of mental power characterizing the society of which it is a part. This average state of the social mind is precisely indicated by the laws and institutions which society frames or permits to be framed for its own governance; and hence it may be very safely taken as a rule, that every person is sane to the requisite extent for the performance of social duties, so long as he possesses the mental power and disposition to act in obedience to the laws." (p. 6.)

This is what we presume may be called social sanity. As the laws of countries differ widely from each other, what is sanity in one may, upon Mr. Sampson's doctrine, be insanity in another. But there are two classes of persons for which this singular definition of sanity does not provide; namely, 1st, those who infringe the laws from ignorance, and who can hardly, therefore, be pronounced insane; and 2d, those who having been born and bred under one system of laws, find themselves in after life in another country, where the customs, of which they may be utterly ignorant, are different. It may be said obedience to the law implies a knowledge of it; we reply that such a doctrine is practically disregarded: if ignorance of the law were a sufficient answer to any criminal charge, few convictions would take place. In some eastern countries, murder is scarcely regarded as a crime; but the English law would not allow an inhabitant of these countries, who had committed murder in England, to escape, merely because he had just arrived and was ignorant of the fact, that what he had been taught to look upon as a venial offence in one place was a capital crime in another. For this reason, we do not think the rule proposed by Mr. Sampson either safe for the individual or for the society of which he is a member. If it be urged that a man might possess the mental power and disposition to act in obedience to the laws, provided he knew them, the definition appears to us to be frittered away. His sanity would then be a continually fluctuating quantity, varying with the care and attention which he had bestowed upon the subject of legislation, and with the kind of society of which, for the time, he formed a part.

The second proposition appears before us in the following form:

"We are bound, when a person has committed or attempted to commit a crime, to receive that fact as a sufficient evidence that his brain is in an unsound state, the degree of his unsoundness being indicated by the extent of his offence. The recognition of this fact necessarily leads us to the conclusion that the infliction of punishment in any case whatever is wholly inconsistent with all ideas of justice." (p. 24.)

Thus, then, there is one degree of insanity for larceny, a second for rape, a third for murder, and a fourth for treason. In every case the

individual perpetrating the crime is to be regarded with pity, as an unfortunate sufferer:

"It would be quite as rational," says our author, "to flog a man at the cart's tail for having become infected with the scarlet-fever, owing to a predisposition and exposure to the disease, as to pursue the same course to one who, falling into temptation, had given way to a predisposition for taking possession of whatever he could lay his hands upon. To be sure, it might be said that the flogging could not operate so as to deter the man from catching another fever, while it might deter the thief from repeating his offence; but this distinction will not hold good, because, in the first instance, dread of the punishment might possibly induce the patient to attend in future so closely to the laws of health as to keep him safe from infection, (!) and it could do no more in the latter case with regard to the laws of morality." (p. 35.)

To the very obvious objection that in one case the mischief done affects only the person, while in the other it affects the security of society, the author replies by ingenious sophistical arguments. Thus, it is said, "society is liable to suffer even more seriously by the impairment of the bodily health of one of its members than from the results of the moral depravity of another." (p. 35.) The "moral offender" may, it is true, commit one or more murders, but this is balanced by the physical sufferer not being able to devote his energies to the good of society (!); and in addition to this, society may suffer far more seriously by the "man of ruined constitution transmitting to another generation his own delicate and enfeebled powers." (Ib.)

The author then proceeds to reason himself into the belief, that the infliction of punishment for a crime, which he assumes to be always indicative of disorder of the brain, is no more reconcilable to our ideas of justice, than the infliction of punishment on a man because he is labouring under inflammation of the liver or lungs. We are not surprised that he should himself be somewhat startled at the conclusions to which these singular doctrines lead. He admits, as one objection, that they would tend to destroy all ideas of responsibility; in which we perfectly agree with him. It is true, he attempts to show that this objection is without any solid foundation; but we are by no means satisfied with the explanation which he offers; it is a tissue of ingenious special pleading, and nothing more. All persons, he says, are responsible for the acts which they commit; but this responsibility consists in making them undergo "the painful but benevolent treatment requisite for their cure."

Thus a man vicious and depraved may walk about the streets of this metropolis, and so long as he commits no offence, he receives no pity for his situation; he is even allowed to perish from want. If he commits murder, rape, or arson, and more especially if the crime be accompanied by any features of peculiar atrocity, he at once becomes, on Mr. Sampson's principles, a proper object of benevolent treatment: he may have committed a cold-blooded murder to possess himself of valuable property, rape from sheer licentiousness, and arson from revenge; still he is an object of pity: he could no more help perpetrating these crimes than he could avoid an attack of inflammation of the liver. Most medico-legal writers have thought that the existence of motives for crime should be at least looked for, and that there was a distinction to be drawn between cases where the murder was committed for gain and the crime concealed, and other cases where the killing took place under a fit of delirium or

mania; but Mr. Sampson does not trouble himself with any such distinctions. All crime proceeds from insanity. Insanity and moral depravity are synonymous terms; thus, then, on this principle, one of two inferences must follow, either that all persons guilty of criminal offences should be punished, whether or not they be insane, in the common acceptation of the term, or that no punishment should be inflicted on a person for the perpetration of any crime whatever. "*Punishment from man is not necessary*." (p. 55.)

Many eminent judges, among others Lord Hale, have argued that all crime was the offspring of partial insanity; but no one before Mr. Sampson has ever attempted to draw from this doctrine the inference that every criminal should be exonerated on that ground. There is no doubt that in most criminals there is a degree of moral depravity which prevents them from reasoning or acting like men of a healthy moral standard; but experience has long ago settled that a punishment proportioned to the offence has had considerable influence in checking the conduct of these depraved characters; and whatever ingenuity may be displayed in building up plausible psychological theories, there is a general feeling in society that the dread of punishment has considerable influence in restraining these wicked propensities of the vicious. Mr. Sampson may not agree with this view; but the common sense of mankind, and the most common experience of nations, are against him. The truth appears to us to be, that not finding himself able to draw a clear distinction between insanity and crime, he has thought it better to take up an extreme view at once: it is thus we find him involving himself in numerous inconsistencies.

We do not feel ourselves called upon to enter at length into an examination of the author's views with respect to punishment. The mere infliction of punishment is, in his view, an act of inherent and barbarous injustice; and he appears to regard capital punishment in the light of judicial murder, however atrocious the crime for which it may be inflicted. The substitute which he would propose is restraint, perpetual in the case of murder; because, although the homicidal tendency may be apparently removed, we never can be certain that the impulse may not again arise under the sudden influence of external excitement. During this time the individual should undergo treatment to remove the moral disorder, so to speak, under which he is labouring. "In lesser crimes, the same necessity for *perpetual* restraint does not exist; and therefore the period of the incarceration of the criminal should be contingent entirely upon his own improvement, and certainly need rarely be so prolonged as to terminate only with his life." The shallowness of this mode of reasoning scarcely requires to be exposed. The same motive which would justify perpetual imprisonment in cases of murder, would justify it in cases of rape, burglary, arson, and highway robbery; for how are we ever to predicate that the impulse to these crimes, destructive as they are to the peace and well-being of society, may not again arise "under the sudden influence of external excitement?" So of theft, or of any other crime; in short, it appears to us that this doctrine, carried to its legitimate extent, would condemn all offenders against the law, whether the traitor, the murderer, or the petty thief, to perpetual imprisonment. We have been taught to look upon this as a severe punishment, and most persons

would so regard it: some have held perpetual imprisonment to be a more severe punishment than death itself; and yet, while Mr. Sampson advocates it openly for all cases of murder, and impliedly for other crimes, he contends that the infliction of punishment is an act of inherent and barbarous injustice. His reasoning stands thus: Punishment from man is not necessary. Perpetual imprisonment is necessary; ergo, perpetual imprisonment is no punishment.

Among the fallacious arguments adduced in support of the abolition of the punishment of death, we find it urged that capital punishment tends to keep up a blood-thirsty propensity among nations and individuals. The author says that in all countries where capital punishments are rare, the tendencies of the people are always proportionably humane. We deny this; and appeal to Italy and Spain, if he has any personal knowledge of those countries, as affording examples directly contradicting his statement. It is asserted that out of 167 persons executed during a certain period, 164 had been present at executions. The inference is that public executions tend to increase the number of murders. (p. 105.) Here we have another example of the saying that figures may be made to prove anything. Criminals are commonly found among the most depraved; and it is of the depraved members of society of which these crowds at public executions are chiefly composed; but the inference drawn is wholly false; for where probably one present at an execution subsequently commits murder, two hundred may not: hence, to judge of the effect of executions in the way suggested, we should know what proportion of the whole numbers present the 164 executed criminals formed. By this convenient application of figures, the author might equally prove that horse-races and fairs had an equal tendency to increase the crime of murder; for a large proportion of those who are executed would probably be found to be regular attendants at such places.

Then we have the old argument of the inefficacy of these punishments as examples, because persons who have witnessed them have soon afterwards committed murder. A little reflection will prove that this is an argument against any punishment at all. Persons just discharged from prison have been known to be apprehended within an hour for a repetition of the offence. The truth is, with some persons punishment of whatever kind entirely fails as an example; but there is good reason to believe that it influences the majority: so that solitary examples of this kind prove nothing.

The author has taken up the extraordinary notion that the punishment of death increases the number of murders, because some who wish to die commit murder in order to be put to death by the public executioner; and he does not hesitate to express his belief, "that a remarkable diminution in the number of murders by which our country is annually disgraced, would be the immediate consequence of its abolition." (p. 98.) Admitting, as we do, the fact upon which this assumption is built, we must remind Mr. Sampson, that many persons annually steal or commit other crimes in order that they may be transported; therefore, it may be argued, transportation encourages theft. So, again, offenders are continually brought before our police-courts who admit that they have broken lamps, windows, and committed other wilful damage to property, in order that they might be sent to prison; therefore, the abolition of

prisons would be advisable; since there might be a great diminution in the number of offences of this description. Notwithstanding these well-known facts, it is considered that transportation and imprisonment tend to check crime; and if capital punishment does not check the crime of murder in a like degree, it is not for the reasons assigned by Mr. Sampson.

All right-thinking men agree that capital punishment was too frequently resorted to formerly, and wholly failed in its effect as example; but it is a general opinion, except with theorists like Mr. Sampson, that it would be unwise to abolish it in cases of murder. The laws of all civilized countries sanction it; it is considered to be the best security which can be afforded to society, and one that must be occasionally resorted to. Even those who, like the author, have recommended the substitution of perpetual imprisonment, have not said how a criminal is to be dealt with who refuses to submit to this punishment, who murders his keeper, who escapes from prison, and commits other murders or atrocious crimes upon innocent members of society. Mr. Sampson might consider such a man, although not a lunatic in the general acceptation of the term, as an object for increased benevolent treatment; but we apprehend that society would consider itself fully justified in putting an end to the career of a monster of this description, just as a man would in defending his life against the attack of an assassin.

Our readers will judge from the preceding remarks what our opinion is of this book. The doctrines are as pernicious as the reasoning upon which they are based is fallacious. The author blames one half of mankind for the immorality or depravity of the other; and while looking forward to a "Utopia," in which there will be no tendency to the perpetration of crime, he allows that the innocent members of society are to be openly the subjects of assassination and robbery, with no other punishment for the criminals than restraint and benevolent treatment. We can scarcely bring ourselves to believe that any person should in the present day seriously propound such wild and reckless views.

IV. We shall here briefly advert to the Commentary on Insanity by Sir A. Crichton, because it bears strongly on the subject which we have been discussing, and furnishes an answer to the crude doctrines of criminal responsibility which we have considered it our duty to expose. The definition and treatment of insanity require no remark. The author objects to Dr. Prichard's definition of moral insanity, as comprehending "a number of diseased affections as well as varieties of moral character which have no similitude to each other, or even to true insanity." (p. 186.) This is undoubtedly correct; Dr. Prichard must himself perceive that his "moral insanity" may be easily made to include all cases of moral depravity; for we have elsewhere shown that the distinctions which he has attempted to draw between the two classes of cases are artificial and often inapplicable.

Sir A. Crichton, like all other medico-legal writers, denies that crime is necessarily indicative of insanity, or that every criminal is insane. Perhaps he carries his views on this point a little too far, and is inclined to look upon many persons, who would probably be acquitted on the

ground of insanity, as being responsible for their acts. Nevertheless, we highly approve the manner in which he discusses the subject.

"In our fallen state, evil propensities and bad passions are the inheritance of every descendant of Eve; but experience teaches us that there is not a propensity or passion to which we are subject which may not be weakened and rendered even inoperative by accustoming an individual to associate with its first impulse or suggestions emotions of a contrary nature. Hope and fear are the great engines. It is on this principle that moral education depends. It is in this way alone that vicious propensities in youth can be corrected. But it is too much to say, that because many unfortunate individuals have never been accustomed by due instruction and discipline to exercise their reason and to associate in their minds the dread of future or temporal punishment with evil propensities and sinful acts, therefore they ought to be considered as irresponsible agents and lunatics." (p. 190.)

The unsettled state of opinion which at present exists among medical men on the subject of insanity is, in his opinion, the source of two evils,—that some criminals who ought to be punished are acquitted of responsibility, while, in other instances, true homicidal monomaniacs are convicted and executed. It is difficult to say of which evil society has now the greater reason to complain; but it is a matter of sad reflection that the result depends less on the facts proved than on the medical and legal ability and ingenuity engaged in the prosecution or defence. The more conspicuous the crime, the greater are the exertions made, and the result is commonly successful: on the other hand, trials frequently take place at our assizes for offences, affording primâ facie evidence of insanity, in which the accused is either undefended or left to the chance defence of some counsel in court, and the result is a conviction.

Sir A. Crichton differs from Dr. Prichard in thinking that there is often a power of control on the part of a criminal, who might be laboring under what is termed moral insanity; and this, as our author properly suggests, and as we have elsewhere urged, should be the real test of responsibility. A man is caught in the act of attempting murder,—he finds his antagonist better armed than himself,—he attempts to kill him, falls on his knees imploring mercy, or endeavours to escape by running away. "Such a variety of motives, from which he chooses one according to circumstances, show evidently that his conduct is guided by understanding, though at the time he is under violent excitement." (p. 194.) This adaptation of mean to ends is certainly a strong proof of a reasoning power, and consequently of a power of self-control,—its existence may be frequently inferred from the manner in which the crime is perpetrated.

Medical men have, on these occasions, passed often beyond the legitimate sphere of their duties. It is not for a medical witness, as such, to discuss the subject of free-will, human responsibility, or the degree of punishment which should follow an offence. By so doing he usurps the functions of the judge and jury, and takes upon himself to dictate to society the means which it should employ for the protection of its members. In these cases, his professional studies give him no advantage over the common sense and experience of mankind,—a fact made evident by the very different opinions entertained on these subjects among equally eminent members of the profession. A medical witness has only to state to what degree, in his opinion, the mind of a person accused of crime devi-

ates from the ordinary standard; while, from the evidence given on some recent occasions, it would rather appear that the accused was tried by the medical witnesses than by the court before which he was arraigned. On this point Sir A. Crichton very justly observes:

"If revealed truth, which declares that we are to answer for the deeds we commit in the flesh, is discredited, it is not by metaphysical arguments that we can hope to settle our faith; but this every one must needs know with certainty that in this country, and in every other civilized state which is careful of the interests of society, the laws are founded on the doctrine of the freedom of the will, as it is called, or on human responsibility; and also on the belief, that examples of punishment and the dread of future condemnation will influence the determinations and actions of men until they are bereft of reason by real disease. To the judge must be left the task of considering the circumstances which palliate offences, and make criminals objects of special commiseration and mercy; but the attempt on the part of learned doctors in law and medicine to confound vice with insanity, and, consequently, to condemn the right of human punishment, I consider as one of the many dangerous innovations which the proud philosophy of the nineteenth century has produced." (p. 195.)

IV. The last three pamphlets on our list refer to the late trial of M'Naughten for the murder of Mr. Drummond. The first of these is a non-professional report of the trial, with the whole of the legal arguments and medical evidence, unaccompanied by comments. It should be in the possession of all who feel an interest in the medical jurisprudence of insanity; for probably there is no case in modern times which has excited so much attention, in and out of the profession, as this.

We consider it unnecessary to detail the facts of this case: they are so recent that they must be familiar to the whole of our readers. We shall here offer only a few remarks on the defence: this was to the effect that at the time the accused perpetrated the act he was laboring under homicidal monomania. It was deposed to by many witnesses that the prisoner was latterly of a sullen and reserved character; that he imagined himself to be the object and victim of the most unrelenting persecution; that he was surrounded by persons who had formed a conspiracy against his comforts, his character, and even his life; and that wheresoever he went these persons still pursued him, and gave him no rest either by day or by night. It also appeared that he imagined the deceased, who was a perfect stranger to him, to be one of his persecutors, and that it was necessary he should fall a sacrifice in order to free him from persecution. There was no proof of intellectual insanity about him, if we except the existence of these delusions; and it was admitted by all that he was shrewd in business transactions, that he was fully competent to the management of his affairs, and had realized a considerable sum of money by his own industry in trade.

These were the principal points in the defence; the remainder of the evidence in favour of insanity being made up by the opinions expressed by the medical witnesses. We will now compare this evidence with those characters which have been assigned by Prichard and others as proofs of homicidal monomania. We have already expressed our opinion that these characters are loose and vague; but the counsel for the defence chiefly based his arguments in favour of the prisoner's insanity upon them. There had been certain peculiarities of conduct and absurd delusions;

the man was of a morose and reserved disposition, but we do not collect from the evidence that he had ever attempted suicide. It does not appear that he was ferocious or cruel: his counsel dwelt much on his humanity to the brute creation, a fact which, in his view, did not accord with the ferocity of a sane assassin. (p. 46.) In this we have a good example of legal ingenuity; for one strong feature of moral insanity, leading to homicidal madness, is cruelty or ferocity of disposition; so that some physicians have given to it the name of brutality. Thus it will be seen that the very reverse of the usual condition was received without comment as a proof of the existence of homicidal madness. In the language of Mr. Rumball, there was no indication of diseased destructiveness about him. We put no great stress upon this, one way or the other, or upon the absence of the suicidal tendency before and after the crime; but it shows that the proofs of homicidal insanity are of so ambiguous a character that a barrister may select either of two opposite conditions in favour of his argument.

Next we come to *motive*. There was no apparent motive on the part of the prisoner in shooting Mr. Drummond; but we think we have said sufficient to show that this should not be received as evidence of insanity; it merely makes out a primâ facie case for inquiry. Further, it was argued by the counsel for the defence:

> "The manner in which the murderer sets himself to the consummation of his crime, as well as his subsequent conduct, is very different from the proceedings of a madman. The former often has accomplices; he commences with premeditation, lays a plan beforehand, chooses time, place, and circumstances adapted to the perpetration of the deed, and generally has contrived some method of escape. He always studies concealment and personal safety, and when there is danger of detection, uses all possible dispatch to escape the punishment due to his crime. All these particulars are reversed in the proceedings of the madman. .... A common murderer would have acted in a different manner, he would have chosen a different time, a different place, he would have sought safety by escape." (p. 58.)

It is with something like dread that we witness these displays of forensic eloquence and ingenuity in questions of criminal responsibility. While, on the one hand, they may lead to the acquittal of one who is responsible; on the other, they may bring about the condemnation of an irresponsible agent: this is a pure matter of accident, depending on the fact of whether the ability be displayed on the side of the prosecution or defence. In the quotations which we have above made from Mr. Cockburn's speech for the prisoner, we have what appears to be well-marked points of difference laid down; although he had just before adduced and commented in favour of his views upon cases which completely overturn the differences thus sought to be established! Thus Hadfield's case is quoted among the instances of homicidal insanity; but probably there never was an attempt made upon life, in which there was greater premeditation, precaution, or a better choice of time, place, and circumstances, than in the attack made by this monomaniac on the life of George the Third! We also think it is obvious that if a man, whether sane or insane, have the design to shoot the sovereign, a minister of state, or any great public character, he can seldom have an opportunity of doing this except in public, and therefore under circumstances in which any at-

tempt at escape would be commonly futile. With respect to accomplices, it is true that we never find them in cases of homicidal monomania but they are also generally wanting in crimes of peculiar atrocity and magnitude. The cases of Greenacre, Good, Courvoisier, and others, afford a sufficient proof of this fact. M'Naughten made no attempt to escape, or to deny that he had shot the deceased: this, as we have already observed, is a pretty uniform character of homicidal monomania; although a question might have arisen as to what he would have done, supposing he had not been seized in the act of discharging a second pistol. Still, however, allowing the prisoner the full benefit of this point in his favour, we must protest against this being drawn, as it was in this instance, into an absolute proof of homicidal monomania.

Mr. Cockburn adopts Lord Erskine's opinion of the case of Earl Ferrers, and considers, in opposition to the able commentary of Dr. A. Combe, that this nobleman was properly convicted of murder; although the evidence in favour of previous insanity was undoubtedly much stronger in this than in M'Naughten's case. The Earl did not attempt to deny that he had shot the deceased, and exclaimed when the act was done, "I am glad I have done it, he was a villain, and I am revenged." Yet, by the exercise of legal ingenuity, the same facts are in one case adduced as strong proofs of insanity and in another of perfect sanity. We have no doubt that had Lord Ferrers been as ably defended as some modern monomaniacs, he would have been acquitted on the ground of insanity; and if we compare the verdict in his case with later decisions, his execution can be regarded in no other light than as a judicial murder. In the mean time, the facts in Lord Ferrers's case are made to serve two purposes: they furnish proofs in favour of responsibility or irresponsibility, according to the option of the advocate.

It is well known, that when murder is committed through the motives of passion or revenge, whether apparent or concealed, there is frequently no attempt made to conceal or deny the crime, there is no attempt at escape, and yet such persons are made responsible for their acts.

The only other point in the legal defence upon which we have to offer a remark is this, that the counsel adopted Lord Erskine's doctrine, i. e. in order that there should be irresponsibility, two facts must be proved: 1, that there should be delusion; 2, that the act of homicide should be connected with the delusion. Admitting that the first point was proved in M'Naughten's case, we do not see how in any part of the defence the delusion was brought to bear upon the deceased as an individual. It was urged that the prisoner considered him to be one of his persecutors; but he was a perfect stranger to him, and therefore the prisoner might as well have shot any other person in the queen's dominions; for it was of course a matter of accident as to who might appear to him to be his persecutors. From the decision in M'Naughten's case, then, we infer that there may be the most broad and unrestricted application of this principle relative to the connexion of the act of murder with the delusion.

Eight medical witnesses gave evidence on the prisoner's state of mind. They all agreed that at the time he committed the act he was labouring under a delusion, and that he was led on by an impulse so irresistible, that nothing but a physical impediment could have prevented him from committing it. (Dr. Hutchenson.) It is remarkable that questions were

allowed to be put to the witnesses on this trial, which have seldom been permitted on former occasions. Thus some were asked whether they considered the prisoner responsible for his actions—a question which has been hitherto left to the jury to decide from the medical and other evidence adduced in the case. There are strong objections to this mode of examination, for it is like placing the issue of guilty or not guilty in the hands of the medical witnesses; and we attribute to this, the great public dissatisfaction expressed at the verdict in M'Naughten's case. So long as a prisoner, or those who act for him, are allowed to select the medical witnesses, who are to speak to his state of mind, we think it would be at least prudent not to permit questions to be put in this form. If the witnesses are really independent, and they might easily be made so for this purpose, there could be no objection to the prisoner having the benefit of their opinion under these circumstances. But it is obvious, that the counsel for a prisoner would never summon any witness who could not speak in his favour; and if what this witness is to deliver under the name of evidence substantially includes the verdict of the jury, we do not see why the prisoner and his friends should not be at once allowed to select their own jury. Let us suppose in a case that twenty medical witnesses are appealed to, and while one half agree that the case was one of insanity, the other half do not; it is very clear that only the ten first witnesses would be called for the defence; and, unless an equal amount of industry were displayed on the part of the prosecution, the verdict must be carried in the prisoner's favour. It will be impossible, we think, to eradicate the suspicion of unfair dealing on these occasions so long as this bad practice is adhered to. The result concerns the country, and if medical witnesses are in any case to assume the functions of the jury, their selection should lie with the country and not with the prisoner. In this case the evidence was felt to be so conclusive in favour of the existence of homicidal monomania, that the jury under the direction of the judge acquitted the prisoner on the ground of insanity.

The case of M'Naughten has called forth many comments, a strong impression having existed, both in and out of the profession, that the plea of insanity had been stretched to an improper extent. On this point most men will of course form their opinions according to their reading and experience on the subject; nor will these opinions be much influenced by what is said by journalists and reviewers. In accepting the verdict of the jury as the only verdict which the medical evidence would warrant, we are bound to state that in our judgment there is no case on record, if we except that of Oxford, where the facts in support of the plea of insanity were so slight; and when we reflect upon the cases of Bellingham, Lees, and Cooper, the last two tried at the same bar and executed within the last few years, we feel that there is both uncertainty and injustice in the operation of our criminal law. Either some individuals are improperly acquitted on the plea of insanity, or others are most unjustly executed.

This state of things requires to be remedied: it is wholly inconsistent with our views of justice, that the acquittal or conviction of supposed lunatics on capital charges should depend not on the merits of their respective cases, but on the ability and ingenuity of counsel, and the metaphysical speculations of medical witnesses upon the question of

criminal responsibility. One case becomes a subject of prominent public interest, and every exertion is made to construe the most trivial points into evidence of insanity; an acquittal follows. Another case is left to itself; and, as the line of demarcation between sanity and insanity is scarcely appreciable without good legal and professional assistance, the accused is necessarily convicted, and either executed or otherwise punished; although the proofs of insanity, had they been as carefully sought for and brought out, would have been as strong in this as in the former instance. In point of fact, there is no more stability in judicial decisions than there is in medical evidence; and we think that one great improvement in the present system would be to leave the question as to the state of the mind only to the medical witnesses; and that of responsibility and punishment for the act, to the judge and jury.

M'Naughten's case has certainly proved that there is a very narrow line which separates crime from insanity; and the expressed intention on the part of certain members of the legislature to bring forward some preventive enactment shows that there is at least a well-founded dread that after the result of this trial, the plea of insanity may be carried too far. The verdict has completely overturned the old doctrine of implied malice in law; for, after this, how can a person be convicted of murder for killing one who is wholly unknown to him? If a man wilfully, and without provocation, fired a gun into the midst of a crowd and killed a person, this was formerly held to be murder, since the act was considered to imply malice against all mankind; but it might now be fairly contended, the individual was not responsible; "he had no accomplices, he had no motive for the act, he did not watch his opportunity and kill the person secretly, he laid no plan for his escape, and did not attempt to escape after perpetrating the crime; he made no denial of the crime, but calmly resigned himself to his fate." This is the substance of Mr. Cockburn's defence in M'Naughten's case; and, taking this as a precedent, such a defence would probably lead to an acquittal under the circumstances above supposed, on the ground of insanity. It might be contended that an act so committed would of itself always indicate insanity; we, however, would say that the act might sometimes depend on moral depravity, and that then the perpetrator should be dealt with differently to one who killed another in a fit of delirium, or during a paroxysm of mania.

V. Mr. Rumball's pamphlet is chiefly devoted to an examination of the case of M'Naughten. He says of it, "There is not a man in the country who does not feel that the late decision was a legal but not an equitable one,—that a foul murder has been done and justice is unsatisfied." (p. 11.) The author's views of insanity are strictly phrenological, and in our opinion are much more sound than those advocated by Mr. Sampson. He considers that "So long as a man can subject his delusions to the correction of his own understanding, he is not mad, but he is mad the moment the delusions refuse to be controlled. So long as his feelings, however intense, can be antagonized and conquered by other feelings, whether fear or affection, so long is he sane." (p. 13.) His opinion, therefore, is, that "Insanity is the excitement of any of the mental faculties beyond the control of the remainder." (p. 15.)

Mr. Rumball occupies some space on the question of the expediency of

capital punishment in any case. He admits that, although the putting another to death is inherent as a right, it is not expedient; and he considers that if the punishment of death were abolished and imprisonment substituted, juries would not hesitate to convict on these occasions, and judges to sentence. We believe that this is the best argument which can be adduced for the abrogation of capital punishment, and that in many cases the plea of homicidal insanity would not have been raised, but for the fact that conviction would probably be followed by death. It appears to us, however, that Mr. Rumball loses himself in the argument, for those who are acquitted on the ground of insanity undergo the very punishment which he would substitute for death, namely, perpetual imprisonment: therefore we are positively at a loss to know, why he should complain of the acquittals of Oxford and M'Naughten. In respect to the latter, he says, "the assassin is suffered to escape," (p. 25;) but this is not true; the man is as much punished as if the author's own alteration of the law had taken place, and the judge had sentenced him to imprisonment. It is true that he is not imprisoned for having shot Mr. Drummond, but to prevent him from shooting others. Again, we find him asking the question, "Was Oxford labouring under a delusion terminating in homicidal climax? did he ever pretend to be so? was he or was he not perfectly aware, that wrongfully to kill was murder?" (p. 28.) We certainly never could comprehend why Oxford should have been acquitted and Francis convicted for an offence perpetrated under such similar circumstances; but we do not see that Mr. Rumball has any reason to complain, when Oxford is undergoing the punishment which he himself recommends for infliction in all similar cases.

We must here take leave to remark, that medical men venture entirely out of their province when they discuss the subject of punishments. The great object of all punishment is to prevent crime, not merely to put it out of the power of the same individual to repeat his offence against the laws, but to prevent other reckless and depraved persons, of whom there are numbers in society, from imitating him. This question, therefore, falls more within the department of the statesman and legislator than of the physician or surgeon. The studies or duties of the latter cannot possibly fit them for deciding on the effect of punishment.

There are certain atrocious crimes which it is considered should be punished with death, as this is the only punishment which our legislature holds to be fitted for their repression. Whether it answers this purpose or not cannot be determined by an individual who has not exclusively devoted himself to the study of the subject for many years, and who has not had ample opportunity of determining the general effects of punishment on mankind. We can hardly say that homicidal monomaniacs are acquitted: they are imprisoned for life, and the great discussion which has arisen on some recent decisions is really whether this punishment, for it must be so considered, will have any effect in deterring others from imitating the crime.

Perhaps, as Mr. Rumball suggests, it may have this effect much better than if they were executed as criminals. Upon that point we shall offer no opinion, since it can only be settled by experience, and we should decline debating a question of such importance with one whose views must be founded on mere theory. We have no doubt that many, for

whom the plea of homicidal monomania would be raised, are to be deterred by the effects of punishment: some of them have sufficient reason to know, that what they are about to perpetrate is contrary to law, and that the act will certainly be followed by imprisonment, and probably by death, if the plea of insanity should fail. Thus, then, some preventive means are in force; and we consider that the great question of the expediency of continuing capital punishment, or of substituting for it perpetual imprisonment, is in their case undergoing a trial. Homicidal monomania is well known to be imitative; and it is fair to suppose that this tendency to imitate is a proof that all power of reasoning and reflection is not lost. Society is at least justified in endeavouring to coerce such impulses; and the knowledge that perpetual imprisonment will certainly follow the attempt may have an effect on the minds of some. Those who are labouring under violent mania or delirium cannot be so influenced as to their acts; but we consider that the cases of such men as Oxford are not to be placed in the same category with these.

VI. With respect to the pamphlet on the Amendment of the Law of Lunacy, we have no doubt the anonymous author means well; but he has clothed his opinions in obscure and somewhat flippant language. The letter is addressed to Lord Brougham, and is a violent attack upon the opinions of all who differ from the author on the nature of insanity. He does not seem to give any credit to others for entertaining conscientious views on the subject; but his inference is the common one of persons writing with more enthusiasm than discretion, namely, that he himself is right and every other individual wrong. It is to us a matter of surprise that so many ingenious writers on insanity, while endeavouring to expose the delusions of others, should invariably fall into delusions themselves.

There is one good suggestion made by the author in his letter, namely, that questions relative to insanity should be left to be decided by an English board of "experts" of competent knowledge, and appointed under proper restrictions, as to their independence. We must all agree that the practice of taking opinions on this subject from witnesses specially selected by those who are to benefit by their evidence, or from persons accidentally present at the proceedings, is radically bad, and should be forthwith abolished.

To this pamphlet are appended the speeches of Lords Lyndhurst and Brougham in the House of Lords on the case of M'Naughten; and in commenting upon these, we shall endeavour to see what medical or legal inferences can be drawn from the foregoing considerations on the question of criminal responsibility. We must repeat a complaint which we have elsewhere urged, (No. XIX. p. 140,) that the legal test adopted by our judges is, with due respect to these great authorities, imperfect. Lord Brougham doubts whether the juries before whom the question is tried really comprehend what is meant by it. Most judges, in laying down the law of responsibility, tell the jury that to make a man responsible, he must be capable of knowing *right* from *wrong* at the time of committing the act. This Lord Lyndhurst stated to be the general law of the land, and that any alteration in it was impracticable; Lord Brougham properly complained that the test was vague and unsatisfactory; and while one judge required a knowledge of right and wrong, another required a power

of distiuguishing between good and evil, a third that a man must know what is proper or wicked. (p. 24.) Such nice differences as these should rather be addressed to a jury of metaphysicians, than to one composed of plain simple-minded men.

Lord Brougham suggested that the test ought to be whether a man knew that the act he was committing was legal or illegal; in other words, whether, as it is sometimes expressed, the prisoner was aware that his act was contrary to the law of God and man. Now the objection to every one of these tests is that they do not answer the purpose intended. A man may know that the act of murder or incendiarism which he is perpetrating is wrong, that it is an evil, wicked, and illegal act, and yet be a homicidal monomaniac. If the test thus laid down by our law-authorities be correct, and be, as Lord Lyndhurst pronounced it, the general law of the land, we would ask why were Martin, the incendiary, and Hadfield, the assassin, acquitted? The test may appear to answer in some cases: it is also undoubtedly convenient of application; but, if generally and rigorously carried out, it would certainly consign to death many monomaniacs; if it cannot be carried out, it must be erroneous, and some other should be substituted. The true and only test of responsibility appears to us to be, whether or not the individual had at the time *any power of control over his actions*. (See No. XIX. p. 140, and the case of Greensmith, p. 144.)

It has been seriously argued that some remedy is required to amend the present state of the law relative to insanity in criminal cases. This can only be applied, as Lord Lyndhurst remarked, by adopting some measures of precaution to prevent the recurrence of such evils; but it is difficult to understand how this can be done. Are we justly entitled to confine a man who displays habits of eccentricity and waywardness; who is sullen, morose, and reserved; who is ferocious in his disposition, or, on the contrary, remarkable for his humanity to the brute creation; who fancies himself persecuted, while he is shrewd in business, industrious in his habits, and shows no sign of intellectual aberration? We should say certainly not; and that any medical man, who, as the law now stands, signed a certificate for his confinement as a lunatic, would probably have to pay a heavy penalty for his indiscretion. The whole world would exclaim against such a person, on such grounds, being consigned to perpetual imprisonment among lunatics; and yet anything short of this must expose one or more innocent members of society to the risk of assassination. The memorable case of Anderdon *v.* Burrows has rendered medical practitioners scrupulously cautious in respect to the signing of certificates of insanity; and we doubt whether before the commission of their crimes any member of the profession would have felt himself justified in consigning to an asylum such men as Oxford and M'Naughten. Even where a person has indulged in wild and reckless conduct, has given way to vicious and depraved habits, threatened the lives of his parents and friends for no apparent motive, and has passed his time in gambling-houses and brothels, the law has determined that there was no ground for the imposition of restraint. A case occurred to our knowledge within the last few years, in which these facts were proved of a particular person; he was sent to an asylum under the certificate of two medical men; he indicted the parties for conspiracy and false imprisonment, including the keeper

of the asylum, his parents, and a respectable solicitor, who had testified to his insane habits, and this man obtained a verdict in his favour! The conduct of the defendants was considered to involve a serious infringement of the liberty of the subject; and this person is now roaming about the metropolis! We again ask, what can be done with such cases in the present state of the law? It is not to be expected that medical men or magistrates should incur the risk of actions or indictments for false imprisonment; and until some indemnity is afforded to those who act bonâ fide on these occasions, assassinations may be perpetrated, and the preventive remedy applied only after the mischief is done.

But we cannot conceal from ourselves that there are cases of homicidal monomania which no law and no means of precaution can reach. We allude to those where the murderous act is committed from a sudden impulse by a person who had previously displayed no immoral conduct and no sort of intellectual aberration. Instances of this description have been unfortunately frequent of late years; we have elsewhere related one. (No. XIX. p. 144.) Other examples might be found in the cases of Nicholas Steinberg, who killed his wife and four children at Pentonville in Sept. 1834; of a man named Staninought, a respectable tradesman, who killed his son in 1835; of Lucas, who destroyed his three children in March 1842; of Jessup, who killed his wife in Oct. 1842; and of a man named Giles, who cut the throats of two of his infant children at Hoxton in January of the present year. All these were fearful examples of homicidal mania, in which there were no previous symptoms indicative of insanity, or any irregularity of conduct on the part of the homicides, to justify the least interference with their civil liberty. Now it is clear that society can no more protect itself against such cases as these than it can against the effects of the earthquake or the volcano. Their occurrence is purely accidental; but one remarkable feature is, that the murderous propensity in these unrecognizable cases is commonly directed against those who are closely connected with the homicides in blood, and to whom they are attached by the tenderest ties.

There is one way, however, in which the legislature might interfere. Some of these dreadful crimes have been perpetrated under the strong impressions produced by the sight of artificial representations of similar scenes of blood and murder. In the case of Staninought, the murderous propensity, which led him to kill his only son, was clearly traced to the effect produced on his mind by the exhibition of the scene, in which the assassin Fieschi was represented with his infernal machine, and figures of murdered corpses lying around, with all the horrible details of that atrocious act of treason. After the exhibition, the man had neither sleep nor rest from the dreadful vision which haunted him, and he murdered his son the same evening. To those who have studied the subjects of the human mind, and who are therefore acquainted with the influence of the power of imitation on certain intellects, such a result cannot be surprising. The public exhibition of these scenes of blood with their disgusting details in private houses and in public theatres is a disgrace to the metropolis. There is scarcely a murder of any atrocity which is not thus represented; and the real clothes or weapons of the murderer are eagerly sought after to make the exhibition more appalling and attractive. Such exhibitions are visited by thousands; and among these, it is not

extraordinary that cases of homicidal monomania should occasionally appear. So strange are the caprices of the mind, that by giving notoriety or intense interest to any crime, however atrocious, you will produce imitations. Such exhibitions should be suppressed by law, as of a highly dangerous and immoral tendency.

We are desirous of calling attention to the cases of homicidal mania from impulse, to which we have just specially referred, in order to expose the fallacy of a common dictum among our law authorities. It has been repeatedly held by judges that insanity must never be inferred from the act committed; and that the nature and atrocity of the act could not be adduced in support of that plea. We would simply ask, after the occurrence of such cases as those above referred to, how is it possible that a principle of this kind can be maintained as consistent with either law or reason? It cannot be enforced; or if it be, then persons must be executed whose execution could have no effect as an example. In cases of this kind, the act itself is in general the only evidence of insanity: it is said, the admission of such a principle would be dangerous; in our view there is equal, if not greater, danger from its exclusion. In the present state of the law there is always a risk of such persons being executed like sane criminals. (See Greensmith's case, No. XIX. p. 144.)

On the other hand, the plea of insanity may be improperly made and abetted by medical testimony. The act committed may be wrongly taken as evidence of insanity; for it is impossible to say, à priori, what criminal acts should, and what should not, be deemed acts of insanity. This is undoubtedly true; and the only remedy, in our opinion, is to abolish medical testimony as it is at present given on trials for crime where insanity is the plea. Questions of this important nature should be referred to a board of twelve or more competent men: the state of mind of a person accused of crime should not be left to be decided by those members of the profession whom the prisoner or his friends may select for their known support of his case. As to the question of responsibility and punishment, this should be intrusted to the authorities of the law.

ART. VI.

Mémoire sur le Glaucôme. Par le Docteur JULES SICHEL. *Extrait des Annales d'Oculistique*, Vols. V. VI. et VII. — *Bruxelles*, 1842. 8vo, pp. 260.

Memoir on Glaucoma. By Dr. J. SICHEL.—*Brussels*, 1842.

IN this work, Dr. Sichel claims our attention in the triple character of a critic, a pathologist, and a medical historian; and notwithstanding certain serious offences which he has committed, especially in the execution of the last of these offices, we have no hesitation in awarding him the praise due to labour and perseverence in his researches, and to the merit of having produced a book which will not merely repay the perusal of those interested in the pathology of the eye, but which, we conceive, they are bound to study with the greatest care, as containing by far the most complete account of a very serious disease of the organ of vision.

The scope afforded, on the present occasion, to Dr. Sichel's critical

powers, arises from the obscurity attached to the meaning of the Greek word *γλαυκὸς*. The use made by Homer of the same word, to designate the blue colour of the eyes of Minerva, *γλαυκῶπις Ἀθήνη*, and the blueish-green colour of the sea, *γλαυκὴ θάλασσα*, is sufficient to show the original ambiguity of the term. As if, however, on purpose to puzzle the critics, Virgil applies the word to the colour even of horses, *honesti spadices glaucique*. Dr. Sichel thinks, that by *glauci* Virgil meant gray, for which interpretation he assigns, with all due gravity, the very satisfactory reason, that no one ever saw a green horse—" puisqu' aucun individu de l'espèce caballine ne présente dans sa robe le moindre mélange d'une nuance tirant sur le vert."

Dr. Sichel has satisfied us, that the word *γλαυκὸς* was applied by the ancient Greeks to the blueish or grayish colour of the iris, presented by a large portion of the human race, and that it was thence transferred to certain states of the eye, in which the pupil has no longer the natural jet black colour of youth and health, but has assumed the grayish or greenish tinge of age and disease. Some will have it that Hippocrates, under the plural *γλαυκώσεες*, comprehended all those diseases of the eye in which an *opaque* appearance is seen within the pupil;* but Dr. Sichel supposes that he refers to cataract only, and that he had no notion of the *glaucoma* of modern oculists.

The pupil of a young and healthy eye presents a jet black colour, and one unacquainted with the real state of matters might suppose, on looking into such a pupil, that the whole of the light which entered it passed unobstructed to the bottom of the eye, and was there absorbed by the choroid pigment. This, however, is not the case; but, as was first shown by Purkinje,† a quantity of the light is arrested by the anterior crystalline capsule, and another quantity by the posterior, both quantities being reflected back again through the pupil and out of the eye. If the source of the light so reflected be a small luminous object, such as a lighted candle, two distinct images are formed of the object, the one erect and the other inverted, as is readily perceived by the spectator. This catoptrical effect of the two membranes forming the capsule of the lens, since it was first announced by Purkinje, has been studied with considerable attention by Sanson and others,‡ as a means of detecting various diseased states of the crystalline body; and although its value seems very insufficiently known to Dr. Sichel, we look upon it not only as one of the most ingenious and beautiful applications of optical principles to the illustration of disease in the eye, but also as one of the most sure and useful. A certain size, brightness, and colour attend the images in the young and healthy eye, but are modified by whatever disease affects the lens or its capsule. Among the diseases which modify the images is glaucoma.§

If we look into the eye of an old person, we very rarely find the pupil of a jet black colour. On the contrary, it presents more or less of a

* Mackenzie, On Glaucoma. Glasgow Medical Journal for August, 1830, p. 254.

† Purkinje, De Examine physiologico Organi Visus, &c.—Vratislaviæ, 1823.

‡ Mackenzie, On Lenticular Glaucoma. London Medical Gazette, vol. xvi. p. 107.—London, 1838.

§ We need scarcely mention that the two images referred to in the text are independent of the image reflected by the cornea.

greenish hue, and this is what constitutes strictly and truly glaucoma. If we take a lighted candle, and move it in front of such an eye, we immediately discover, that, while both the erect image formed by the anterior capsule, and the inverted one formed by the posterior, are distinct, the erect one is larger and brighter than in the young and healthy eye, and has somewhat of a yellowish hue. The inverted image is also larger and more of a yellowish colour than in the young eye, and not unfrequently its outline has lost its original sharpness and become somewhat diffused. These changes indicate the *first* degree of glaucoma. They are attended by a lively pupil, and a distinct vision of distant objects: they are the result of the lens having assumed a yellow or amber colour, and of its kernel having acquired a greater density than what it possessed in youth.

What we have now stated is altogether at variance with the opinions of Dr. Sichel. He will not admit that the greenish appearance which we have just described has any claim to be considered as a glaucoma at all (p. 30), but insists that it is merely what he calls "un reflet particulier du cristallin sur la choroide." "A particular reflection of the crystalline on the choroid" does not appear to be a very accurate mode of expression. Perhaps what Dr. Sichel meant to say was this, that the light entering the eye, being refracted by the crystalline, is brought to a focus on the choroid, and is thence reflected through the humours of the eye, so as to be seen by a spectator; but we are convinced that the appearance which Dr. Sichel designates by the name of "reflet particulier du cristallin" is a reflection, not from the choroid, but from the crystalline itself, and is a result, along with the changes in the images already referred to, of the kernel of the lens having assumed an amber hue, and acquired a greater degree of firmness, and therefore a greater reflective power than what it possesses in the young and healthy eye. It is easy to ascertain the truth of this, by opening the cornea and extracting the lens from the eye after death, when the glaucomatous appearance will be seen to be entirely removed. If Dr. Sichel will only throw the light, refracted to a focus by a double convex lens, on his hand or on a sheet of paper, and try to see it reflected from the hand or the paper, through the lens, we think he will give up the notion of any similar reflection being seen from the choroid through the crystalline. Were there a window in the sclerotica, through which the doctor might peep without looking through the lens, then, indeed, something might be seen, resembling "le foyer d'une loupe mobile, qu'on promenerait au-dessus d'un fond opaque," (p. 30,) but to discern anything like this through the crystalline is impossible.

The glaucomatous reflection from the lens of an old person generally increases slowly, as age advances; but so long as it is combined with a lively pupil, distinct vision of distant objects, and a natural firmness of the eyeball to the touch, we should call it the *first* degree of glaucoma, or a simple uncomplicated glaucoma. In what may be styled the *second* degree, the greenish reflection is more striking, the eyeball assumes a preternatural degree of hardness, the motions of the pupil become limited and slow, and the vision of distant objects by the naked eye and of near objects through convex glasses is more or less disturbed. On examining the eye catoptrically, the inverted image is distinct when formed

near the edge of the crystalline, but becomes less and less distinct, as, by moving the candle, it is brought towards the centre of the pupil. Photopsia, pain in and round the eye, partial changes in the colour of the iris, and a dilated state of the subconjunctival vessels, not unfrequently attend this stage.

In the *third* stage, the green appearance of the lens is still more remarkable, the eyeball becomes of a stony hardness, the pupil is dilated and fixed, and vision is extinguished. On examining the eye catoptrically, the inverted image is no longer visible, even at the edge of the lens. In this third stage, as also in the second, the erect image formed by the anterior capsule, is large and evident, but its outline is indistinct, so that it looks like a diffused blaze. The fact of its being so distinct, is to be attributed to the kernel of the lens having acquired, not merely an amber, but a reddish-brown colour, with a certain degree of opacity, so that it serves as a foil to the image.

A *fourth* stage may succeed, in which the lens becomes cataractous, as well as glaucomatous; and a *fifth*, in which the opaque lens, much hypertrophied, is pressed, through the pupil, against the cornea, till the cornea ulcerates and gives way, allowing the lens to escape, along with a considerable quantity of blood from the internal vessels of the eye.

While it may be a matter of doubt, whether Hippocrates employed the plural γλαυκώσεες to signify cataract only, or to comprehend every sort of opacity seen within the eye, it is certain that Rufus and Galen used the term γλαύκωμα for an opacity of the crystalline, and ὑπόχυμα to denote a concreted fluid occupying the posterior chamber. This concreted fluid, as they took it to be, (for it was in fact the lens in the state of cataract,) they found could be removed, and vision restored, by the operation of couching; glaucoma they declared to be incurable. The notion that cataract or ὑπόχυμα was a concreted fluid being refuted by Quarré, Lasnier, and others, in the early part of the 17th century, and the seat of that disease being ascertained to be the lens, the term glaucoma ran a chance of being lost, had it not been taken up by Brisseau, and applied by him to signify an opacity of the vitreous humour. This notion of Brisseau regarding the nature of glaucoma has, in a great measure, kept its ground down to this very hour, notwithstanding the light thrown on the pathology of the disease by the dissections of Dr. Mackenzie, an account of which he published in 1830, and from which the following are extracts:

"The following are the particulars which I observed.

"1. The choroid coat, and especially the portion of it in contact with the retina, of a light-brown colour, without any appearance of pigmentum nigrum.

"2. The vitreous humour in a fluid state; perfectly pellucid, colourless, or slightly yellow. No trace of hyaloid membrane.

"3. The lens of a yellow or amber colour, especially towards its centre; its consistence firm, and its transparency perfect, or nearly so.

"4. In the retina, no trace of limbus luteus, or foramen centrale.

"To the first of these changes, namely, the deficiency of pigmentum nigrum, I am inclined to ascribe, in a great measure, the opaque appearance of the deep-seated parts of the eye in glaucoma. This appearance I regard as a reflection merely of the light from the retina, choroid, and sclerotica; it is probably blueish when it first leaves the reflecting surface formed by these membranes, but immediately assumes a greenish hue from passing through the yellowish fluid

which occupies the place of the vitreous humour, and through the lens, which is still more decidedly of a yellow, or even amber colour, at that period of life when glaucoma is most apt to attack the eye. . . . .

"In my dissections of glaucomatous eyes, I have detected no other change in the retina than what I have already mentioned; namely, a want of the limbus luteus and foramen centrale. The membrane never appeared to be thickened or changed in colour; and as for the vitreous humour, instead of being thickened or opaque, as described by authors, it was always fluid and perfectly transparent. I by no means presume to assert that a turbid state of the vitreous fluid, or an opacity of the hyaloid membrane never occurs; nor do I deny that the retina, in certain cases, becomes thickened and opaque. What I believe myself warranted in maintaining is this, that the well-known appearances of glaucoma are independent of any of these changes, and to be attributed to certain other morbid alterations of the internal parts of the eye. . . . .

"There is no green surface in the human eye to reflect the light of that colour, as there is in the eye of the sheep; it must be, then, in its transmission that it acquires the greenish hue, and the part most likely to affect it in this way is the lens. Were it proved that the retina, which is naturally somewhat blueish, supported by a choroid destitute of pigment and a whitish sclerotica, reflects the light forward into the eye of a blueish colour, then one of the principal phenomena of glaucoma might be regarded as no longer difficult of explanation. In confirmation of this, if the lens is removed in this disease, or sinks to the bottom of the dissolved vitreous humour, the green appearance is almost entirely lost."

We have been led to make these extracts from Dr. Mackenzie's first paper "On Glaucoma," published in the Glasgow Medical Journal for August 1830, because, strange to tell, Dr. Sichel, while with an extreme minuteness he refers to every other publication upon glaucoma, and while he makes frequent mention of Dr. Mackenzie's opinion on that disease, avoids all reference to the paper in question, as well as to the first edition of Dr. Mackenzie's Practical Treatise on the Diseases of the Eye, published also in 1830, and in which that paper was reprinted *verbatim*. The reason of this disingenuousness does not appear, till the very end of Dr. Sichel's work, where, in a summary of conclusions, he puts in the following claim to the very observations originally given to the world by Dr. Mackenzie in 1830:

"It was only in 1831, that M. Canstatt and myself made another important step in advance, as to the knowledge of the nature of glaucoma. We proved that the decoloration of the choroid, and the yellow hue of one of the refracting media, the vitreous body, and not the green opacity of the latter, are the true causes of the seeming greenish opacity of the bottom of the eye in this disease. In 1837, I showed that the crystalline, become yellowish, had more influence in producing this hue, than the vitreous body." (p. 258.)

On this occasion, then, Dr. Sichel has not acted "*en historien fidèle;*" what he states not to have been done till accomplished by himself and M. Canstatt in 1831 and in 1837, was already known to the profession in 1830; and we can assure him, that the leap which he makes in his chronological review, (p. 221,) from 1828, *Schoen*, to 1831, *Canstatt et Sichel*, however it may pass without detection in Paris or in Brussels, will at once appear to an English or a German reader in its true light.

It is plain, both from the symptoms during life, and the state of the eye after death, that various elements enter into the production of a glaucoma of the *third* degree, or what Dr. Sichel calls "*un vrai glaucôme.*" Pain and congestion are present, the one affecting the ramifi-

cations of the ophthalmic nerve, and the other the whole vascular structure of the eyeball. There is a change in the colour and consistence of the lens, the chief factor in the production of the greenish hue seen behind the pupil; an accumulation of dissolved vitreous humour, compressing the retina, and over-distending the tunics; an absorption of the choroid pigment; and an insensibility of the retina. These several elements are observed to coexist in different degrees, in different cases. In some, the retinal element precedes the rest, or is more than ordinarily advanced. The disease is then regarded as an amaurosis accompanied by glaucoma, and by some is called *amaurosis glaucomatosa*. In other cases, the lenticular element is the most remarkable; the hyaloid membrane and the retina are comparatively sound. The lens, having first of all grown yellow and glaucomatous, may now become cataractous; opaque, that is, on its surface, as well as glaucomatous in its kernel; and may be extracted with advantage. This constitutes the "*cataracte verte opérable*" of Dr. Sichel. If the glaucomatous lens becomes cataractous, after the hyaloid membrane is dissolved, the retina insensible, and the blood-vessels of the eye varicose, the disease is styled *cataracta glaucomatosa*, and is altogether hopeless.

Various views have been entertained as to the first and principal pathological change in glaucoma. Dr. Mackenzie seems to attribute the absorption of the choroid pigment, the insensibility of the retina, and even in part the glaucomatous change of the lens, to the pressure of a superabundant vitreous fluid. Mr. Tyrrell, who still holds to the exploded doctrine that the vitreous body is opaque, and that this is the cause of the glaucomatous appearance, considers the morbid action to be first set up in the retina, and thence to spread to the hyaloid membrane, and to the lens.* One of the peculiar doctrines of Dr. Sichel is, that inflammation of the choroid is the cause of glaucoma. This opinion he insists on with great earnestness, and we consider his reasonings on the subject as deserving serious consideration. If we admit with Dr. Sichel, that choroiditis is always the direct and principal cause of glaucoma, (p. 98,) it is somewhat remarkable that this effect follows only in those who are past middle life, and that inflammation of the choroid produces no such change in the transparent media of the eye in young persons. He tells us, that his dissections have always presented manifest signs of disorganization, more or less extensive, of the choroid; and we know that, in the *fourth* stage, the choroid and retina have occasionally been so much changed as scarcely to be recognized. We must confess, however, that the signs of disorganization mentioned by Dr. Sichel, namely, the injection of the vasa vorticosa by blood, the thinness of the choroid, and the deficiency of pigment, scarcely seem sufficient for the foundation of so positive and unrestricted an opinion on the choroidal origin of the disease, as that which prevades the work before us.

Dr. Sichel announces (p. 1,) that his special object is the pathological anatomy of glaucoma, hitherto, he says, very incompletely studied. But, as far as we have discovered, he has made no remarkable addition, under this head, to what was previously known. He seems anxious to make out the choroid to be of a blue colour, (p. 87,) in order to explain the

* Tyrrell, Practical work on the Diseases of the Eye, vol. ii. p. 134.—London, 1840.

green hue of the humours, by what he calls a *fusion* (p. 92) of the blue light reflected from the choroid with the amber colour of the lens; but after all, he scarcely ventures to say more than this, that the choroid, when he looked at it, in his dissections, through the grayish retina, appeared blueish; adding, however, that during life this blueishness must be greater, from the blue colour of the venous blood with which the choroid is surcharged. To the swollen state of the choroid he attributes the propulsion of the lens through the pupil, in the last stages of glaucoma; and what is generally called the arthritic ring, seen between the sclerotica and cornea, he believes to be the effect of a dilated state of the circular venous sinus of the iris. The thinning of the sclerotica which occurs in advanced cases, he ascribes to pressure produced by the choroid.

Dr. Sichel has pointed out a symptom of glaucoma, which had escaped the notice of previous observers; namely, a change in the colour and structure of the iris. This membrane, in the part affected, assumes a grayish-slate colour, and, losing it natural fibrillary texture, becomes smooth. In this state, the iris grows thin, and is deprived of pigment, although only in patches here and there. Dr. Sichel states, that this symptom sometimes precedes the glaucomatous appearance of the humours.

In tracing the diagnosis between glaucoma and various diseases with which glaucoma might be confounded, Dr. Sichel mixes up three diseased states of the eye, which are totally different from one another, namely, fungus hæmatodes of the optic nerve, the deep-seated non-malignant deposition which so frequently follows punctured wounds of the eye, and the silvery appearance, called amaurotic cat's eye, represented by Ammon in the 10th and 11th figures of the 15th plate of the 1st volume of his *Klinische Darstellungen.*

From the remarks he makes on *opalescent amaurosis*, it is plain Dr. Sichel has never seen this curious affection. He would fain confound it with his favorite *reflet particulier du cristallin sur la choroide.*

Dr. Sichel's description of sub-choroid dropsy is good; it bears internal marks of exactness and originality, and, combined with Panizza's case,* in which the diagnosis was so mistaken, that the eye was extirpated, affords some excellent hints, of which systematic writers on eye diseases would do well to avail themselves. Dr. Sichel's notion, however, that it is only Jacob's membrane which is inflamed in sub-choroid dropsy, we cannot subscribe to, and doubt very much if that membrane is vascular in any degree.

The cases which Dr. Sichel has related (§ xvii) are well drawn up, and highly instructive. They are chiefly cases of glaucoma with arthritic ophthalmia. The first case, however, (p. 37,) appears rather to be one of dislocated lens from a blow than a pure case of glaucoma. The blow was received two months before Dr. Sichel saw the patient. Instead of immediately extracting the lens, Dr. Sichel went on endeavouring to abate the inflammation by leeches, purgatives, calomel and opium, frictions with blue ointment and laudanum, and the internal use of colchicum. Disorganizing inflammation proceeding in spite of all these remedies,

* Panizza, Sul Fungo Midollare dell' Occhio Appendice, p. 9.—Pavia, 1826.

when at last, five months after the injury, Dr. Sichel extracted the lens, no wonder that, the vitreous humour escaped, followed by profuse bleeding from the interior of the eye.

In a work of such extent, and printed in detached portions at considerable intervals of time, it might be expected, that some contradictions should occur. The following are remarkable, and deserve Dr. Sichel's attention in a new edition.

Speaking of the green colour of glaucoma, he tells us, (p. 4,) that it is sometimes "un peu sale, comme mêlé de gris;" but at page 241, he condemns those authors who have seen any grayish tinge in the opacity of glaucoma, and gives it as his opinion, that they have confounded glaucoma with sub-choroid dropsy. At page 227, he speaks of the different shades of the yellow colour of the lens in glaucoma, and of its greater or less transparency or opacity, which is flatly in contradiction to the statement made in page 91, that the lens is entirely transparent, and normal in all respects. At page 95, he says, that neither Benedict, Rosas, nor himself had observed the fluidity of the vitreous body to be so general as Mackenzie states it to be; but at page 123, he says it is generally fluid. At page 227, he declares that the choroid in glaucoma is inflamed in its whole elements; but at page 241, he says glaucoma often commences with simple venous congestion of the choroid. In the same page, (p. 98,) appear two statements regarding the green colour of glaucoma totally at variance with one another. The first is, that the alteration of the choroid, the thinning and the change in colour of its pigmentous layer, joined to the yellow tinge of the crystalline, and often also of the vitreous body, alone are sufficient to explain the apparent green opacity of the bottom of the eye. The second is, that, in certain cases, the particular alteration of the choroid, without any yellow colour of the refractive media, may, by the mere refraction which the rays of light undergo in passing through these media, give rise to the green opaque appearance.

Perhaps the most defective part of Dr. Sichel's work is that which treats of the causes of glaucoma. Enslaved, as it were, by a particular notion, he does little more than tell us, that "les causes du glaucôme sont celles de la choroïdite."

As to the treatment, he declares glaucoma to be completely incurable.

"Neither the antiphlogistic treatment," says he, "employed in all its extent and with all its rigour, nor small derivative or depletive bleedings, nor the most energetic counter-irritants, tartar-emetic ointment, the seton, cauteries or moxas to the mastoid processes and to the temples, so much vaunted by some authors, nor mineral waters, nor specific remedies extolled by others, have ever cured or even arrested the progress of a true glaucoma."

We are sorry we cannot afford more space for extracts from Dr. Sichel's interesting publication, on the general character of which we have already pronounced a favorable judgment. It grieves us to have been compelled to speak in some respects disadvantageously of the work of so assiduous an observer as we believe Dr. Sichel to be. It is an irksome task to find fault; but the candid reader will, we think, confess, on examining Dr. Sichel's work for himself, that on this occasion the fulfilment of the task has not been unnecessary.

Art. VII.

The Life of Sir Astley Cooper, Bart., interspersed with Sketches from his Note-books of distinguished contemporary characters. By Bransby B. Cooper, Esq. f.r.s. In Two Volumes.—*London*, 1843. 8vo, pp. 448, 480.

When we learned that Mr. Bransby Cooper had in the press a memoir of his uncle, Sir Astley, we naturally expected that the history would have reference chiefly to that profession of which the subject was so bright an ornament, and the author a distinguished member; nor was it without disappointment that we discovered, on taking up the volumes, that they contained simply the life of Sir Astley Cooper as an individual. Biography of this kind affords, for the most part, little that is interesting or instructive. Men may be very illustrious in a professional or public capacity, and yet differ very little from ordinary mortals in the details of their private life. They are born like others; they live by the exercise of similar functions; they have their peculiarities of disposition and of temper; their personal attachments and aversions; they die, and are remembered by the world only through the enduring influence of the master mind, which has impressed its own character on some particular department of human knowledge or human activity.

The only individuals whose private history is possessed of much interest are those who have been involved in striking personal adventures. However impressive may be the scientific life of a Newton or a Kepler, the political history of a Richelieu or a Chatham, or the military career of a Napoleon or a Wellington, the private life of any one of these individuals will contain very little more than that of the most commonplace man in the world, and will be far inferior in interest to that of a Turpin or a Vidocq, or any one, good or bad, who has passed through extraordinary vicissitudes of fortune, or surmounted extraordinary personal dangers and difficulties.

Our author tells us in his introduction, that "there is no doubt that Sir Astley Cooper intended that whatever biographical memoirs of him might be published, should comprise an analysis of his professional writings, an account of the circumstances under which they were produced, their peculiar merits, and a comparison of them with the existing state of knowledge at the time of their appearance; that, in short, they should afford a complete view of him, both as the surgeon and the author." Considering, however, that such a biography would have been a sealed book to all but professional readers, and conceiving that the public would be gratified by "an insight into the habits and pursuits of a man, who for many years served them extensively in his professional capacity, and in whom they always exhibited the greatest interest," Mr. B. Cooper determined on publishing, at present, a merely personal memoir of Sir Astley, and deferring to a future opportunity the history of his strictly professional life. We regret extremely that Mr. B. Cooper has acted on this resolution. Sir A. Cooper, we know, was much and worthily beloved by his friends, but we apprehend that the "interest" taken in him by the public, arose simply from their admiration of his character as a surgeon. Take him out of his profession, and his history is merely that of

a worthy and kind-hearted man, clever, and somewhat given to waggery, endowed with a shrewd eye to his own interest, and a benevolent disposition to promote the welfare of others. This is not a very remarkable character, nor one requiring two octavos for its delineation. We do not think the memoir before us will be found to possess as much general interest as the author imagines; and, unhappily, it lacks that which would have made it in the highest degree interesting to the medical reader, viz. a critical retrospect of Sir A. Cooper's professional life; a comparison of his surgical character and labours with those of Scarpa, Dupuytren, and other illustrious surgeons of his day, and a comprehensive view of the progress of the art throughout Europe during the last half century. The name of Astley Cooper, however, has been so long familiar to the world, and his great talents and well-known *bonhomie* have so endeared his memory to his own profession, that we have no doubt even the memoir before us will be perused by many of that profession with considerable interest. It has moreover one curious feature, which, although anomalous, and derogatory from its character as a classical biography, renders it entertaining, and in some respects valuable, namely that of giving a sketch of the lives or characters of all the persons with whom Sir Astley was thrown into any intimate relation, and many of whom were necessarily persons of distinction.

Under ordinary circumstances, purely biographical works do not come within the range of criticism prescribed to this Journal. In the present instance, however, the great professional eminence of the individual who is the subject of the memoir, induces us to deviate from our usage, and leads us further, to present our readers with an outline of the personal history of Sir Astley Cooper, for the materials of which we are almost entirely indebted to the volumes before us.

Astley Paston Cooper was born at Brooke, in Norfolk, on the 23d of August, 1768. He was the fourth son of the Rev. Dr. Samuel Cooper, who then held the rectory of Brooke, and his wife, Maria Susanna, eldest daughter of James Bransby, Esq. of Shotisham. In the year 1781, Dr. Cooper was appointed to the perpetual curacy of Great Yarmouth, and, on obtaining that preferment, left Brooke, where he had resided for thirteen years, and where he appears to have been much beloved by his parishioners. His affluence and hospitality made his acquaintance much sought for, and among his visitors were some of the most celebrated literary and political characters of the day. He possessed, in an eminent degree, the feelings and habits of a gentleman, though his disposition was tinctured with austerity, and was not altogether free of the dogmatism into which men much courted in provincial society are so apt to fall. He was the author of numerous publications, on religious and political subjects. He died at Great Yarmouth, in January, 1800, in the sixtieth year of his age.

Maria Susanna, the mother of Sir Astley Cooper, was born at Shotisham, in the year 1737. Her father, James Bransby, Esq. was descended from an ancient family in Yorkshire, a scion of which afterwards settled at Harleston, in Norfolk. Her mother, Anna Maria Paston, was the daughter of James Paston Esq. of Harleston, a first cousin of the Earl of Yarmouth. She appears to have been a lady of rare endowments

both of mind and heart. She published several works, chiefly novels, having a moral or religious tendency, which enjoyed a high reputation in their day. She survived her husband some years, and died in July, 1807.

In conformity with a very absurd and pernicious custom then prevalent among the more respectable families of Norfolk. the infant Astley Cooper was transferred to the care of a foster-nurse, Mrs. Love, the wife of a substantial farmer, one of Dr. Cooper's parishioners.

As soon as he was of an age to receive instruction, his mother undertook the care of his general education in common with that of her other children, and his father subsequently initiated him into the rudiments of classical learning. As is well known, however, he never attained any great proficiency as a scholar, his turn of mind being entirely different from that which leads to excellence in philological pursuits. His only other preceptor, at this period, was Mr. Larke, the master of the village school, who used to attend at Brooke Hall to instruct Dr. Cooper's children in writing, ciphering, and mathematics. Among all this gentleman's pupils, none seems to have done him less credit than young Astley. Mr. Lark, it appears, laboured under so great a susceptibility of external impressions in the epigastric region, that if he received a slight blow, or was merely touched in that part, he was for some time deprived of respiration. Of this infirmity our young Pickle did not omit to avail himself on certain occasions, where the state of his own studies rendered a suspension of his tutor's faculties desirable.

He grew into a lively and daring boy, always in mischief, yet so open-hearted and good-humoured, that no one could retain their anger against him long. He delighted in all sorts of extravagant and dangerous enterprises; he would ride the most vicious horses without a bridle, and drive the cows out of the fields mounted on the back of a bull; and, on one occasion, he fractured his collar-bone in an attempt to leap an ass over a recumbent cow, who was so disobliging as to get up in the midst of the exploit, and roll him and his Bucephalus on the field.

A short time previous to Dr. Cooper's removal from Brooke, an accident occurred which afforded striking indications of the presence of mind and aptitude for practical surgery by which Astley Cooper was afterwards so much distinguished. A son of his foster-mother, Mrs. Love, somewhat older than himself, having been sent to convey some coals to the house of the vicar, fell down by some accident in front of the cart, a wheel of which, passing over his thigh, caused, among other injuries, a laceration of its principal artery. The boy was carried home almost exhausted with hemorrhage, and all was alarm and confusion, when Astley Cooper, who had heard of the accident, arrived, and with admirable presence of mind bound his pocket-handkerchief so tightly around the limb above the wound, as to arrest the bleeding. In this manner the life of the patient was prolonged till the arrival of the surgeon who had been sent for from Loddon. Sir Astley Cooper used to relate that this accident, which he always regarded as a remarkable event in his history, first bent his thoughts towards the profession of surgery.

On his arrival at Yarmouth, Astley Cooper continued as great a madcap as ever, and some of his pranks evince a keen perception of fun and considerable talent in the invention of ludicrous positions. The following eccentric contribution to meteorology is amusing enough:

"Having taken two pillows from his mother's bed, he carried them up to the spire of Yarmouth church, at a time when the wind was blowing from the north-east, and as soon as he had ascended as high as he could, he ripped them open, and shaking out their contents, dispersed them in the air. The feathers were carried away by the wind, and fell far and wide over the surface of the market-place, to the great astonishment of a large number of persons assembled there. The timid looked upon it as a phenomenon predictive of some calamity, the inquisitive formed a thousand conjectures, while some, curious in natural history, actually accounted for it by a gale of wind in the north blowing wild-fowl feathers from the island of St. Paul's. It was not long, however, before the difficulty was cleared up in the doctor's house, where it at first gave rise to anything but those expressions of amusement which the explanation, when circulated through the town, is reported to have excited." (pp. 73-4.)

On another occasion he secreted himself in the church, and obliged the Rev. Dr. Cooper with an exact echo of his voice while reading the marriage service. The doctor gravely observed that he had never perceived an echo in that place before; while the clerk, who had a shrewd guess at the real nature of the phenomenon, could with difficulty restrain his laughter, though filled with alarm lest the trick should be discovered.

But amid these frolics Astley Cooper now began to feel dissatisfied with his mode of life, and conceived a growing desire of occupation, independence, and distinction. The bent towards surgery, which he had acquired from the circumstance already related, was greatly increased by the conversation of his uncle, Mr. William Cooper, who occasionally visited his father, and was at that time senior surgeon to Guy's Hospital. Their intercourse affording mutual pleasure, it was finally determined that Astley should embrace the profession of surgery, and he was accordingly articled as pupil to his uncle.

Our student set off for London in August 1784, leaving his family in a state of solicitude as to his future conduct, which was naturally engendered by his previous unsettled habits, and neglect of the more serious duties of life. It being inconvenient to Mr. W. Cooper to receive his nephew into his own house, it was determined that he should reside at that of Mr. Cline, who then took a few pupils to board with him. This arrangement turned out highly advantageous to Astley Cooper; for his uncle, though warmly interested in his success, and entertaining a high opinion of his good qualities and abilities, had a certain roughness of manner, and strict notions of discipline which might have ill accorded with the impetuous and ungovernable disposition of his pupil.

For a short time, there seemed some reason to fear that the apprehensions of Astley Cooper's family with respect to his conduct would be realized. His handsome person and engaging manners rendered him a general favorite in society, and his lively temperament, as yet uncontrolled by addiction to any serious pursuit, led him into a gay and dissipated course of life; while the love of adventure, which still adhered to him, occasionally urged him into extravagances of conduct like those which had characterized his earlier years. His connexion with Mr. Cline, also, though highly desirable in a professional point of view, was very much the reverse in some other respects. The political principles of that gentleman brought him into the closest intimacy with Horne Tooke, Thelwall, and other leaders of the Jacobin party in England. The ardent and undisciplined mind of Astley Cooper made him an apt pupil in such a school,

and he became deeply tinctured with political fanaticism. Fortunately, however, a growing fondness for his profession soon engaged him better than in the pursuit of chimeras. At Christmas 1784, he was transferred, as an articled pupil, from Mr. Cooper to Mr. Cline, and about this time he seems to have laid aside his idleness, and to have devoted himself earnestly to the study of surgery. So great was his diligence that, by the spring of the following year, he had become as distinguished for industry as he had before been notorious for wasting his time, and had, in particular, attained a degree of proficiency in anatomy far exceeding that of any other pupil of his own standing at the hospital.

It is uncertain whether, at the conclusion of his first winter session, he remained in London, or paid a visit to his family at Yarmouth, though the latter is more probable. In October 1785, Astley Cooper resumed his studies with renewed ardour. He was led to the particular cultivation of anatomy, both as the only solid basis of medical and surgical knowledge, and as the chief means of professional advancement for a young man in the position in which he then stood. The demonstratorship of anatomy at the school of a large metropolitan hospital has always been deemed important as a first step in the career of an operative surgeon, and was considerably more so at that time than it is at present, from the smaller number of those capable of fulfilling its duties. The office of demonstrator at Guy's was at that time filled by Mr. afterwards Dr. Haighton, whose want of affability seems to have rendered him unpopular among the students. Astley Cooper had already so distinguished himself by his superior knowledge of anatomy as to be generally referred to by his fellow-students in their difficulties; and this, together with his connexion with Mr. Cline, caused him to be regarded as a sort of second demonstrator, while his kindly disposition and frank address made him a universal favorite among the pupils. These circumstances encouraged him to look forward to the place of demonstrator in the event of its becoming vacant. Having rendered himself an efficient anatomist, he began to feel the advantages of accompanying Mr. Cline in his visits to the Hospital. He watched the progress of the cases with a scrutinizing curiosity, carefully examining the nescropic appearances in such as were fatal. He took notes of Mr. Cline's cases, and soon became remarkable for his quickness in seizing upon their leading points.

During the first winter of his studies, Astley Cooper had become a member of the Physical Society, the oldest and one of the most valuable institutions of its kind at that time existing in London. He seems, however, to have been a very negligent member; and we learn that, in the course of the session, he was fined for absence no less than fifteen times. In the second session of his studies, however, we find him absent only once. Indeed, his mind seems now to have become thoroughly impressed with the importance of his studies, and imbued with the love of them; and his conduct, instead of exciting alarm lest he should become an incompetent member of his profession, called forth feelings of pride and satisfaction from his friends at home, and congratulations from his teachers and acquaintance at the hospital.

In the summer of 1786 he again visited Yarmouth, where, instead of wasting his time in his former vagaries, he did not even spend it in mere recreation, but engaged himself much in the surgery of Mr. Francis Turner, an intelligent general practitioner, with whom he was intimate,

with a view of acquiring a knowledge of pharmacy. Truth, however, obliges us to observe, that he must either have profited little by his experience at Mr. Turner's, or forgotten in after-life much of what he had acquired; for Sir Astley was notoriously deficient in pharmaceutical knowledge. He also, during this visit to Yarmouth, conversed much with the well-known Dr. Aikin, and with his friend Mr. Holland, then a highly intelligent medical student, the father of the celebrated author of the Medical Notes and Observations.

On his return to town he renewed his studies with increased ardour. If he had imbibed from Mr. Cline some tenets which he would have been better without, he derived, from the same source, a predilection which had a very favorable influence on his professional character; and he became, like his preceptor, an enthusiastic admirer of John Hunter, a course of whose lectures he attended. The obscurity of expression which rendered the writings and discourses of that great man so difficult to understand, did not deter him from unravelling his meaning by earnest attention, while, by discussing the subject of each day's lecture with his friend Mr. Holland on his way home, as well as by private experimental inquiry, he succeeded in fixing Hunter's principles in his mind, and became fully convinced of their truth and importance.

About this time, Astley Cooper was near falling a victim to malignant fever, contracted by a visit he paid, at the request of his father, to a person named Gregson, who was a prisoner in Newgate, and whose history Mr. B. Cooper has thought sufficiently interesting to be recorded. We think otherwise: suffice it therefore to say that he was hanged. Astley Cooper's illness proved very dangerous; but he recovered under the care of Mr. Cline and his family. It was not, however, until he had for some time breathed the air of his native county, that he was completely restored to health.

In the winter of 1787, he visited Edinburgh, carrying with him letters of introduction to the most eminent medical and scientific men of that city. Having presented his letters, he immediately applied himself to the objects of his journey; and, for the convenience of doing so, hired a lodging close to the principal scene of his studies. Neither his abode nor his style of living appear to have been of a very sumptuous character, judging from a note of his own relating to this period:

"August 30th. Walked out to the college, and to Bristow Street, and saw the infirmary. Saw my lodging, No. 5, in Bristow Street; walked up into my room, where I spent six shillings and sixpence per week in lodging; dining in Buccleugh Place with Mrs. Mackintosh, at one shilling per diem."

In this part of the memoir Mr. B. Cooper gives various anecdotes of the principal men who figured in Edinburgh at that time, which can interest only those who were acquainted with the place and the period. As a specimen of the kind of notes which Astley Cooper was wont to make, we subjoin a few written after he left Edinburgh, and retrospective of some of the characters he there met with. They exhibit some vivacity, but otherwise nothing remarkable.

"Adam Smith was good natured, simple-minded, unaffected, and fond of young people. Mackenzie I saw little of. Gregory's lectures on clinical medicine were admirable; yet he thought most highly of his physiology, on

which he enlarged in his evening lectures on therapeutics. Having on one occasion been confined to my room by illness, I expressed my regret to Dr. Gregory at losing his clinical reports, but he said, 'Sir, that does not signify; but you have lost my therapeutics.' Black said, 'Sir, you will speak to me after lecture if you do not understand anything. Have you fixed upon a tailor or a shoemaker? I can recommend you to one and the other:' but seeing he might carry his furnaces in his shoes, and that his coat was probably like that worn by Noah in the ark, I thankfully declined. Lord Meadowbank was a sharp man, something like Wollaston. Charles Hope was a man of reading, a gentleman, and dignified, and very eloquent. Old M—— grunted like a pig. He was a tolerable lecturer, possessed a full knowledge of his subject, had much sagacity in practice, was laudably zealous, but was much given to self and to the abuse of others. I gave him two instruments, Cline's gorget, and an instrument for scratching the capsule of the lens, and the next day he said, 'Gentlemen, Mr. Cooper has given me two instruments, one for scratching the capsule of the lens, which *may be* useful; the other, a cutting gorget, and it is curious I myself invented this very instrument twenty years ago.' Fyfe I attended, and learned much from him. He was a horrid lecturer, but an industrious worthy man, and a good practical anatomist. His lecture was, 'I say—eh, eh, eh, gentlemen; eh, eh, eh, gentlemen—I say,' &c.; whilst the tallow from a naked candle he held in his hand ran over the back of it and over his clothes: but his drawings and dissections were well made, and very useful.

"I was glad I went to Edinburgh, because I learned that distance enhances the character of men beyond their deserts. Cullen, and Black, and Dugald Stewart, however, were great men, and being near them did not diminish the importance I had been led to attach to them from their public character. Dugald Stewart was beyond my power of appreciation,—metaphysics were foreign to my mind, which was never captivated by speculation;—but Dr. Black's lectures were clear, and I knew enough of the subjects he treated upon to understand them. Never shall I forget the veneration with which I viewed Cullen; he was then an old man: physic may have much improved since his time, but if Hippocrates was its father, Cullen was its favoured son." (Vol. I, pp. 170-2.)

While in Edinburgh, Astley Cooper became a member of the Royal Medical Society, and so distinguished himself in its discussions that he was offered the presidency in the event of his return to Edinburgh, which, however, never took place. He was also a member of the Speculative Society, and read before it a paper in support of the Berkeleian doctrine of the non-existence of matter; but this he probably maintained only for the sake of argument, as the society in question was considerably more given to ratiocination than to the establishment of fact.

At the period of Astley Cooper's visit to Edinburgh, his comparison of the state of surgery in that city with the state of the same art in London could not have been very favorable to the former; he nevertheless formed a high estimate of the general state of medical education in Edinburgh, and attributed much of his well-known facility of concentrating his whole knowledge and experience on each case that came before him to the correct order and comprehensiveness of the Edinburgh course of study.

At the conclusion of the session, he made a tour to the Highlands which extended to the Western Isles; after which, beginning to be tired of leisure, and not a little reduced in his pecuniary resources by an inconsiderately expensive entertainment which he had given to his friends previously to leaving Edinburgh, he bent his steps homeward, and arrived in London towards the end of Autumn 1788, improved in health,

professional knowledge, and general information. In his memoranda are the following allusions to this period :

"When I returned to London, I found that I had learned much during my absence, and in seeking the sources from which I had derived most information, Drs. Ash, Gregory, and Fyfe seemed specially to claim my gratitude. Big with my own importance, I became presumptuous, but was soon taken down by Newell, afterwards of Cheltenham ; Shrapnell of Berkely : and others in the Physical Society." (Vol. i, p. 181.)

At this time he formed an intimacy with Mr. Colman, who became a pupil of Cline, and this intimacy ripened into a close friendship, which was cemented by the mutual assistance rendered in their studies, and the high distinction subsequently attained by each in his particular department. Their friendship continued uninterrupted till Mr. Colman's death. Sir A. Cooper has left among his papers a brief biography of Mr. Colman, from which some extracts are given in the work before us. These passages are very neatly and appropriately written, and convey a more favorable idea of Sir Astley's literary style than we should have formed from his published works.

From the winter 1788 until the year 1791, no record has been preserved of any particular events in the life of Sir Astley Cooper ; but, at some time during this period, he was appointed demonstrator of anatomy at St. Thomas's Hospital, in the place of Mr. Haighton, who had resigned. He appears to have been fully occupied with the duties of this office and with his professional studies till the year 1791. At this time his great proficiency in his profession, the connexions he had formed with eminent men, his high popularity with the students, and his well-known industry and energy, led Mr. Cline to perceive the advantages that would accrue to the school as well as to his pupil, from associating the latter with himself in the anatomical lectures. It was accordingly arranged that Astley Cooper should give a part of the lectures and demonstrations, Mr. Cline promising him the sum of £120 per annum, to be increased £20 annually, until he gave one half of the lectures, when the proceeds were to be equally divided. It had been customary in these lectures, to unite the subjects of anatomy and surgery, but Astley Cooper, who had for some time seen the necessity of devoting to each a separate course, took the present opportunity of making a proposal to that effect, which, however, at first met with opposition even from his friend and perceptor Mr. Cline. But at last he carried his point, and it was agreed that he should himself take the surgical department, and deliver his lectures in the evening at St. Thomas's hospital. There cannot be a stronger proof of the estimation in which he was held than that so young a man, and still merely a pupil, should have been able to bring about so important a change in an institution by no means unduly addicted to innovation. Mr. Cline had a motive in addition to his high opinion of his pupil, for wishing to advance him rapidly in his profession. Astley Cooper was under a matrimonial engagement to Miss Cock, the daughter of a friend and family connexion of Mr. Cline. Her father had made a considerable fortune as a Hamburg merchant, and having for some years retired from business, had taken up his abode at Tottenham. The marriage, which had been deferred by desire of the parents of both parties, till the termination of Cooper's pupilage, was to have been cele-

brated on the 21st of November, 1791, but was postponed on account of the death of Mr. Cock, which happened on that very day. Within a year from that time, however, the wedding took place, but was conducted with perfect privacy. On the evening of the same day on which the ceremony was performed, Mr. Cooper met his class as usual, delivering his lecture with his wonted ease and freedom of manner, and the pupils dispersed without the smallest suspicion of the change which had taken place in their preceptor's state. The new-married couple took up their abode at a house in Jefferies Square, which Mr. Cock, promising himself the happiness of seeing his daughter surrounded with every comfort, had, but a short time before his death, purchased and furnished for them.

Soon after the conclusion of the winter session, Mr. Cooper and his wife visited Paris, where the former anticipated much gratification and improvement, from a comparison of the French with the English surgery. His peculiar opinions, also, made him regard with interest the great political crisis of which Paris was then the scene. The frightful events of the revolution, which passed before his eyes, soon converted his feelings into those of disgust and horror, which were scarcely less excited by the fiendish expression and gestures of the democratic leaders in the National Assembly, than by the wholesale murders committed at their instigation. The alarm of his wife, and the peril and uncertainty of their situation, conspired with his own feelings to determine him immediately to return to England. A difficulty, however, of obtaining passports, protracted their stay till the middle of September, when they arrived in England in safety. The events which he had witnessed appear to have produced no change in the political tenets of Astley Cooper, but he had probably learned more from them than he liked to acknowledge, as his biographer tells us that he had always a repugnance to making any mention of the views he had entertained at this period. During his stay in Paris, he had attended the hospitals, and heard the surgical lectures of Desault and Chopart, but does not seem to have been greatly struck with the abilities of either of these celebrated surgeons. The latter he merely mentions in his notes as "a good kind of old woman, of little firmness of character." The notes, here published, in connexion with the Parisian expedition are so poor that they are not worth transcribing.

On his return to London, he began to give gratuitous advice at his own house early in the morning, which he seems to have done merely with a view of familiarising himself with disease in all its forms. At this period he was not very solicitious for private practice, his time being much occupied at the hospital. Fortunately, his circumstances in life emancipated him from all pecuniary anxieties, for, in addition to the income he derived from his lectures, he received, with his wife, fourteen thousand pounds.

His surgical lectures engaged much of his attention, and caused him considerable anxiety. He was disappointed to find that the number of pupils at his second course was less by twenty-five than it had been at his first. He had hitherto adopted the recently divulged opinions of John Hunter as the basis of his lectures; but it now struck him "that however just his first views had been in teaching surgery to his pupils upon the sound principles of John Hunter, still it was necessary, not

only that his auditors should have a previous understanding of the meaning of John Hunter, but that they should have advanced to a considerable extent in the general knowledge of surgery, in order for them to comprehend the application of his theory to their practice. He therefore determined to change his plan; nor had he to go far a field to discover another which promised to be not only popular in itself, but more readily to secure the ultimate object he had in view, of so combining the doctrines of Hunter with the science of surgery, that they only should regulate its practice. For this purpose he selected the cases of disease, and the casualties admitted into the two hospitals, and, bringing such of them before the notice of the pupils as would illustrate the subject on which he was treating, he first pointed out to them the nature of the disease or accident, described the appropriate treatment, and afterwards inculcated the theoretical views which indicated it."

From this moment his class increased, and he became a highly popular lecturer on surgery. There was probably another reason why this change in the character of his lectures proved so beneficial, namely that he was not formed by nature for a framer or expositor of doctrines of any kind. His mind was of a strictly practical cast, and his chief endowments consisted in an extraordinary quickness and accuracy of perception, and a wonderfully retentive memory for facts.

At this time few of the surgeons in extensive practice had any confidence in the professional knowledge of John Hunter, whom they considered as a mere speculator; so unwilling is the world to do homage to living genius, while mere talent, if exercised in a popular direction, usually obtains more than its due meed of applause. But true genius has its bright reversion in after time, while mere cleverness enjoys only an ephemeral ascendancy.

In the year 1793, Mr. Cooper was appointed professor of anatomy to the College of Surgeons, and was reappointed in the two succeeding years. In November 1793, Mrs. Cooper was delivered of a female child which was christened Anna Maria. The bad health of this child proved a source of distress, and her death, in March 1794, a severe shock to both her parents. They afterwards adopted as their daughter, a child whom Mrs. Cooper accidentally met with at Hornsey.

In the year 1796, Astley Cooper accompanied Mr. Gawler to Hamburg, to act as medical attendant to that gentleman on the occasion of his duel with Lord Valentia. During the voyage homeward, they encountered a severe gale, and Astley Cooper afforded an example of the fact that the boldest have their moments of fear, and that the same mind which despises danger in one shape may cower before it in another. From the combined effects of terror and sea-sickness, he actually became delirious; and he was often afterwards heard to say "not the riches of the Indies should ever again induce me to make a longer voyage than from Dover to Calais." In all his subsequent visits to the continent, he invariably went by way of Calais.

Towards the end of the year 1797, he left Jefferies Square, and took up his residence at 12, St. Mary Axe, in the house which had been occupied for a long time by Mr. Cline, who, on removing into Lincoln's-Inn-Fields, strongly advised his young colleague to take possession of his late residence. At this time Astley Cooper being no longer desirous

of retaining the office of demonstrator at St. Thomas's, and having a high opinion of Mr. Saunders, afterwards so eminent as an ophthalmic surgeon, he got that gentleman appointed in his stead. In 1798 Astley Cooper sustained a severe concussion of the brain, in consequence of a fall from his horse, and was confined to his room for five or six weeks. In the course of this illness a conversation occurred between Cooper and Cline, who was in attendance on him, which exemplifies the professional enthusiasm of the one and the philosophic *sang froid* of the other. Cooper, in the full persuasion that he should die, was lamenting the approaching event, not so much on his own account, as because it would arrest a train of professional inquiry in which he was then engaged, and which he thought would prove of the highest public benefit. "Make yourself quite easy, my friend," rejoined Cline, "the result of your disorder, whether fatal or otherwise, will not be thought of the least consequence by mankind."

About this time a grand discussion took place in the Physical Society of Guy's Hospital, between Astley Cooper and Abernethy. These distinguished surgeons held very different opinions on the treatment of injuries of the head; and the former invited the latter, by a sort of challenge, to a public discussion. An evening was accordingly fixed, and the theatre was crowded to the ceiling by the pupils of the rival professors. The debate was sustained with great spirit by both champions and their adherents; and, having been twice adjourned, terminated as arguments generally do, namely, by each party being more than ever convinced of the truth of their own opinion.

In the same year Astley Cooper became joint editor, with Drs. Haighton and Babington, of "Medical Records and Researches," which were the transactions of a private society connected with St. Thomas's hospital. Of thirteen papers contained in this volume, two were original essays by Cooper. He became a member of another society which was formed in the early part of the year 1800, which seems to have enjoyed a high reputation in its day, namely, the Edinburgh Club; so called because it consisted exclusively of gentlemen who had, at one period or other, studied at Edinburgh. Many eminent London practitioners were members of it, and all medical foreigners of distinction used to visit it. The discussions were carried on in a free and colloquial manner, without even the formality of any one taking the chair. The attention of the members, however, was entirely devoted to business; no cards or other species of amusement being allowed. A singular feature in the discipline of this society was, that the only stimulating beverage allowed to be drunk at its meetings was cold punch.

It need scarcely be intimated that a principal object of Cooper's ambition at this time was to become an hospital surgeon; and he was in all probability aware of his uncle's intention of retiring, in a short time, from Guy's. Mr. William Cooper, however, seems to have done nothing to favour the appointment of his nephew as his successor: on the contrary, he is supposed to have exerted his influence in behalf of another candidate. He seems to have been in some degree envious of the great fame and popularity of his nephew, and jealous also of the greater respect shown by the latter for the professional attainments of Mr. Cline, and his preference for St. Thomas's hospital. In the year

1800 Mr. W. Cooper resigned his office, and Astley Cooper started as a candidate, being opposed by three others,—Mr. Buckle, an army-surgeon; Mr. Whitfield, a brother of the apothecary of St. Thomas's hospital; and Mr. Norris, who, though entirely unconnected with the hospital, was Cooper's most formidable opponent, owing to the strong and influential exertions of Mr. Warner in his favour.

Astley Cooper stood on the strong ground of his own merits, and his services to the institution. The treasurer seems to have regarded him as professionally best fitted for the office; and he had on several occasions intimated with reference to this subject, the gratification he should derive from a change in Cooper's political opinions and associates: what these had to do with his qualifications as surgeon to Guy's hospital must have been more apparent to the treasurer than it is to us. On Cooper's political tenets, however, which, as we have seen, were sufficiently extravagant, his opponents rested their chief hopes of success; and during the canvass, the treasurer received an anonymous note deprecating, in the strongest terms, the election of a man holding such opinions. Mr. Harrison was much embarrassed as to the course which he ought to pursue, and, sending for Astley Cooper, explained to him the awkward position in which he had placed himself and his friends by his peculiar political relations.

We are sorry that our limits will not permit us to subjoin Cooper's answer in his own words, as communicated by Mr. Harrison to Mr. Bransby Cooper. The substance of it is that, in a late conversation between Mr. Colman and himself, they had come to the mutual conclusion that their political views were at once subversive of their peace of mind, and injurious to their success in life, and hence that they had resolved to discard them. The real moral philosophy of this change of sentiment we leave to the investigation of those who are profound in ethics; but, from the fact of no such abjuration having been made in Mr. Cooper's conversation with the treasurer a short time before, we surmise that this remarkable mutation was in some mysterious manner connected with the event of the pending election, and think on the whole that the less that is said about it the better.

As soon as this change in Cooper's political opinions was made known, and he had obtained the sanction of the treasurer, the tide immediately turned in his favour, and he was elected in October, 1800.

A short time previous to this event, Mr. Travers had been articled to him as a pupil, and resided in his house for some years. Mr. B. Cooper has inserted in his memoir a letter from Mr. Travers, illustrative of the character and habits of Astley Cooper at the period of his early acquaintance with him. This is on the whole the best written letter in the book.

Just prior also to his appointment to Guy's, Mr. Cooper had an accession to his household in his god-son Astley, the present Baronet and brother of Bransby Cooper. He had been prevented by the accident before referred to from attending the christening of the child, and he now saw him for the first time while on a visit to his father, the Rev. Lovick Cooper, who was resident at Yarmouth. He took a fancy to him, and, with his father's consent adopted him. On his return to London he brought the child with him, and presented him to his wife who, ever after bestowed upon him the care of a mother.

A domestic occurrence of no small importance to Astley Cooper happened this year, in the arrival of his celebrated servant Charles Osbaldiston, whose inconveniently long name was soon abbreviated into that of Balderson, which has been retained by him ever since.

By a rare combination of fidelity, shrewdness, and finesse, this person identified his master's interest with his own, and improved every opportunity with so much zeal and sagacity that he contributed no little to extend Mr. Cooper's practice, while he eventually raised himself to a substantial and respectable position in society. Mr. B. Cooper says he has heard him boast that, during the twenty-six years he was Sir Astley's professional servant, he never let but one case escape him in which it was possible to get his master called in.

A less laudable occupation of Mr. Balderson was that of procuring dogs for his master's experiments, and here the humanity either of master or man does not show to much advantage. We read with infinite delight of the escape of a friendly and confiding dog from the clutches of Michael the coachman. Sir Astley's relations to the canine species seem to have been by no means amiable, for we find him during his pupilage perpetrating, in conjunction with Mr. Holland, a most remorseless and inhospitable canicide on a poor destitute cur who followed them home in the hope of protection and kindness.

In reading the account of these way-layings and kidnappings of unhappy dogs, we are almost tempted to wish, for the sake of their tormentors, some of them had proved to be of the kith and kin of that tremendous "Pudel," who was trapped in the study of another great maker of experiments, Dr. Faust:

Aber was muss ich sehen!
Wie wird mein Pudel lang und breit!
Er hebt sich mit Gewalt,
Das ist nicht ein Hundes Gestalt;
Welch ein Gespenst bracht' ich in's Haus;
Schon sieht er wie ein Nilpferd aus,
Mit feurigen Augen, schrecklichem Gebiss.

In furtherance of his anatomical pursuits, Mr. Cooper made an arrangement with the people connected with the menagerie at the Tower, to send to his house the bodies of all their dead animals. He also contracted with Mr. Halls, a stuffer of birds and other animals in the City Road, to purchase all his carcases with the exception of the skin. His supplies did not end here. All the fish and poultry markets in the city were laid under contribution, and Charles was a constant frequenter of Billingsgate, for the purpose of selecting such productions, as seemed better adapted to the scalpel than to the palate. All such specimens were put aside by the fishmongers for Astley Cooper, and if Charles happened not to make his appearance for a day or two, were not unfrequently sent to St. Mary Axe.

Astley Cooper's pursuits necessarily involved him in extensive transactions with the resurrection men of his day, and Mr. B. Cooper has thought it expedient to give not only a detailed history of these transactions, but also biographical sketches of all the principal gentlemen engaged in the reputable branch of commerce alluded to. Now this we think highly injudicious. We do not mean to deny that a very enter-

taining book might be written on the lives and fortunes of resurrection men; but we object to their introduction in the work before us, as being just as much out of place as would be the lives of the persons whose bodies were exhumed.

Had this been a scientific and professional life of Sir Astley Cooper, its chief interest would commence at the period at which we have now arrived; but as it is merely a private life, we find less incident, and consequently less to attract attention now than previously, and Mr. B. Cooper's second volume will demand a less detailed notice than the first.

We now find Astley Cooper fairly settled in life, occupying a very distinguished place in his profession, and regularly established in practice. In the year 1800, he laid before the Royal Society his views with respect to the influence on audition, of perforation of the membrana tympani, and, in the following year, a second paper entitled "Further observations on the effects which take place from the destruction of the membrana tympani; with an account of an operation for the removal of a particular species of deafness." Of the nature of this operation our readers are doubtless aware, and we need not dwell upon it, since Mr. Cooper's first case was the only one which was permanently successful; and although the operation has been frequently repeated with alleged success both in this country and on the continent, the fact of its having fallen into complete disuse seems to indicate its general inefficiency. For these two papers, however, the Royal Society awarded to Astley Cooper the Copleian medal, which is considered the highest honour they have to bestow, and which, fifteen years before had been awarded to John Hunter for certain researches in comparative anatomy.

In February 1805, Astley Cooper was elected a Fellow of the Royal Society. In the course of the same year he cooperated actively with Drs. Marcet and Yelloly in the formation of the Medico-chirurgical Society, which at first consisted chiefly of members who had seceded from the Medical Society in Bolt court, on account of a quarrel regarding the presidency. In the first volume of the Transactions of the Medico-chirurgical Society is an account of an operation for carotid aneurism by Astley Cooper, who had the honour of being the first surgeon who applied a ligature to the artery as a means of curing that disease. The patient died, but the circumstances of the case showed the applicability of the operation, and warranted future trials. A few years after he had the gratification of laying before the same society a case in which the operation was perfectly successful. He continued for some years occasionally to furnish papers to the society, and was also engaged in the preparation of his magnificent work on Hernia, to which disease he had himself been subject from a very early period of life.

Early in the year 1806, Mr. Cooper left St. Mary Axe, and moved into Broad street.

His work on Hernia appeared in two parts, the first in 1804 and the second in 1807. It at once established its author's reputation as an anatomist and surgeon of the first order. It was far, however, from being a source of pecuniary emolument to him. The plates accompanying it were got up in a needlessly expensive style, and when every copy of the work had been disposed of, Cooper found himself a loser by it to the

amount of more than a thousand pounds. During the progress of this undertaking, he met with the zealous cooperation of his professional friends, who did not fail to supply him with all the materials relating to his subject which their own practice afforded. This work extended the fame of Astley Cooper all over the kingdom, and caused a great accession to his consulting practice. Shortly after its appearance, it was the occasion of an interview between himself and Mr. Hey of Leeds, whose name was also intimately associated with the subject of hernia. Cooper's views respecting a certain point in the anatomy of femoral hernia differed so essentially from those of Mr. Hey, that the latter commenced a correspondence on the subject, in consequence of which it was arranged that Mr. Hey should come to town, and that they should institute a conjoint examination of the point at issue. A day being appointed, Mr. Hey called in Broad street in the afternoon, and after he had inspected Cooper's preparations, they commenced their investigations on a subject which had been procured for the purpose. So interested were both in their occupation, that they continued it almost without intermission till between two and three o'clock in the morning, to the great discontent of Charles, who was candle-bearer on the occasion. The inquiry, however, terminated to the perfect satisfaction of the parties more immediately interested, and Mr. Hey left town fully convinced of the accuracy of Cooper's description.

Astley Cooper having recommended that his nephew Bransby, his present biographer, should be brought up to the profession of surgery, as that in which he should have most opportunity of being of service to him, this advice was acted upon, and young Bransby was placed under the care of Mr. Colman, one of the surgeons of the Norwich hospital, in order that he might acquire the rudiments of surgery and pharmacy previously to entering as a pupil at one of the London hospitals. Astley Cooper still continued to pay his visits to Norfolk, during one of which an incident occurred which was very characteristic of that unobtrusive kindness towards his professional brethren which made him so deservedly beloved by them. The reader will find it in Vol. ii, p. 67.

Astley Cooper was resolved not to sacrifice his nephew's true interests to any feeling of personal kindness. He told him plainly that he need not expect his support in the profession unless he deserved it; and Bransby having come to town some time before the commencement of the winter session, according to his account to "acquire a knowledge of the preliminary studies incident to a student's occupations in London," but in reality because he was tired of staying at Norwich, Astley Cooper entirely disagreed with his junior relative as to the necessity of such preliminary measures, which he perceived to mean simply a few weeks' idleness, and accordingly he started Bransby back to Norfolk the same evening, giving him a number of cases to transcribe for him during the vacant period before the sessions commenced. When the time arrived for his return his uncle sent him to live with Mr. Hodgson, now of Birmingham, and celebrated for his work on Diseases of the Arteries and Veins.

At this time Astley Cooper was lecturing on surgery assisted by Mr. Henry Cline, while Mr. Joseph Henry Green, Mr. Cline's nephew, held the office of demonstrator. Cooper's surgical class was larger at this time than any other in Great Britain, and perhaps the largest that he

himself ever had. His practice too was now immense, and his time was most fully and laboriously occupied.

Mr. B. Cooper gives an entertaining account of his morning consultations, and of the dexterous manner in which Charles managed the concourse of patients. (Vol. ii, pp. 72-7.) The occupation of his time in hospital and private practice, as well as the great extent of his scientific pursuits obliged Mr. Cooper to engage persons to assist him in his researches. In the anatomical department he employed Mr. Lewis, who was the only person except himself who was allowed to enter his private dissecting rooms which were over the stables at the back of his house, and were fitted up with every convenience. Here Mr. Lewis usually commenced his day's labour at six in the morning, and was frequently occupied, with certain interruptions, till eleven at night. Early as was the hour of his arrival, he generally found Astley Cooper already at work; but, if not, there was always something on the table ready for him to proceed with, if the task on which he had been previously engaged had been completed over night. To these rooms were brought most of the diseased parts removed by operation or in post-mortem examinations in Mr. Cooper's hospital or private practice. Mr. Cooper was indefatigable in his exertions to eradicate the natural though injurious prejudice against necroscopic inquiries. When Mr. Lewis became connected with him, the task of obtaining permission for such investigations devolved on him, and so experienced did he at length become in the best mode of attaining his object, that from the time he was first employed by Mr. Cooper till he entered on private practice, he did not fail in more than six cases. For anatomical delineations, Astley Cooper first engaged an artist named Kirtland, who possessed good abilities, as evinced by the fact that many of the engravings in the work on Hernia were executed by him. He fell, however, into drunken and irregular habits, and Cooper was obliged to dismiss him. His place was supplied by Mr. Thomson, a miniature painter, whose talents had never been duly appreciated, and who was in poor circumstances. Mr. Cooper sent him to St. Thomas's to attend the anatomical lectures and to dissect, and he was thus rendered an efficient artist for the subjects on which his services were required. This gentleman in the course of his pursuits imbibed a considerable knowledge of surgery, and devoted himself particularly to the study of the eye. He ultimately left England for the West Indies, with a view of practising there as an ophthalmic surgeon, but died on the voyage. Mr. Cooper does not appear to have regularly engaged any other artist after Mr. Thomson left him; subsequently, however, to his removal from Broad street he secured the permanent assistance of Mr. Canton. In the formation of casts and models he had Mr. Lewis instructed by an artist named Schianelli, and he became very useful to him in this department. If in the course of the morning a patient called having a disease of any interest, to which this mode of copying might be applied, Mr. Lewis, having obtained his consent, would in a short time make a model of the diseased parts. Requests of this nature were very seldom refused by patients. Lewis appears to have been a man of great industry and varied capacity, for, in addition to his other occupations, he acted as Mr. Cooper's amanuensis.

In the manner above described, Astley Cooper's collection, which,

when he first went to Broad street, consisted of only twenty preparations, soon became very extensive and valuable. In the winter of 1808 or 1809, he met with a slight accident; walking in the evening along Cannon street he slipped from the curb-stone and fell, and one of his feet becoming fixed in some ice, the fibula was in consequence fractured. This accident, however, did not long detain him from his practice. While in Broad street, Mr. Cooper gave frequent soirées, which were attended chiefly by the physicians and general practitioners whom he was in the habit of meeting, or his dressers and favorite pupils at the hospital. He also made it a rule, once a week, to invite certain of these professional acquaintances to dine with him. He was a member of several clubs, the meetings of which he frequently attended; as that of the Athletæ, the Kent Medical Club, and the Pow-wow. The Athletæ, who were embodied, as their name imports, for the cultivation of active exercises, consisted chiefly, though not entirely, of medical men. The late Dr. Babington, who greatly excelled in athletic feats, was one of their most active members. The Athletæ seem to have been very funny and good-humoured fellows, but we have not space to detail their recreations. The origin of the Pow-wow club is disputed, some referring it to John Hunter, and others to his intimate friend Dr. Patrick Russell. The word Pow-wow in some of the eastern languages signifies a meeting of conjurers, and appears to have been adopted by the club as a caprice. However these things may be, the Pow-wow seems to have been a source of much gratification to some of the most distinguished men in the profession. It was dissolved in 1830.

In the early part of the year 1806, Mr. Cooper evinced his acuteness in detecting the murderer of Mr. Blight of Deptford, whose death occasioned so great a sensation at the time. He was called in to Mr. Blight, whom he found in a hopeless condition, from a wound inflicted with a pistol by an unknown hand. Circumstances led him to suspect Mr. Patch, the partner of Mr. Blight, and on investigation of all the facts connected with the discharge of the pistol, he became convinced that it must have been fired by a person who was left-handed. On learning that Patch was left-handed, he entertained no further doubt on the subject, and on his return home said to his servant, in secrecy, "you will see, Charles, that Mr. Patch, the partner of Mr. Blight, has been his murderer." And so it turned out, though no one but Cooper entertained any suspicion of the truth until the inquest on the body of Mr. Blight, when, strong evidence appearing against Patch, he was committed for trial, and subsequently condemned and executed.

Some years afterwards, Astley Cooper displayed the quickness of his intuitive powers in the detection of another remarkable murderer. Mr. and Mrs. Thomson Bonar had their town-house in Broad-street, near that of Mr. Cooper, and were on intimate terms with him. One morning Mr. Cooper was horrified at the arrival of Nicholson, the servant of Mr. Bonar, who came on horseback from that gentleman's country residence at Chiselhurst, to say that Mr. Bonar had been murdered during the night, and that Mrs. Bonar was in a very dangerous state from the wounds which she also had received from the assassin. Mr. Cooper was at once convinced, by the appearance and manner of the messenger, that he was himself the murderer, and when setting off for Chiselhurst, he

desired Mr. Frederick Tyrrell, then his pupil, to stay in town and look after Nicholson. The truth of his suspicions must be known to all our readers who can recollect the case.

At the period of which we are speaking, many, if not most, of the principal merchants in London had residences in the city. This had an immense influence on Astley Cooper's income; for although, perhaps, he might not see so many patients in a day as after his removal to Spring-Gardens, the remuneration he received was much greater. The merchants generally gave him a cheque instead of a fee, and no one wrote for less than five guineas, though had the payment been made in cash it would have been only two. When sent for out of town, he was also remunerated with extraordinary liberality. Mr. William Coles, of Mincing Lane, for years paid him £600 per annum for visits to his seat near Croydon. In the year 1813 he performed the operation for stone on Mr. Hyatt, a West India merchant, and received from him probably the largest fee ever given by a private individual for a surgical operation.

"The manner in which the fee was presented was not, perhaps, the least extraordinary part of the circumstance........His patient, who was sitting by the fire, took off his night-cap and jocularly threw it at him, saying at the same time, 'there, young man, put that into your pocket.' Mr. Cooper, guessing at the contents of the missile, took from it a piece of paper, and, tossing the cap back to his patient, took his leave. The paper was a cheque for a thousand guineas."

While Astley Cooper maintained these agreeable and profitable relations to the public, he was also particularly happy in the terms on which he stood with his own profession.

"It appeared," says his biographer, "as if he had by some magic gained the confidence of every medical practitioner who had access to him; and this insured an extension of his fame over a very large portion of England. This influence did not arise from his published works, nor from his being a lecturer, nor indeed from any public situation which he held, although each of these circumstances had its share in producing the result; but it seemed to originate more from his innate love of his profession, his extreme zeal in all that concerned it, and his honest desire, as well as great power, to communicate his knowledge to another, without, at the same time, exposing the ignorance of his listener on the subject even to himself." (Vol. ii. pp. 162-3).

All this is strictly just, and in no respect more so than in reference to his power of enlightening other practitioners on points of which they were ignorant, without, in the smallest degree, wounding their self-love. This was one of the most amiable as well as useful traits in the professional character of Sir Astley Cooper.

At this time the gullibility of the public was severely taxed by a most amusing set of scamps, termed by those who understood their manœuvres, the "Ashley Cooper Set." They were a junta of advertising quacks, who held solemn sessions in robes and wigs, presided over by a personage styled "Doctor Ashley Cooper." We have not space to record their proceedings; but the reader will find in Mr. B. Cooper's work, an amusing account of these, as well as of some other impostors who founded their schemes on the fame of Astley Cooper.

In the year 1811 Mr. Cooper purchased the estate of Gadesbridge, at Hemel Hempsted, in which he does not appear to have made a very good bargain; though both he and his wife were pleased with their new acquisition.

In May, 1813, Mr. Cooper was appointed Professor of Comparative Anatomy to the College of Surgeons. The immense labour entailed by this appointment would have disinclined him from accepting it; but as there was no one at that time connected with the college willing, or perhaps very competent, to undertake the task, he considered it a duty on his own part to do so. On accepting the appointment, Mr. Cooper devoted himself with the utmost zeal to the preparation of his lectures. His engagements would not allow him to study the works of the various authors who had written on the subject, but he gave up all his leisure time to the practical investigation of it by dissection, and even curtailed the period of his nightly rest, brief as that already was. Mr. Lewis having in the preceding year entered upon practice, Mr. Cooper employed Mr. Parmenter in the same capacity, and found him a most efficient assistant. While these lectures were in course of delivery, the death of Mrs. Parmenter, the adopted daughter of Astley Cooper, so disturbed his mind, that he felt himself unequal to sustain such an accumulation of labour, and an abrupt termination was put to the lectures.

In the same year he removed from Broad street to New street, Spring Gardens. One motive which induced him to take this step was, that he might be in the near vicinity of certain connexions which he was forming about the court, to be attached to which had now become an object of his ambition. But his chief motive was to escape from the excessive fatigue of his immense city practice, the extent of which may be inferred from the amount of his annual income, which, in the year 1815, exceeded twenty-one thousand pounds.* His multifarious labours were beginning to tell on his health. His spirits and his appetite did not fail; but, from privation of exercise, he had become corpulent, and his naturally ruddy complexion assumed now and then a bluish tint when he used exertion. He was subject also to attacks of vertigo; and one morning when the Duke of Manchester had called to consult him, he was seized with one of these attacks and fell prostrate. His subsequent demeanour on this occasion was very characteristic of the care and caution so conspicuous in all his professional doings. On recoveriug himself, his first words conveyed an earnest request to the Duke that he would never speak to any one of what he had just witnessed, a request with which his Grace strictly complied. This circumstance was so closely concealed during the life of Sir Astley, as to have been known only to the Duke of Manchester, Lady Cooper, Mr. Bransby Cooper, and the trusty servant Charles.

In May, 1816, according to our biographer, Astley Cooper performed his celebrated operation of placing a ligature on the aorta. But Mr. B. Cooper is here at fault in his chronology: the operation in question was performed on the 25th of June, 1817. This was, beyond all comparison, the boldest enterprise in the annals of operative surgery. When it became known to the profession it excited general amazement, and some did not hesitate publicly to express their doubt whether any circumstances of danger could justify so fearful an expedient. It would be dissonant from the character of the present article to discuss the merits of this operation, which is the less needful because it is now admitted to have

* This sum includes all monies paid into his banker's hands on his account; his actual *fees* for one year *could* not have amounted to this vast sum.

been wisely and advisedly undertaken, and to have been so borne out by the circumstances of that case, and of others in which the operation has been repeated, as to justify its repetition and afford a hope of eventual success.

In the year 1818, Mr. Cooper and his distinguished pupil, Mr. Travers, who was now one of the surgeons of St. Thomas's Hospital, published conjointly the first volume of their Surgical Essays. The very favorable reception of this led to the publication of a second part, as soon as circumstances would permit; the work nevertheless was not continued, owing, as Mr. B. Cooper thinks, to the conviction of Astley Cooper that the publication of these essays in volumes appearing at distant intervals would be less useful than the separate exposition in complete treatises of the important subjects to which they related.

In the year 1820, Astley Cooper lost a much valued friend by the death of Dr. Marcet, of whom a biographical sketch is given. His history is curious, but we have not space to advert to it.

From this period Mr. Cooper was in frequent attendance on Lord Liverpool until the death of that nobleman. They were on terms of intimate friendship, and appear to have entertained a very high opinion of each other. Mr. B. Cooper has given some anecdotes of his lordship from the papers of Sir Astley, which are worthy of perusal.

In the same year, 1820, Mr. Cooper was called into attendance on George the Fourth, although, at that time, he held no official situation about his majesty's person. He seems to have been sent for merely on the strength of his great professional reputation, which had reached the ears of the king. In the spring of 1821 he removed a steatomatous tumour from the head of the royal patient, in the presence of Sir H. Halford, Sir M. Tierney, Sir Everard Home, Mr. Cline, and Sir B. Brodie. The length to which this article has already run, prevents us from noticing the details of his attendance on the king, and the anecdotes connected with it: most of these, indeed, have come repeatedly before the public through other channels.

The observations found in Sir Astley's notes relative to George the Fourth are extremely flattering to that monarch, but give far too high an estimate of his knowledge and attainments. They seem written too much in the spirit of a courtier, and rather indicate that Sir Astley had passed into an extreme of political sentiment, opposite to that into which he had fallen in his youth.

It might be thought that the strong disinclination of Mr. Cooper to undertake the operation on the king, and his attempt to devolve it on another, savour of timidity, selfishness, and littleness of mind. The following is his own account of his feelings:

"I called upon Lord Liverpool, and requested him to persuade the king to let Home do the operation, as that was the usual etiquette, he being sergeant-surgeon. Lord Liverpool said that it was very difficult to interfere respecting the choice of a medical man. I was very averse from doing it; I had always been successful, and I saw that the operation, if it were followed by erysipelas, would destroy all my happiness and blast my reputation.........Soon afterwards Sir Henry Halford was called out of the room, and almost immediately returning, said to me, 'you are to do the operation.' I was thunderstruck, and felt giddy at the idea of my fate hanging upon such an event." (Vol. ii. p. 229.)

Sir Astley has here omitted to mention what abundantly exculpates him: he has however mentioned it in another place. He was still subject to those attacks of vertigo, one of which overwhelmed him in the presence of the Duke of Manchester, and he observes, "this state of my nerves made me dread the operation upon George the Fourth, as I could not help fearing that I might be similarly seized at that moment."

It was not the moral man that was to blame; for nothing annihilates confidence in oneself so completely as the apprehension of any sudden seizure which suspends the command over one's faculties: moral resolution is based on the full consciousness of such command, and when this is no longer felt the firmest resolution will waver.

Shortly after the operation, the king made Mr. Cooper a Baronet, and, in compliance with his request, entailed the title upon his nephew and adopted son, Astley.

Sir Astley afterwards received as a present from his majesty a beautiful epergne, which cost five hundred guineas, and for which the king gave the plan himself.

Two days after Astley Cooper's elevation to the baronetcy, his heir was married to Miss Rickford, the only child of William Rickford, Esq. M.P. for Aylesbury. This marriage gave Sir Astley the greatest satisfaction, and he always showed the kindness of a parent to the young couple.

In the year 1822, Sir Astley Cooper was elected one of the Court of Examiners of the College of Surgeons. His conduct in this capacity was characterized by remarkable kindness towards the candidates.

"This was not only evinced by his expressions of encouragement, but equally so by his kind of examination, for he never asked catch-questions, or made abstract inquiries; but invariably dwelt upon practical matters, worded in simple and straightforward language. It was always a most painful task to him when it became his duty, as president, to reject candidates, and he mingled on these occasions with his terms of disapprobation, such friendly and even paternal advice, that he always succeeded in lessening the pangs of disappointment, and has been known to be the means of thus changing a young man's habits from idleness and dissipation to those of industry and professional study. ....I have frequently known him, when pupils have been rejected at the College of Surgeons, write, unsolicited, to explain to their parents the circumstances under which they had been sent back, so as to mitigate their regret, and prove to them that a few weeks' more study would remove the obstacle which had prevented the admission of the young man as a member of the college." (Vol. ii. pp. 245-6.)

In the same year he brought out his great work on Dislocations and Fractures of the Joints, a considerable portion of the substance of which had already appeared in the Surgical Essays. It is needless to say that this work invested the name of Astley Cooper with new honours, and was executed in the masterly style which is common to all his surgical writings. Towards the latter end of the year 1824, his attacks of giddiness became more frequent, and were occasionally attended with difficulty of breathing, from which symptoms, however, he was invariably relieved by a few days' retirement into the country. Being somewhat alarmed at the frequent recurrence of these attacks, and attributing them in a great measure to the exertion of lecturing, he limited himself, from the commencement of this season, to the delivery of the surgical lectures. After Christmas, however, he resolved to give up lecturing entirely, and

sent in his resignation to a general meeting of the governors of the hospital in January, 1825. For some time before his resignation Mr. Key had taken a part in the surgical lectures, and Mr. Bransby Cooper had officiated instead of Sir Astley in the anatomical course. As he was satisfied with the manner in which these gentlemen had fulfilled their duties, he wished not to retire without an understanding that they would be appointed as his successors, and it was under the conviction that this condition had been acceded to by all parties that he tendered his resignation. Contrary to his expectations, however, Mr. South was appointed to the anatomical chair, and a feud hence originated which led to the establishment of a school of medicine at Guy's hospital, and the separation of what had hitherto been called the United Borough Hospitals. In connexion with these events, a duel had like to have taken place between Sir Astley and Dr. Cholmondely. The doctor, however, having put himself in the hands of Mr. Power, to do as he thought right for him, and that gentleman being of opinion that he was in the wrong, he made a public apology, and thus the affair ended.

On the establishment of the new school at Guy's, Sir A. Cooper exerted himself in the formation of a museum adequate to illustrate the lectures; for his former preparations, which had been deposited at St. Thomas's, were lost to him in consequence of his secession from that school. As soon as he was established in New Street, he began to accumulate similar preparations, which were immediately taken to Guy's, and being added to those already placed there by Dr. Haighton and others, formed the nucleus of what is now one of the most valuable museums in Europe.

The farming and sporting occupations of Sir Astley at Gadesbridge, of which a sufficiently entertaining account is given, we must entirely pass over. Like most gentlemen farmers, he derived more pleasure than profit from his pursuits.

In 1824, Sir Astley performed the operation of lithotomy on the Vice-chancellor, Sir John Leach, whose fortitude under a severe and protracted operation excited general admiration.

In October, 1826, he was called into consultation on the case of the Duke of York, and continued to attend him till his death. Various anecdotes of his Royal Highness are given from Sir Astley's note-book.

In January, 1827, Mr. Cline died, and Sir Astley was naturally much affected at the loss of his old friend and preceptor, the more so because circumstances connected with the separation of Guy's and St. Thomas's hospitals had led to a partial estrangement between them.

In 1827, Sir Astley Cooper was elected President of the Royal College of Surgeons; an honour which was again conferred on him in 1836. In the month of June, in the same year, he lost his wife, who died at Gadesbridge, of erysipelas of the head. The grief occasioned by this event increased the frequency of Sir Astley's attacks of vertigo, and he was often obliged to protract his visits to the country longer than he had proposed. This continual interruption of his professional pursuits so annoyed him, that, in September of this year, he determined to retire from practice, and give himself up to a country life on his estate.

For a short time he was much gratified by the change, and delighted at the novelty of having his time entirely at his command; but, as might

have been anticipated, he soon began to experience an ennui to which he had hitherto been a stranger, and felt the want of that excitement which long habits of professional activity had rendered necessary to his happiness. During his retirement in the country he had an attack of intermittent fever, which had nearly proved fatal. On his recovery he wisely resolved not to allow the fear of being laughed at for so sudden a change in his plans to deter him from resuming the practice of his profession; and, on the re-establishment of his health, he returned to London, after an absence of about six months. At first he took lodgings, intending merely to see a few of his old patients three or four days in the week. His return, however, was no sooner known to the public than patients crowded in upon him; and, his health being restored, he resumed practice with little less than his former zest. He soon afterwards purchased the lease of a house in Dover street, but not finding this tenement suited to him, he disposed of it at a loss, and took the house in Conduit street, in which he remained till his death. His old servant Charles, whom he had placed on a farm called Warner's End, did not return to him.

In July, 1828, Sir Astley again entered into the married state, espousing Miss C. Jones, with whose family he had been acquainted from the earliest period of his residence in London.

In the same month he was appointed sergeant-surgeon to the king.

In 1829, he published the first part of his admirable Illustration of the Diseases of the Breast. In 1830 he was elected vice-president of the Royal Society, being already a member of the council.

Towards the latter end of the summer he made, in company with his wife and another lady, a tour of considerable extent on the continent. Mr. B. Cooper has published Sir Astley's notes of this journey, but, as we think, very injudiciously, as they contain nothing but common-places. In the autumn of 1831, he made a tour over the southern and south-western parts of England. On his way home he purchased at Lyme Regis, of the well-known Mary Anning, a very fine specimen of the ichthyosaurus, a drawing of which afterwards appeared in Dr. Buckland's Bridgewater Treatise.

On the accession of King William the Fourth, his appointment as sergeant-surgeon was confirmed. He had been brought into personal intercourse with the king, when Duke of Clarence, as early as the year 1818, when he attended his Royal Highness's son, then Captain Fitzclarence, who had received a severe compound fracture of the leg, in consequence of a fall from his horse, and who, ever after, retained a grateful sense of Sir Astley's attention to him.

Early in the following year Sir Astley published his work on the Anatomy of the Thymus Gland, which he dedicated to Dr. Babington, whom he very highly esteemed, in common with all his medical brethren, and who shortly after, fell a victim to the influenza then prevalent.

At this time Astley Cooper was elected a member of the Institute of France, and received from the King the rank of officer of the Royal Order of the Legion of Honour. Shortly afterwards he was elected a corresponding member of the Royal Academy of Sciences, a place having become vacant in the section of medicine and surgery, in consequence of the death of M. Delpech.

In June, 1834, on the occasion of the installation of the Duke of Wellington at Oxford, Sir Astley Cooper received from that university the honorary degree of Doctor of Civil Law.

In this year he made another tour with Lady Cooper, and some members of her family, over a considerable part of Germany, and through Switzerland into France. The acquisition of professional knowledge was still one of his principal objects. There was not an hospital in his route that he did not visit; and he not unfrequently made a detour for the express purpose of examining some public museum or private collection illustrative of his favorite pursuits.

In the year 1835, Sir Astley was elected an honorary fellow of the Royal College of Surgeons of Edinburgh, and soon afterwards determined to make a tour to Scotland; he was, however, prevented by various engagements, from accomplishing his object till 1837, in August of which year he arrived in Edinburgh. On the 5th of September a dinner was given him by the College of Surgeons, when sixty gentlemen, Fellows of the College, met to welcome him. On his health being drunk, Sir Astley returned thanks in a long speech, in the course of which he took a review of his past life, comparing his position at that time with his situation and prospects when he had been among them as a student, and stating the circumstances to which he attributed his success.

On the morning of the same day, the degree of LL.D. had been conferred upon him by the university. On the same day also, at a meeting of the magistrates and council, the freedom of the city was voted to Sir Astley, and on the following morning a deputation, consisting of the Lord Provost and a large number of the council, accompanied by many members of the university, arrived at the hotel at which he was staying, and presented him with the customary documents. In the afternoon he set out on a tour to the highlands. On reaching Carlisle in his homeward route, he writes,

> "This has been a delightful and flattering tour in Scotland; delightful from the scenery, the variety of the prospects, the goodness of the roads, and is flattering, from the great civility we received in Edinburgh and Glasgow, and, indeed, wherever we went. It was the reward of a life spent in the industrious pursuit of a profession."

Three years after this tour he was proposed with two other candidates, the Duke of Wellington and the Marquess of Breadalbane, for the office of Lord Rector of the University of Glasgow. The Marquess was elected.

In September, 1835, Mr. Bransby Cooper was alarmed at a false report of Sir Astley's sudden death in the Isle of Wight, which he heard casually in a stage-coach. Having ascertained the inaccuracy of the report, he wrote to his uncle to tell him of the apprehension he had been under. Sir Astley returned an answer which begins as follows:

> "My dear Bransby,—It is with much self-gratification that I assure you that I am not *dead*, and that the only fit I have had is a fit of hunger, to which disease I have been extremely liable ever since I was born. Indeed, it is my full intention to practise my profession for the next thirteen years; after that time to retire for twenty, and then be at God's disposal for as many more as he pleases. I suppose the false report has arisen out of Brodie's accident, whom I saw at Newport very well; but the injury he met with was severe." (Vol. ii. p. 448.)

In the year 1840, Sir Astley published his last work, On the Anatomy of the Breast, a work of transcendent merit, and truly astonishing, when we consider that the author, at the time of its appearance, was upwards of seventy years of age, and that its materials were chiefly derived from researches undertaken in the latter years of his life. During the progress of this work he found time to contribute several papers to the Guy's Hospital Reports, the first volume of which appeared in 1836. These papers contain the results of dissections, experiments, physiological inquiries on various subjects, and numerous surgical observations, and tended greatly to promote the success of the work.

The tone of the letter above quoted does not indicate any consciousness of a failure in the powers of his constitution. He had, however, continued subject to his attacks of vertigo, which latterly had been resolved by fits of gout; and towards the end of 1840, he became afflicted with extreme difficulty of breathing, greatly augmented by any ascent. A short time before Christmas he performed an important operation upon Lady ——. Before he would undertake it, he sent Balderson* to ascertain the number of stairs which led to her bedroom, saying "if there are more than lead up to my own bedroom, I leave it to you to manage that the lady shall be moved to an apartment in the lower story, otherwise I shall not attempt to operate."

At Christmas, Sir Astley and Lady Cooper went to the Rev. Mr. Board's, at Westerham, to pass a fortnight, as they had been in the habit of doing for several years. That gentleman was at once struck with the failure of Sir Astley's health and spirits. When the period fixed for his return to town arrived, he was too ill to leave the country; but anxious to conceal from his friends the real cause of his delay, and the serious nature of his illness, he wrote to town merely saying that he intended to prolong his visit at Westerham for another week. Mr. Bransby Cooper, however, was quite convinced that nothing but serious indisposition could detain him so long from his occupations in Conduit street, and he therefore wrote entreating him to return to town and place himself under medical treatment. A few days afterwards Sir Astley arrived in Conduit street.

"I called and saw him," says Mr. B. Cooper, "and never was I so shocked as when I witnessed the change which one short month had worked upon a frame which had always hitherto indicated health and vigour, and been graced by a countenance expressive at once of intellect and happiness. On entering the room I found him sitting with his head bent upon his chest, and, instead of greeting me in his wonted lively and affectionate manner, he scarcely moved; but, with a half-extended hand and melancholy expression, watched narrowly what impression his altered appearance excited in my mind. I could not conceal my emotion: he observed it, and pressed my hand. The interview was too painful to me to sustain for any lengthened period, and I left the house fully convinced that my poor uncle's days were drawing rapidly to their close. (Vol. ii. p. 452.)

He died on the 12th of February, 1841, in the seventy-third year of his age. For some days previous to his death, he had frequent attacks of delirium, but in the intervals he affectionately recognized his friends, and his frame of mind was that of religious resignation to the will of

* Who, we presume, was on a visit to his old master, as we read of his having quitted his service some years before.

God—for he was perfectly aware of his condition. He had been attended during his last illness, by Drs. Bright and Chambers, and Messrs. Key, Tyrrell, and Bransby Cooper. He derived great comfort from the spiritual ministrations of his nephew, the Rev. Beauchamp Cooper. The remains of Sir Astley Cooper were interred by his own desire, beneath the chapel of Guy's Hospital. A few days after the funeral, a meeting of the elder pupils of the hospital was held, for the purpose of erecting, by subscription, a tablet to his memory in the chapel. This design, however, was abandoned, and, in its stead, a bust, executed by Mr. Town, has been placed in the museum of the hospital. Another bust, to be executed by Mr. Behnes, is to be placed in the council-room of the College of Surgeons. The senior members of the profession at large met at the Freemasons' Tavern, and formed a committee to receive subscriptions for the purpose of erecting some monument to record the services rendered by Sir Astley Cooper to the public; they have since determined on placing a colossal statue of him in St. Paul's Cathedral. This work, which has been intrusted to Mr. Baily, is still in progress.

Sir Astley Cooper established a triennial prize of three hundred pounds for the best original essay on some medical subject, and determined the first six subjects himself. The competition for this prize is open to all members of the profession in this country, or any other, except the medical staff of Guy's and St. Thomas's hospitals, who are appointed the adjudicators of the prize.

The general character of Sir Astley Cooper will be sufficiently collected from the foregoing sketch of his life. We regret much that we are debarred from dilating on his professional character by the nature of the work under review. We may, however, transcribe Sir Astley's opinion of himself contained in some notes on the characters of the principal surgeons of the day:

"Sir Astley Cooper was a good anatomist, but never was a good operator where delicacy was required. He felt too much before he began ever to make a perfect operator. For the operation of cataract he was quite unfitted by nature. Quickness of perception was his forte, for he saw the nature of disease in an instant, and often gave offence by pouncing at once upon his opinion. The same faculty made his prognosis good. He was a good anatomist of morbid, as well as of natural, structure. He had an excellent and useful memory. In judgment he was very inferior to Mr. Cline in all the affairs of life, and hence he was continually walking upon a mine ready to explode under his feet. His imagination was vivid, and always ready to run away with him if he did not control it." (Vol. ii. p. 475.)

Sir Astley, however, here underrates his own powers as an operator. He was not a particularly neat operator, but he was self-possessed, dexterous, and *successful* in a degree seldom equalled. We think, also, he errs with respect to the extent of his imagination. We do not believe he had a particle of imagination in his composition. He probably mistook for it the vivacity of mind arising from his quick and vivid perceptions.

We must not conclude this article without some reference to the merits of Mr. B. Cooper's book. We here repeat the remark with which we set out, namely, that the plan and object of the work are essentially faulty. The author, with the best feelings and intentions, has done injustice to the memory of his illustrious relative by presenting to us the great Sir

Astley Cooper, divested of nine tenths of all that made him great, and, consequently, reduced to very moderate dimensions. We wish we could speak favorably of it in a literary point of view; but this is impossible. It would appear that the task of its composition had been imposed on the author by the will of his late uncle—a will so sacred to his affectionate heart, as to induce him to undertake a labour for which he was very imperfectly fitted by previous habits of study. Had he, indeed, undertaken the *professional* life, the want of which we so much regret, the result, we doubt not, would have been very different, as he would then not only have found himself perfectly at home in his own proper style of composition, but would have been amply qualified by his critical knowledge of all the subjects treated of. Entering abruptly and without preparation on the field of general literature, Mr. Cooper shows manifest unacquaintance with the ordinary modes of proceeding and conventional customs of the cultivators thereof, and even with the common weapons wielded by them. At the same time, it must be admitted that he brings with him a vigorous constitution of mind, and strong and ready mother-wit, which need only the training of experience and the study of good examples, to issue in works of a very different character from the present. Above all, Mr. Cooper evidently possesses the candour, the goodness of heart, the kindliness of nature, and the innate sense of honesty and honour, which form so important elements in the composition of a good writer, and without which, intellectual powers, however great, can never take a permanent hold on the sympathies of men. As it lies before us, however, a sense of justice compels us to say that the work is in many parts incorrectly and feebly written, and that the materials are heterogeneous and ill digested. If it had fallen to the lot of a Greek critic to review this book, its violations of the "unities of time, place, and action," would inevitably have been the death of him. In our analysis of it, we have been obliged now and then to transpose an event over a space of twenty years or so, in order to render the narrative in any degree consecutive. We must observe also, that some passages are in very bad taste, and are much too redolent of the hospital and the dissecting-room. For example, an anecdote is related of a trick practised upon a hair-dresser, by Astley Cooper and Mr. Colman, from which they derived infinite amusement; whereas we perceive in it much nastiness but no fun, and are sorry that it has been published.

With all its faults, however, the work is exceedingly entertaining, and contains a vast store of professional anecdote and gossip which is not to be found elsewhere. Some of the stories are well told, and there is a vein of good humour running through the volumes, which renders them pleasant to read. Although as honest critics, we cannot speak favorably of the literary character of Mr. B. Cooper's book, we nevertheless strongly recommend our readers to peruse it. We ourselves, in fact, having read it once for the purposes of criticism, intend to read it again for the sake of amusement.

ART. VIII.

Chemie und Medicin in ihrem engeren Zusammenwirken, oder Beduetung der neuren Fortschritte der organischen Chemie für erfahrungsmässige und speculative ärztliche Forschung, als vollständige Lehrschrift für die studien der organischen Chemie uberhaupt, insbesondere aber für die im Gebiete der Medicin und Pharmacie, so wie für die Fortschritte der Heilmittellehre. Von Dr. F. L. HUENEFELD. In Zwei Büchern. —*Berlin*, 1841. 8vo, pp. 396, 372.

Chemistry and Medicine in close cooperation; or a view of the latest progress of Organic Chemistry in relation to experimental and speculative medical research, intended as a complete text-book for the study of organic chemistry generally, and particularly in the domain of Medicine and Pharmacy, as also Materia Medica. By Dr. F. L. HUENEFELD, Professor of Chemistry, Pharmacy, and Mineralogy at Gräfswald. In Two Books.—*Berlin*, 1841.

IT was justly remarked by the experienced Rudolphi, that "no mere chemist is competent to be the author of a treatise upon animal physiology."* Indeed we are thoroughly persuaded that those exact habits of research which qualify a man for investigating with success the changes of composition that occur among the constituent parts of different bodies, unfit him for grappling with a power so mysterious and inscrutable as that of life. Thus it is that the philosopher, in professing to account for vital processes on purely chemical principles, has tended rather to perplex than elucidate the subject. We are not prepared to deny that chemistry may materially aid, to a certain extent, in explaining various organic phenomena, but its sphere is limited. Were it otherwise, the laws of chemistry would be the laws of life.

It is no easy task to review the work before us, as it is extremely diffuse, and in some places contradictory. To have done ample justice to the wide field which the author embraces might have well occupied at least half a dozen such volumes. Hence the work is neither well suited for elementary study nor for reference. The first volume is little more than a *catalogue raisonnée* of vegetable and animal substances. It commences with a brief historical notice of the early iatro-chemical doctrines and of the action of remedies. The author then goes on to discuss the present state of organic chemistry; the *general* conditions and relations of the various substances, as their aggregate form, specific gravity, sensible properties, microscopic peculiarities, spontaneous resolution, modes of decay, and the like; next their *specific* relations as regards water, acids, alkalies, bases, metals, salts, and so on.

He ushers in his second volume with some remarks concerning the physico-chemical endowments of the organism, more especially elasticity and capillarity. In pointing out the composition of the arterial texture, allusion is made to the circumstance of the lining membrane being composed of the fibrinous protein, and hence not liable to be acted upon by weak alkaline solutions, since had it consisted of the albuminous protein, it could not have withstood for any length of time the incessant contact with alkaline blood. (p. 8.)

* Physiologie, Bd. i. p. 118.

The chemical apparatus of the animal body appears to be essentially the capillary system. Hence endosmose, which may be reckoned a modification of the capillary force, plays a conspicuous part. Animal membranes, it is observed, become more permeable for the passage of liquids, when freed from fat by means of ether. It is therefore hinted that the different amount of fat deposited in membranous textures may influence more or less the power of absorption. (p. 14.)

The chapter on assimilation contains some original researches upon the saliva, which are interesting so far as the subject has of late given rise to much controversy: 1. The saliva notably reddens delicate litmus paper in the early part of the day, and one or two hours before, but particularly after eating. The author employed thin post paper, which did not of itself redden litmus, imbued with a solution of litmus, previously treated with ether, then allowed to stand for a considerable time and afterwards filtered. 2. The saliva offers an alkaline reaction directly before and during the act of eating (the period of appetite and mastication). It is expedient that an adequate quantity of saliva be placed on the paper. In some individuals, particularly in the instance of robust labourers, the saliva was at other times almost always faintly alkaline. 3. The saliva of children and adults frequently renders delicate red litmus paper blue, chiefly in the forenoon and night. The last paper was prepared by suspending the other paper in an atmosphere impregnated with hydrochloric acid gas, until it acquired a faint but appreciable red tinge. 4. Saliva taken from the same person, while it strikes a red colour with the blue, also strikes a blue colour with the red litmus paper; except during the time of meals and of appetite, when the alkalescence invariably predominates. 5. The saliva of a child at the breast, six months old, did not affect either red or blue litmus. 6. Litmus paper, whether red or blue, to which saliva had been applied became after drying permanently red. (p. 44.)

The fixed redness of the litmus paper would seem to depend on the presence of some ammoniacal salt; either the lactate, the acetate, or the phosphate. But why the saliva should at certain times possess the property of rendering blue litmus red, and red litmus blue, is another question; it is undoubtedly a mixture of the buccal mucus and of the fluids of several glands—the parotid, the submaxillary, the sublingual; and these most likely afford secretions differing as regards acid and alkaline reaction If we consider therefore in a chemical and physiological point of view, that the normal saliva is what belongs to the process of mastication, or rather to the period of appetite, we shall find it to be alkaline, as can be distinctly proved by the blue colour imparted to the juice of bilberries, shortly before and during a repast. Our author thinks the reddish-blue powder of iris or violet petals, when put on the tongue or in the mouth, affords a still more delicate test. It is always reddened, unless at the above periods.

We are indebted to M. Mandl for some curious investigations into the chemical nature of the secretions in connexion with the nerves which go to the respective secernent organs. He says that the saliva has an alkaline reaction; and that all organs supplied with nerves from the cerebro-spinal system (in the present instance the *nervus trigeminis* and *facialis*) afford an alkaline secretion, while those furnished from the

ganglionic system yield an acid secretion in the state of health. The alkaline condition of the saliva coincides with the formation of salivary concretions, and with the so-called tartareous crust upon the teeth. Mitscherlich detected chloride of calcium in the residuum of incinerated saliva.

The saliva, according to M. Donné, acquires an acid reaction in inflammatory states of the system and in cases of irregular menstruation, becoming again alkaline on the subsidence of the inflammation and the restoration of the catamenia.

"If we call to mind that Pelouze and Fremy have shown that mucous membranes have the power of transforming sugar of milk, dextrine, and the like, into lactic acid at the ordinary temperature of the human body, it appears very probable that this peculiar property may be exalted in cases of disease, more especially in those attended with inflammatory irritation; and that while the mucous membrane of the mouth pours out an acid which is more than neutralized, being rendered even alkaline by the proper saliva, still this alkalinity will be counteracted by the excessive formation of lactic acid. If we consider further, that certain vegetable salts are converted by animal membranes into carbonates of their respective bases, a thing which, according to Wöhler, takes place in the living organism, we see good reason for the employment of such medicines as tartrate of potash, citrate of potash, and the like, in the slighter forms of inflammatory irritation." (p. 49.)

Some pages are occupied with details of the examination of phthisical sputa, and the distinctive characters of pus-globules. As the blood-corpuscles, with their nuclei, dissolve and disappear in bile, so likewise do the pus-globules, and that very rapidly, with the aid of heat. The bile used by Dr. Hünefeld was that of the ox.

"If pus or puriform sputa be triturated together with bile, there results a gelatinous very viscous ropy fluid, not readily diffusible in water from its consistence, but which eventually, by being heated, dissolves into a slightly turbid liquor. When purulent serum is mixed with gall (freed from mucus by digestion in alcohol, &c.), filtered, then decomposed by alcohol and made to boil, a kind of loose coagulation and a muco-pulverulent deposit of albumen ensue. When, again, a solution of the pus-globules decomposed by alcohol is made to boil, no coagulation takes place, and even after long standing only a trifling sediment can be perceived. In this manner the mass of pus-globules can be completely distinguished from albumen and fibrin, for the solutions of these substances in bile will coagulate on the free addition of alcohol and the application of heat. As these corpuscules are the essential constituents of pus, the comportment towards bile forms a very important characteristic of pus, particularly in microscopico-chemical analysis. If mucus be taken by way of contrast (I employed the buccal mucus of man and the gastric mucus of pigs) it is reduced to a tenacious ropy liquid, and the vesicles disappear; if alcohol be added and heat employed, it then separates again exactly as happens when bile is so heated for removing its mucus, and the separated mucus rises in thick shreds to the surface of the liquor, presenting a grumous mass when examined with the microscope. This, therefore, affords a good criterion of pus and mucus." (p. 65.)

The circumstance that pus burns with a flame in consequence of the fatty matter it contains, supplies an excellent and ready test, first proposed by Güterbock.

The author adopts the view of Langlois, that tubercles consist essentially of fibrin.

"Tubercular substance heated along with concentrated hydrochloric acid

forms presently a beautiful deep amethystine or lilac transparent solution, as is precisely the case with pure fibrin. Thus fibrin can be separated from albumen. It is prerequisite that the substance to be examined be freed from fat by means of ether, otherwise the solution will have a muddy brownish-red appearance." (p. 74.)

Under the head of chymification, or the alteration produced on food by the gastric juice, Dr. Hünefeld, after commenting upon the various opinions of physiologists, details a series of experiments instituted to prove the non-existence of free hydrochloric acid in that secretion. They are based upon the volatile nature of that acid. But we do not consider the negative evidence he has adduced sufficient to countervail the results obtained by so accurate an observer as Dr. Prout.

Wasmann's recent and elaborate researches upon pepsine, to which the author refers, place in a very strong light the solvent agency of hydrochloric acid. It appears to us that the true theory of digestion is that which attributes the *act of solution* to the *hydrochloric acid;* and the *preparation* of the aliment for that solution, to the pepsine or peculiar animal principle of the gastric fluid, which seems to act the part of a ferment, by causing such an incipient change in the substances to be digested as makes them more readily soluble in the dilute acid. This appears from the fact, that a long-continued moderate heat will enable hydrochloric acid to affect the solution without the pepsine; and that a small quantity of pepsine is sufficient to prepare for solution an almost unlimited amount of aliment, whilst the quantity actually dissolved depends upon the quantity of free muriatic acid in the liquor.

Our knowledge of the chemical constitution of the bile is at present very indeterminate. The experiments of Demarçay, and the conclusions to which he arrived, have been impugned by Berzelius. Our author having, as above stated, ascertained its energy in dissolving the blood-corpuscles, was led to try its action on various organic matters. The results are as follows:

"1. Bile reduces the fibrous clot of the blood to a clear liquor, in from a quarter to half an hour, at a temperature ranging between 95° and 100° Fahr. This is seen best betwixt two slips of glass, sealed together at the edges. 2. Bile dissolves casein (separated from milk by alcohol or a vegetable acid) under like circumstances, within the space of a few minutes; the cheesy substance obtained from sour milk is most rapidly dissolved. If that be triturated along with bile a thin pap is formed almost entirely soluble in water: the liquor is at first somewhat turbid, but becomes ere long transparent. The application of heat quickens the effect, but causes a creamy pellicle, consisting of butter, sometimes mixed with a little casein, to rise and float on the surface. The liquid itself is a combination of casein and bile, unaffected by heat, and from which acetic acid precipitates a mixture of casein and cholic acid. By the above property we can distinguish casein from coagulated albumen and from fibrin. The prompt solution of casein by bile, I hold to be an important fact. 3. Gall dissolves at the above temperature, in the course of a few hours, flesh grated and cooked; the experiment being conducted between slips of glass sealed together. The inflammatory crust of blood, under similar circumstances, was very soon reduced to a clear liquid." (p. 105.)

Ox-gall has been long reputed as stomachic; and if the above results be confirmed, its remedial agency may be ascribed in some degree to its favouring the solution of certain kinds of food in weakened states of the stomach and intestines. The kidneys stand in close relation to the liver,

hence the bile may be considered as complementary to the urine. By the latter the blood is freed of saline matters, particularly ammoniacal salts and analogous combinations; by the former of fatty substances, cholesterine, margaric acid, choleic acid, and of biliary colouring matter, which last is probably the colouring matter of the blood partially modified. Haller says very justly, that the bile, if purely excrementitious, would pass, not into the duodenum, but into the rectum.

While treating of the changes produced upon food by the juices of the alimentary canal, Dr. Hünefeld points attention to the fact that the ammoniacal decomposition of mucus and its accumulation are frequently the cause of crystalline deposits in the intestines. Valentin found in the small intestines of a bear beautiful rhombic crystals of gypsum associated with organic matter. In the human subject, likewise, microscopic crystals have been detected, composed of phosphate of lime, sulphate of lime, and a salt of soda, namely, in the excrement of a typhous patient, a circumstance to which Schönlein has attached considerable weight. Gluge concludes, from more recent investigation, that crystals are very common in the fæces of typhous patients, and differ from those in the fæces of persons labouring under other diseases or in health. The excessive formation of ammonia may have something to do with this. Wittstein has shown that phosphate of lime is soluble in certain ammoniacal salts when aided by warmth.

The cecum contains both lactic and butyric acids. In this portion of the bowel there is carried on, to a certain extent, a repetition of what takes place in the stomach and small intestines; and its mucous membrane exhibits an acid reaction whenever the food contains much that is indigestible and stimulating. In the colon are found the insoluble remains of food, chiefly vegetable, brown colouring matter of the bile, mucus, fat, cholesterine, azotised extractive matter which acids redden, several soluble and insoluble salts (of soda, lime, and magnesia), besides fetid volatile products. (p. 120.)

In the chapter on respiration, we are told that the air remaining in the lungs after death consists for the most part of azote, with some carbonic acid. A solution of deoxidized indigo on being injected assumed merely a greenish tint: a new *docimastic* test is thus furnished. The residual air is retained with great force in the lung; the whole being expelled only after boiling in water, whereupon a lung which has breathed sinks to the bottom of the vessel. A large amount of air is thus obtained. The specific gravity of the lung which has breathed is about that of ether, therefore it floats in alcohol and oil of turpentine. The lungs can be washed with chlorine water, nay even steeped in it for some time, without losing their buoyancy; a thing of importance in a medico-legal inquiry.

The blood is fully considered. It is a singular fact, that various salts, namely, sulphate of soda, nitrate of ammonia, iodide of potassium, possess, under particular circumstances, the property of facilitating the mechanical separation of the coloured molecules from the *liquor sanguinis*, so that they can be easily removed by mere filtration. (p. 159.)

The saline ingredients of the blood are chiefly derived from the food. In the blood of the herbivorous animals there is a predominance of potash over soda salts, while in that of the annelides there appears to be very little soda. Sal ammoniac is stated by Lecanu to be present, but of this

Dr. Hünefeld is doubtful. The minute portion of ammoniacal salts which exist would seem to be only the lactate and phosphate. Berzelius, indeed, is of opinion that ammonia is seldom in the blood; and that where chemical analysis has revealed the existence of sal ammoniac, it is more than probable the result of the mutual decomposition of chloride of sodium and phosphate of ammonia. It is possible that more ammoniacal salt would be discovered in the blood were it not removed as soon as formed, as is the case with urea.

Ammonia is generated by the action of alkali upon protein and other animal matters, especially mucus; the alkali contained in the albuminate or fibrinate of the blood is sufficient to cause this.

"If serum, blood, serous fluids, and the like, be preserved from putrefaction with ether, and allowed to remain some time in a closely-corked glass phial, the liquid will be found to contain distinct traces of ammoniacal salts. Their proportion will be considerably augmented by a slight addition of caustic alkali. The best plan of proving the presence of ammonia is by triturating the liquor in question (when fresh) with hydrated oxide of lead or chalk, and then distilling along with alcohol. The product will strike a gray colour with protonitrate of mercury, provided there is any pre-existing ammoniacal salt. The constituents of the blood do not seem to modify the crystalline form of sal-ammoniac. I poisoned a rabbit with it (it swallowed daily a scruple, and lived sixteen days); the blood slowly dried between two slips of glass and examined with the microscope, presented distinct crystals of sal ammoniac. Through retention of ammoniacal salts in the blood, originate most probably violent symptoms of irritation, and thence a breaking up of the tissues; the alkali of the blood may also disengage some free ammonia." (p. 170.)

The potassium test lately suggested, would, we believe, set the ammoniacal question at rest.

Some practical directions are given for estimating the composition of the blood, followed by remarks concerning the leading peculiarities of arterial, venous, and portal blood, chiefly drawn from the observations of C. H. Schultz. The chapter winds up with an account of the changes effected on the blood by disease, as recorded by Lecanu, Mulder, and others, but without annexing anything novel or interesting.

In reference to what may be termed the analytical processes of the animal economy, namely, nutrition, secretion, and excretion, there prevails much diversity of opinion. Not a few writers entertain the notion, that all the substances from which the organic solids and fluids originate exist ready formed in the blood. The circumstance of urea being found in the blood after extirpation of the kidneys, and after mortification of the nerves going to the kidney as mentioned by Nysten, as likewise of its being discovered by Barrel and Marchuand in the fluid of dropsy, taken in conjunction with the fact of the prodigious velocity with which certain substances are thrown off from the blood, and the imperfections in the modes of chemical research, affords a fair ground for conjecture that various matters preexisting in the blood may have escaped notice. Agreeably to this view, the organs of secretion and excretion are merely channels of separation and not of formation, which is but the revival of the chemical pathology of a former age.

On the other side cogent arguments can be adduced to demonstrate that various matters have their being without the pale of proper *hæmatosis*, in the intimate texture of organs by the conversion of tissues, so to speak. To these appertain the primary constituents of bile, several modifications

of fat, casein, sugar of milk, gelatine and chondrine, mucus, horn, elastic vascular texture, pigmentum nigrum and other colouring matters; in pathology, xanthic oxide, uric acid, cyanurine, &c. Of the above none have been as yet detected in the blood; though, perhaps it is true, seldom sought for with adequate skill and knowledge. (p. 189.)

On this we would remark, that of the existence of cholesterine and the colouring matter of the bile in blood there can be no doubt whatever; and that, in regard to others of the substances mentioned, there would be great difficulty in distinguishing them from the albuminous matter of the mass of the blood, on account of the absence of any definite characters. Some of the recent French analyses indicate horny matter and casein as substances to be more or less constantly detected in the blood.

The author, again reverting to the conversion of sugar into lactic acid and of vegetable salts into alkalis of the corresponding bases, thinks we may frame some hypothesis as to the power by which parenchymatous organs, especially those of the complex mucous glands, may act upon this or that constituent of the blood. Besides, the mechanically increased influence of alkali and oxygen on the one hand and of the generally acid fluid of parenchymatous organs on the other, must materially aid. All this reminds us a good deal of the reasoning and doctrines of Silvius de la Boe. He continues:

"The conversion of salts containing organic acids into carbonates is most likely the source of the alkali of the blood and various secretions; the albumen and fibrin evolving carbonic acid, albuminates and fibrinates result, which change is favoured by animal heat, capillarity, and other agents. The salt most uniformly subject to undergo conversion into carbonate is lactate of potash, and which is diffused throughout the whole organism." (p. 190.)

Several pages are occupied with the chemical qualities of the different textures and products of the body. Dropsical effusions are interesting in reference to pathology.

"1. These morbid collections possess in most instances the constituents of serum. 2. The albumen is identical with that of the serum, occasionally corresponding in quantity (7 to 8 per cent.) more seldom exceeding, most generally under (½—1 per cent.) 3. Hydropic fluid is now and then fibrinous, coagulating out of the body as observed by Magnus, in the inflammatory diseases of mucous membranes, and also thrown off along with false membranes, as in pleuritis. Tennant and Quevenne examined a fluid of this description from the pleura, the result of inflammation, which resembled concentrated lymph. 4. Not rarely, perhaps always, a substance analogous to the saliva, by some termed mucus, by others a peculiar uncoagulable substance, by others again of an earlier date, gelatine. It appears to be the source whence hydropic effusions poor in albumen appear thick and ropy, 5. According to some writers cholesterine has been found partly suspended in a fine crystalline form. Lassaigne and Marchand met with it in the fluid of hydrocele and also in that of sarcocele. 6. The dropsical fluid of an icteric patient contained the elements of bile and also cholesterine. Bouchardat sought in vain for the constituents of milk in that of a patient labouring under puerperal peritonitis." (p. 233.)

Buchner made the curious observation that during catarrh the secretion of the mucous membrane of the nose contains no nasal mucus, but is serous and saline; towards the termination of the affection it becomes again mucous, when it contains a peculiar acid volatile fat, also found in the expectoration of phthisical subjects. This may be at once recognized by its emitting flame when dried on a slip of platinum and exposed to the heat of a lamp.

It is well known that healthy urine when just voided gives an acid reaction. This is due partly to the presence of free lactic acid, partly to that of acidulous phosphates of ammonia and lime and uric acid. On account of the property which ammoniacal salts possess of reddening litmus when dried, urine is only fit for testing when fresh and unevaporated. It deserves notice that Berzelius has shown that butyric acid occasionally exists in urine. In the case of nurses and blond females it is uniformly present. This then is another source of acid reaction.

It was presumed that phosphoric acid taken into the stomach would render urine acid, when alkaline from disease. The illustrious chemist last named mentions a case where a calculous patient swallowed that acid in gradually increased doses. The urine was at first not acidulous, but eventually the phosphoric acid caused diarrhea, whereupon the urine became acidulous and clear (which it was not previously) and deposited uric acid. This ceased with the diarrhea, and neither the continued use of phosphoric acid nor of acetic acid counteracted the sediment or the alkaline condition of the urine; the patient progressively wasted away and died. (p. 273.) The phosphoric acid therefore proved a failure. From Mr. Ure's recent researches it would appear that benzoic acid renders alkaline urine acidulous and limpid in virtue of the formation of hippuric acid, and thereby obviates calcareous deposition. (Med. Gaz. February, 1843.) This certainly is an important step as regards the pathology of the urine.

Various modifications in the nature of the urine produced by diseased action are pointed out, but without either order or novelty. Under the directions for testing the urine we find the following recommended for ascertaining the degree of acidity: to a given measure of the fresh urine, tincture of litmus previously brought to a state of perfect neutrality by means of acetic acid is to be added, and then liquid ammonia of a definite strength to be slowly poured into the mixture from a graduated glass vessel until it assumes a blue tint; from the quantity of ammonia expended the precise amount of acid may be deduced. Or a certain weight of ignited carbonate of soda dissolved in water may be used. This is to be gradually dropped into the urine (slightly warmed) until it becomes quite indifferent to test (red and blue litmus) paper. Tincture of turmeric darkened with ammonia can also serve as a means of acidimetry, but the manipulation requires some practical experience. (p. 245.)

Numerous processes have been at different times proposed for determining the proportion of sugar in diabetic urine. Perhaps the most accurate is that of Simon, which Hünefeld has not supplied. It directs the urine, previously evaporated to the consistence of syrup, to be mixed with from four to six times its weight of anhydrous alcohol; this precipitates the greatest part of the sugar, which is next to be washed with a fresh quantity of alcohol. It is now to be dissolved in a little water, and alcohol added in order to precipitate the mucus, salts, &c. The solution is to be filtered, evaporated to dryness, and the sugar dissolved in strong boiling alcohol, from which it is deposited by slow evaporation in mammillary groups of minute crystals. The same author recommends Von Trommer's method for finding out sugar in the blood. The protein combinations are to be precipitated with anhydrous alcohol; to the alcoholic solution some dry carbonate of potash is to be put and the mix-

ture well shaken. A small quantity of solution of sulphate of copper is then to be added and heat applied. If sugar be present, a yellow or yellowish-brown film of protoxide of copper will form in the undermost layer where the potash is diffused.

While on the subject of the urine, M. Lecanu's mode of detecting minute portions of blood deserves notice, as it is a thing of considerable importance and difficulty. He directs us to bring the urine to the boiling point so as to coagulate the principles of the blood, which are afterwards to be collected on a filter, washed, and then squeezed. They are next to be boiled in alcohol containing a drop of sulphuric acid. This dissolves out the hematine, which may be separated by alcohol in the usual way, and at once recognized, after calcination, by its red ferruginous ash. The hematine separated under these circumstances floats upon the surface of the liquor as a black resinous mass, which forms a red solution with acetic acid and ammoniated alcohol.

The third section of this book treats of the chemical comportment of the inorganic and organic matters to the organic fluids, and of these to each other. We subjoin his observations regarding the ammoniacal salts.

"The officinal and probably all the salts of ammonia have the property to a greater or less degree of dissolving the blood-corpuscles even to the nucleus, although slowly, and the protein textures generally. Whether they are thus endowed of themselves or whether it is in virtue of ammonia set free by the alkali of the blood, is the question; at all events we have already ascertained that free ammonia is not essential to these effects. When the blood is combined with an ammoniacal salt it acquires generally a brighter red, but this soon passes into a brownish-red hue; it does not coagulate, or forms at best a mere loose semi-fluid cruor, the corpuscles begin to disappear and the whole becomes more limpid. Succinate of ammonia, however, seems to form an exception, for by the admixture of one part in a hundred the clot remained firm, friable to the feel, and of a nut-brown aspect. Of all the salts tried the cyanate of ammonia (which by evaporation yields urea) exercised the most powerful solvent agency. Hydrochlorate of ammonia renders the blood clearer and heightens its colour more than the other salts; under its influence the blood-corpuscles disappear very quickly. Nitrate of ammonia in excess imparts to the blood a beautiful vermilion-red tint and precipitates the corpuscles rapidly and completely before they are dissolved, so as to allow the clear serum to be readily filtered, ere long solution commences and is fully accomplished at the ordinary temperature. Blood thus decomposed progressively evolves distinct traces of ammonia. It is very probable that we may partially explain upon chemical grounds (solution and disengagement of ammonia) why large doses of sal-ammoniac act as poisons, and smaller doses long continued induce a scorbutic condition, yet the same medicament judiciously exhibited furnishes a valuable stimulant to the secretory and excretory apparatus. That chemical attraction is inadequate to account for the therapeutic and toxico-dynamic power of sal ammoniac is obvious, inasmuch as it exercises a general action and induces inflammation of the stomach, even when introduced into the subcutaneous cellular texture. Frogs, owing to the extreme delicacy of their intestinal mucous membrane, die usually with excessive rapidity after trifling doses of this salt: placed on the dead gut it renders its surface slippery and the mucous coat swollen." (p. 281.)

Dr. Hünefeld observes that urea (anomalous cyanate of ammonia) likewise dissolves the blood-corpuscles.

Grognier discovered sal ammoniac in the serosity of a horse which had been poisoned by its means. Whether ammoniacal salts really undergo

decomposition in the blood, ammonia being set free, or whether they are carried off by some of the emunctories, as the kidneys or the skin, requires further investigation. They no doubt, like other salts, possess the property of dissolving the albuminous elements of the blood, and thus destroying their plastic nature. Hence their utility in inflammatory diseases where the amount of fibrin is notably augmented.

Medical men have generally looked to chemistry to supply them with antidotes against poisons, and they have not looked in vain. Libavius long ago remarked with great truth, "Est quidem chemia in hoc famula medicinæ ad modum solers, sed tantum non sola illud obtinet, et requirit prudentem artificem, qui possit salvo auxilio nocitura tantum tollere, sciatque qua in parte sit utrumque" (Tr. de veneno sing. P. i. 1599). The discovery of hydrated oxide of iron as an antidote for arsenic, by MM. Berthold and Bunsen, and of the mixture of iron-filings and gold-dust as a *galvanic* antidote for corrosive sublimate by Dr. Buckler, are unquestionably important achievements in the department of toxicology. In reference to the former, Duflos recommends what is termed the *liq. ferri acet. oxyd.* as being preferable to the hydrated oxide, inasmuch as that is only available in cases of poisoning from arsenious or arsenic acid, but not so where arsenite of potash has been swallowed, such as exists in Fowler's solution. Hence he advises, whenever any doubt is entertained concerning the precise nature of the arsenical preparation, that the acetate be administered so as to give the patient the benefit of that doubt. It is prepared with one part of hydrated oxide of iron, three parts of vinegar of specific gravity 1·06, and as much water as will make in all sixteen parts. An excess of acetic acid is no ways injurious.

The fourth section is prolix and confused. It is headed, "physiological bases of pharmacology, as an interponent between organic chemistry and special therapeutics." It comprehends articles of diet, water, atmospheric air, warmth, electricity, and disease. The fifth and last section is entitled, "a criticism of the materia medica, in reference to its chemical and physical principles." A new classification is introduced, including: 1. Neutral assimilative substances. 2. Mechanical agents. 3. Excitants. 4. Solvents. 5. Decomponents. The author conceives that the metallic salts and oxides exercise a direct decomposing influence upon the blood and other circulating fluids, but he is unable to assign any sufficient chemical reason for their remarkable toxico-dynamic and pharmaco-dynamic effects. He is, on the contrary, more inclined to ascribe the different changes produced to nervous influence.

"Iron alone is a uniform constituent of the organization, all other metallic combinations invariably act in a deleterious manner upon the animal organism; their very taste shows how heterogene they are. They ere long induce a state of decomposition incompatible with life, the primary cause of which seems to be nervous irritation. It would appear that the ganglionic system is the one to which the dynamic effects of metallic preparations are immediately directed. Hence many cases of deadly poisoning have been known to occur from their administration, in which there was no sign of inflammation." (p. 344.)

He very properly censures those persons who imagine that chemistry can and must lead to an *à priori* knowledge of the action of remedies, and then run it down as futile because it fails to do so. They overlook what life is, what organization is, what chemistry is. They do not think

that if chemistry could accomplish this, it would be essentially medicine. Each external substance, he observes, can act upon the organism mechanically, dynamically, and chemically; but it is only the dynamic agency with which the medical man has to do, independently of surgery and toxicology. C. G. Vogel emphatically declared "half the materia medica is uncertain and full of contradictions," and thus it will remain unless made the subject of rigorous inductive inquiry. Professor Marx says, "were every medical man, or a union of medical men, to select for trial and close investigation during a series of years a single remedy only, and were their results reciprocally communicated and afterwards tested in other places under other conditions, we should then have a materia medica upon which reliance could be placed, when each tried noxious medicament could be exhibited without risk of peril. Until this work is accomplished, the conscientious practitioner can resort to but few remedies; for whenever the choice lies between what is harmless and what heroic, he must unconditionally employ the former."

Taken as a whole the work before us is certainly a most heterogeneous compound, containing here and there useful information, but extremely deficient in clear and consecutive reasoning. The author is evidently a very laborious compiler, for there are nearly three hundred references put down at the end of each "book." Yet the facts are so ill assorted, and there is so much loose hypothetical assumption, as to convey to the reader's mind an impression far from satisfactory. Indeed, it requires no ordinary share of patience to get through these volumes.

ART. IX.

Friederich Tiedemann, Professor in Heidelberg, von den Duverneyschen Bartholinschen, oder Cowperschen Drüsen des Weibs und der schiefen Gestaltung und Lage der Gebärmutter.—Heidelberg, 1840. 4to, pp.42.

Professor Tiedemann on the Glands of Duverney, Bartholinus, or Cowper in the Human Female; and on Obliquity in the form and position of the Uterus. Four Plates.—*Heidelberg and Leipsic*, 1840.

I. ABOUT the middle of the seventeenth century J. C. Duverney discovered (at either side of the entrance of the vagina in the cow) a duct leading to a large lobulated gland which was covered by the skin and the constrictor vaginæ muscle. Bartholinus, to whom Duverney mentioned the existence of these organs, discovered them in the human female, and compared them to the prostate in the male. Duverney confirmed the observation of Bartholinus, and after him Peyer, Harder, Morgagni, Santorini, and others, saw and described them. Haller did not succeed in detecting them, but denied their existence, and asserted that the supposed glands to which the ducts of Bartholinus led were mere mucous crypts. Haller's denial weighed more than the assertions of anatomists for the previous half century, and it was soon forgotten that any such structures had ever been observed. After more than a century they were rediscovered by Mr. Taylor, who described them in the Dublin Journal, vol. xiii., and they are briefly alluded to in Mr. Guthrie's work on Diseases of the Bladder.

The attention of Professor Tiedemann was called to these organs by a visit which he paid to Hamburg in 1830, when Dr. Fricke pointed out

to him in the venereal hospital, the liability of the follicles at the entrance of the vagina to inflammation and ulceration. On referring to the writings of various anatomists, he was surprised to find in some a minute description of the form and situation of two large glands placed at the entrance of the vagina, and accordingly he proceeded to examine them for himself. After a little dissection he discovered them.

"These glands are situated at each side of the entrance of the vagina, beneath the skin covering the posterior or inferior part of the labia. They are likewise covered by the superficial fascia of the perineum, and by the fibres of the constrictor vaginæ. They here occupy a space included between the lower end of the vagina and the ascending ramus of the ischium, and the ramus of the clitoris and erector clitoridis muscle. Above them lie those fibres of the levator ani which take their origin from the ischium, and behind them are the transversi perinæi muscles. The glands are enveloped in very loose cellular tissue, the removal of which requires very careful dissection in order not to cut away the glands with it. They are rounded but elongated, flat and bean-shaped. Their long diameter varies from 5 to 10 lines: their transverse diameter from $2\frac{1}{2}$ to $4\frac{1}{4}$, and their thickness from $2\frac{1}{4}$ to 3 lines." (p. 13.)

Like Cowper's glands in the male they are not invariably present. They frequently differ in size on the two sides, and sometimes the gland on one side is absent. In advanced age they diminish in size, and even totally disappear.

From the anterior edge of the upper part of each gland an excretory duct runs horizontally forwards and inwards, beneath the constrictor vaginæ, to the inner side of the nympha, where its opening may be detected just in front of the carunculæ mystiformes, and surrounded by several small mucous follicles. The secretion of these organs is a thick, tenacious, grayish-white fluid, which is emitted in large quantities during sexual intercourse, and is doubtless the matter erroneously supposed by Hippocrates and the medical writers of antiquity to be the female semen. The uses which this secretion serves are unknown. It is scarcely likely that it answers merely the mechanical purpose of lubricating the parts during copulation. Probably its admixture with the semen answers some important purpose connected with impregnation. Perhaps it is a vehicle for the fecundating principle of the semen, an office which appears to be in part fulfilled by the secretion from the prostate, and Cowper's glands in the male. The existence of these bodies in the females of all animals, in which the males are furnished with Cowper's glands, is another strong argument in favour of the analogy of their functions.

These glands are subject to various morbid affections, and are especially liable to become diseased in the progress of virulent gonorrhœas. Tiedemann found the excretory ducts obliterated in the body of a woman who had suffered from inflammation of the mucous membrane of the vagina, while the lobules of the glands were dilated to little cells, and contained a reddish tenacious fluid. In the body of a young woman who had died of puerperal fever these organs were red, inflamed, and much swollen.

II. The subject of the second essay was briefly referred to in our review of Naegele's work on Obliquity of the Pelvis. Tiedemann, as might be expected, treats the theme as an anatomist, not as an obstetrician, though he glances at the influence of this malformation of the uterus upon labour. In the course of forty years Professor Tiedemann has met eight times with obliquity in the form of the uterus. The degree

of the malformation differed much in the different cases, and it is not very easy to convey a just notion of it to the reader without the aid of plates. In one instance two thirds of the uterus lay to the right of a perpendicular line drawn from the centre of the fundus. With this deformity of the uterus there was usually combined an inequality in the length of the broad and raised ligaments. In one instance the left side of the fundus at the point where the Fallopian tube enters formed a very acute angle, and stood somewhat higher than the right side, facts which clearly point to the mode in which this deformity originates, and show it to be congenital. One of the eight observations, too, relates to an infant which died immediately after birth, and proves that this state is an original malformation, and not a condition acquired in after life. In the seven other instances the subjects were adult, but none of them showed any sign of ever having been pregnant, though in five the hymen was destroyed. This circumstance might lead to the supposition that obliquity of the uterus occasions sterility, perhaps from the curved and narrow state of the canal of the cervix. There is an instance to the contrary however in Cranmerarius and Pfister, *Specimen experimentorum circa generationem hominis et animalium*, p. 10. The same case likewise illustrates an assertion of some of the older writers on midwifery, (concerning which much doubt has been expressed), that obliquity of the uterus renders parturition extremely hazardous and difficult, the woman having died undelivered.

It is evident from the above facts, that an oblique uterus retains this form during labour, and thus Boer's ingenious but hypothetical distinction between obliquity in the form and in the situation of the uterus is fully confirmed.

The observations of obliquity in the form of the uterus before the appearance of Tiedemann's paper are very few; obliquity in its situation, from unequal length of its ligaments, has been noticed by many anatomists, though by but few midwifery writers. The observations of obliquity from this cause related by Tiedemann, or quoted by him from others, do not contain anything novel.

One question of some interest still remains; namely, the circumstances which may produce obliquity of the uterus after birth. In two cases of lateral curvature, with the convexity towards the right side, Morgagni found the uterus inclined to the right. In any form of lateral curvature too the uterus is likely, in the event of pregnancy occurring, to assume a position more or less oblique. Lameness when one leg is shorter than the other may be associated with it. It has also been found associated with obliquity of the pelvis. There are likewise many causes, such as adhesions from peritonitis, or tumours of various kinds, which may throw the uterus out of its natural position; but there is no foundation for the notion, first propounded by Gradi, that a lateral insertion of the placenta may produce obliquity of the uterus.

An appendix contains four observations by Professor Stotz, of Strasburg, of curvature of obliquity in the form of the uterus.

We cannot close this abstract of these two tracts without expressing the wish that there were more in our own country disposed to imitate Professor Tiedemann, who, though full of years and honours, continues with unabated zeal the pursuits of his youth.

Art. X.

1. *Die Lehre von der Reflex-Function, für Physiologen und Aerzte, dargestellt und beurtheilt* von Johann Wilhelm Arnold, Doctor der Medicin, Chirurgie und Geburtshülfe, practischem Arzte in Heidelberg.—*Heidelberg*, 1842. 8vo, pp. 88.

The Doctrine of the Reflex-Function, in its relations to Physiology and Practical Medicine, displayed and critically examined by J. W. Arnold, Doctor of Medicine, &c.—*Heidelberg*, 1842.

2. *On the Diseases and Derangements of the Nervous System, in their primary forms, and in their modifications by Age, Sex, Constitution, Hereditary Predisposition, Excesses, General Disorder, and Organic Disease.* By Marshall Hall, m.d. f.r.s. l. and e. &c. &c.—*London*, 1841. 8vo, pp. 380. With Eight Copperplate Engravings.

The reflex theory of Dr. Marshall Hall has now been fully before the public for more than five years; and it appears to us that the time is come when its merits may be duly estimated. Ample opportunity has been afforded to those who had objections to adduce or criticisms to offer; and such have not been wanting. On the other hand, there has been time to subject many of the most important positions to the scrutiny of experience; and for this, too, there has been no lack of inclination, either amongst the supporters or the opposers of the theory.

We believe that we are justified in stating that, among the most eminent physiologists in this country, especially those who have made the nervous system the object of their especial study, there are few who do not receive Dr. Hall's views, at least in all their most important particulars. There may be a difference of opinion as to the degree in which these views had been glanced at by previous authors; but we believe that all are agreed in attributing to Dr. Hall the merit of originality, as to a large portion, and this the most important one of the doctrines advanced by him. The principal opponent in this country of that view of the action of the spinal cord which is peculiar to Dr. Hall is one for whose opinion we shall always entertain the highest respect; and did we not feel well convinced of the insufficiency of his objections, the weight of Professor Alison's authority would make us pause ere we ventured to take up a contrary position. But in Germany Dr. Hall's views are far from having gained as favorable a reception. By Professor Müller they are adopted to a certain extent; but there is, as it seems to us, so much obscurity and inconsistency in his disquisitions on this subject, that we are really at a loss to know his exact opinions. By several other German physiologists, however, objections have been raised to Dr. M. Hall's doctrines; and, so far as we can judge, the reflex-theory, as such, does not find much favour amongst them. We might be disposed, at first sight, to regard this circumstance as a serious argument against its correctness; since the authority of Valentin, Nasse, Volkmann, Arnold, and others of its opponents, on questions of neurology, stands deservedly high. But it is to be remembered that even Sir C. Bell's doctrines, respecting the distinct functions of the anterior and posterior roots of the spinal nerves, made but very slow progress in Germany, although they are now universally received there; and when we come to examine the objections ad-

duced by Dr. Hall's opponents, we shall perhaps find that they are, after all, rather verbal than real.

We have, on former occasions, expressed our full conviction of the truth of the reflex-theory; and we shall now give evidence of the sincerity of this conviction, by taking up arms in its defence. The caustic and trenchant attack which has been made by Dr. Arnold, in the pamphlet before us, appears to us to call for special animadversion; since it embodies the objections brought by the various German physiologists we have alluded to, and brings them pointedly to bear on the question at issue; and also because it gives us the opportunity of exposing what we believe to be the radical error which pervades their whole train of reasoning on the subject. We shall first, however, give a sketch of the present state of the reflex-theory, and its applications to practice, as propounded by Dr. Hall in his latest work; and shall consider the most important difficulties attendant upon his views. Some of these may perhaps require modification in their detail; and such modifications we shall suggest, not in a captious, hypercritical spirit, but with the simple honest desire to render the system as stable and harmonious as possible.

The reflex-theory regards the true spinal cord as a centre of nervous action quite distinct from the encephalon; and as concerned in a set of actions of a peculiar and easily-defined character. That the cerebral hemispheres are the instruments of the intellectual operations is now universally admitted; in such operations, upon the reflex-theory, the true spinal cord has no participation whatever. The operations of the cerebrum are stimulated by *sensations* only; when they are taking place, we feel conscious of them, and we also feel a certain power of controlling and directing them by a strong effort of the will; and when they terminate in producing a muscular contraction, it is by an exercise of *volition.* Thus, food is placed before us; we become conscious of its proximity by sensation; our judgment is exercised in determining whether the food is desirable, wholesome, and palatable; and, if an affirmative decision is formed, a voluntary contraction is produced in certain muscles, by which it is carried into execution. Hence, the cerebral hemispheres must be directly connected with almost all parts of the system; since there are few parts not capable of receiving sensory impressions, and nearly the whole muscular apparatus is under its control. This connexion is established by means of the nervous trunks, some of which convey the sensory impressions to the sensorium, whilst others carry the mandates of the will to the muscles, and excite contractions in them. Hence we may very properly term the nervous trunks proceeding from and towards the hemispheres, *sensori-volitional* fibres.

Now these fibres exist, not only in the nerves of special sense, and other cephalic nerves, but in every nerve that is given off from the spinal cord; for this organ consists, in part, of a mere collection of fibres, which are continuous with those of the nervous trunks, and which establish the communication between these and the brain. The difference between the *true* spinal cord and the compound structure generally known under that name, is apparent at its lower part, *cauda equina.* The true spinal cord does not usually extend in man below the first lumbar vertebra; and the canal is occupied, below that point, by a bundle of nerves, which is but a continuation of a large part of

the white or fibrous structure in the upper part of the medulla. The *true* spinal cord is entirely distinct from this, as distinct as are the ganglia of the nervous column of the articulata, from the fibrous tract which passes over them. It is, in all essential particulars, a true ganglionic centre, having functions peculiar to itself; and this is clearly established by the fact, that motions dependent upon it are performed after the brain has been removed, or the connexion with it interrupted. That the nervous fibrils which are connected with the spinal cord are *anatomically* distinct from those which are connected with the brain, though bound up in the same trunk with them, and distributed to the same organs and tissues, is a doctrine which seems to follow naturally from Dr. Hall's proofs of their *physiological* distinctness; but it has been repeatedly disavowed by Dr. Hall as a part of *his* system, and it cannot be fairly attacked, therefore, by Dr. Hall's opponents, as one of his positions.

The doctrine of the structural distinctness of the spinal and cerebral systems of nerves was first advocated by Mr. Grainger, in his Treatise on the Spinal Cord. It was adopted by Dr. Carpenter, as the result of his comparative researches into the structure and physiology of the nervous system in the articulated and molluscous classes. And it now derives strong confirmation from the recent inquiries of Mr. Newport on the myriapoda.* He finds that the fibres which correspond to those of the true spinal cord in vertebrata are perfectly distinct from those connected with the cephalic ganglia. They form part of the cord in the intervals between the abdominal ganglia; and may be traced from the periphery into the several ganglionic centres, from which they pass backwards along the cord until they arrive at the next ganglion, from which they pass again to the surface of the body. The function of these fibres cannot be other than reflex; and thus is anatomically proved that which Dr. Hall contended for physiologically, and that which Dr. Carpenter had inferred from other forms of structure. Mr. Newport's experiments (of which, through his kindness, we shall be able to give some account,) seem to prove most decidedly that the supra-œsophageal ganglia in articulata are the seat of sensation and volition; and that, when these are removed or greatly injured, all the movements of the body are reflex.

The peculiarity of the action of the spinal cord, according to Dr. Hall, consists in this; that whilst *sensation* is required to prompt the operations of the intellect, to excite emotions, or to stimulate instincts to action, *no sensation* is necessary to produce those actions which are performed through this organ; but that the *impression* made upon the peripheral extremity of a nerve, and brought by it to the spinal cord, may excite a reflex muscular action, which shall succeed it without any *necessary* consciousness on the part of the individual, and therefore, without any mental operation whatever. In Mr. Newport's paper, although there is no direct *proof* that the ganglia are *not* sensitive, there is strong inferrential evidence that they are not so; since, although reflex acts of locomotion may be repeatedly exerted in the body after removal of the head or destruction of the central ganglia, these are always performed in precisely the same manner, and without any evidence of volition. In the

* "On the structure, relations, and development of the nervous and circulatory systems, and on the existence of a complete circulation of the blood in vessels, in the myriapoda, and macrourous arachnida." Communicated to the Royal Society, April 6, 1843.

ordinary condition of the system, however, consciousness *does* accompany these actions; but for this simple reason, that the impression which excited the spinal cord to action through its nerves, is also conveyed to the brain, and excites *it* to action through *its* nerves. But, if the excitement of a cerebral action be prevented, either by division of the spinal cord in its upper part so as to cut off communication with the brain, or by a state of inactivity in that organ, as in profound sleep and coma, no sensation is produced, yet the reflex actions go on as before. Some reflex actions take place, however, in the natural condition of the body, without sensation; this is the case, for instance, with regard to the propulsion of food along the œsophagus, which has been shown by Dr. J. Reid to be a reflex action; yet, after the food has passed the pharynx, there is no sensation, except such as is produced by its pressure on the surrounding parts, or by an irritation which its heat or acridity may produce in the mucous membrane. The convulsive movements in disease, again, are frequently excited by causes which do not produce sensations,—a circumstance on which we do not think enough stress has been laid by the upholders of the reflex theory. But the most decisive evidence of the non-participation of sensation, as a necessary link in the chain of reflex actions, is derived from the cases (of which many have now been subjected to careful examination) of disease or injury of the spinal cord in the human subject, cutting off the connexion of the lower extremities with the brain, but not impairing the reflex power of the lumbar portion of the cord; in the lower extremities of such persons, distinct and vigorous actions may be produced by slight irritations, which are not in the least degree felt by the patient. Indeed, it would seem that, when the muscles are thus withdrawn from cerebral control, they are much more readily excited to reflex action, than when they are under the direction of the will excited by sensation.

These facts are admitted by Dr. Alison; but he does not go along with Dr. M. Hall in the inferences he draws from them. "It is certainly possible," says Dr. Alison, (Outlines of Physiology, p. 211,) "that impressions made on sensitive nerves in the living state, but when the power of sensation is suspended, may so imitate those changes in them, which in the natural state are attended by sensations, as to produce some of the muscular contractions by which these sensations are generally succeeded and indicated; and all that can be certainly inferred from the facts in question is, that it is in the spinal cord that this *crossing* of the action excited in the nervous matters from the sensitive to the motor filaments takes place. But there is no distinct evidence that *regular combinations and successions* of muscular movements can be effected in distant muscles by impressions on sensitive nerves, without sensation intervening as part of the chain of events." The movements of respiration and deglutition are regarded by Dr. Alison as coming under this last category. Now on this we take leave to remark that, in our opinion, the burden of proof lies with those who maintain that sensation *does* participate in these movements, and that the spinal cord does possess, in any part of it, when isolated from the brain, the property of *sensibility*, by which we mean the power of rendering the individual *conscious* of the impressions conveyed to it. We submit that the very same kind of argument which Dr. Alison adopts (and in our opinion most correctly) to prove that secretion and nutrition are not dependent upon nervous

influence, will, if fairly carried out, necessarily lead to the conclusion that the reflex actions of the spinal cord are not dependent upon sensation. For it can be *proved* of many of these actions, that sensation has no possible participation in their production; therefore the existence of *reflex action without sensation*, must be admitted as a physiological fact. Moreover, it must be admitted that the lower portion of the spinal cord is not a distinct centre of sensation, when separated from the upper. It is for those, therefore, who maintain that the medulla oblongata may become a distinct centre of sensation, when separated from the brain, and that the actions performed through it involve sensation as a necessary condition, to prove their position; and we feel justified in stating that nothing like *valid proof* has ever been given. A great deal too much reliance has been placed upon the *adaptive* character of many reflex actions, as indicative of sensation; but what can be better adapted to the ends they are destined to accomplish than many movements of plants and animals, in which it is not supposed by any that sensation necessarily participates. To go no further than our own bodies; the contractions and dilatations of the heart, the peristaltic motions of the alimentary canal, and the similar movements of the fallopian tubes and of the ducts of the principal glands, exhibit a most obvious adaptive character, though performed without our consciousness; and there are well-known instances on record, in which the act of parturition and the ejaculatio seminis have taken place, when the sensibility of the genital organs had been entirely destroyed by section or disease of the spinal cord. This observation of Dr. Alison seems now likely to be put aside by the experiments and observations just referred to; in which Mr. Newport found that *regular combinations and successions of muscular movements in distant muscles can be excited in the myriapode, after decapitation*, or after removal of the cerebral ganglia without decapitation, by irritating the cut extremity of the cord, by pressure applied to the upper surface of a segment of the body, or by irritation of individual ganglia. By any of these means, acts of locomotion may be repeatedly excited after decapitation; and the body is moved forwards by regular and successive actions of the legs as in the natural state, but these movements are always directly *forwards*, *never* backwards, and rarely to either side, and then only when the direct line of progression is impeded on one or other side, thus giving mechanically a different direction to the body. These motions, in a newly decapitated individual, are at first almost as vigorous as in the natural state, but gradually become slower and slower until they are so feeble as not to carry the body forwards, and are then continued for some time longer until they entirely cease. On re-application of the stimulus after a few minutes of complete rest, these movements may be re-excited and the body carried forwards as before. But in these movements there is not the slightest indication of consciousness, no direction of object, no avoidance of danger. If the body be opposed in its progress by an obstacle not more than one half its own height, it mounts over it and moves directly onwards, as in a natural state; but if the obstacle is equal to its own height its progress is arrested, and the cut extremity of the body opposed to it remains in contact, with the legs still moving as in the act of locomotion. Who can doubt that these motions are entirely reflex, and devoid of what we regard as sensation?

Notwithstanding the great stress laid by some writers, on the medulla

oblongata as a peculiar centre of nervous agency, we quite agree with Dr. Hall in regarding its sole peculiarity to consist in the nature of the actions to which it ministers; these actions not being, like those of the spinal cord, movements of general locomotion, but being intimately connected with the maintenance of the organic functions. The study of comparative anatomy makes this very evident. For in the mollusca and articulata we find the respiratory and pharyngeal ganglia quite distinct from the pedal or locomotive; and the former are placed within the cranium of vertebrata, obviously because they are there more secure from injury.

It is quite true that there *are* actions which, though not voluntary, are still dependent upon sensation as one link in the chain. To these, perhaps, the term *instinctive* might be properly restricted. Some of them seem to require the sensation merely; such are the corresponding or *consensual* movements of the two eyes. In others, something like an emotion takes place; as in the production of laughter by tickling. And in others, the emotion is still more decidedly a necessary link; as when laughter results from the excitement of ludicrous ideas. And we freely admit that it is not always practicable to determine whether a particular action should be referred to this class or to the preceding. Thus, although we do not hesitate in regarding the ordinary acts of inspiration and expiration as of a purely reflex character, we think it still doubtful whether those peculiar combinations of muscular movement which constitute coughing and sneezing are not excitable by sensation only. But if the *principle* of reflex action without sensation be recognized, a difference of opinion on such details may for the present be overlooked.

We shall now briefly follow Dr. Hall through his exposition of the anatomy, physiology, and pathology of the *cerebral* and *spinal* systems, as set forth in the volume before us.

The *cerebral* system consists of the cerebrum, with the two sets of nervous fibres connected with it. One of these consists of fibres which originate in the sensory surfaces of the body, and terminate in the cerebrum, whilst the other consists of those which originate in the cerebrum, and convey the influence of volition to the muscles. All parts which are sensible to impressions, and which can be affected by the will, are thus connected with it; although the same parts may, by another set of fibres, be connected with the spinal cord. The posterior roots of the spinal nerves, and those of the fifth and eighth pairs of cerebral nerves, must be regarded as compound in their structure; containing both sensory fibres which proceed to the brain, and excitor fibres which terminate in the spinal cord. And in like manner the anterior roots of the spinal nerves, with the third and fourth small root of the fifth and sixth portio dura of the seventh, and motor portion of the eighth and ninth nerves, must be regarded as having a double origin in the brain and spinal cord, being subservient to the voluntary motions directed by the former, and to the reflex actions executed by the latter. "Every compound sentient and excitor, and voluntary and motor nerve," says Dr. Hall, "must be presumed, physiologically speaking, to have *two connexions*, *one cerebral*, *the other spinal*, with sentient and excitor *origins* in cutaneous and mucous surfaces, and appropriate *destinations* in the muscular system." (§ 104.) In this and several other passages, Dr. Hall speaks of the *physiological* distinctness of the cerebral and spinal systems; and guardedly avoids

any intimation of his opinion of their *structural* distinctness. Yet we must own that this appears to us a distinction without a difference. For the question at last reduces itself to this:—are the reflex actions, and the sensori-volitional actions performed by the same or by different fibres? If by the same, can we say (consistently with our knowledge of the elementary structure of the nervous system) that any *single* fibre can have *two* origins or terminations in the central and in the peripheral organs respectively? Can *a single* afferent fibre, for instance, have *two* origins, a sensory and an excitor, in a mucous surface, and *two* terminations, one of the brain, the other in the spinal cord? or can a single motor fibre have *two* origins, one in the brain, the other in the spinal cord? If it be admitted that these actions are performed by different fibres, then why not say so? It is true that their structural distinctness cannot be clearly made out in vertebrata; but in many of the invertebrated tribes there is not the same difficulty. And it is interesting to remark that whilst, in the vertebrata the fibres of the spinal nerves are so arranged that the afferent, both sensory and excitor, are bound up in one set of roots, and the motor, both volitional and reflex, in the other,—the arrangement of the fibres in the articulata and mollusca is such, that one set of roots of each nerve contains both the afferent and efferent fibres of the sensori-volitional system, and the other the excitor and motor fibres of the reflex system; as if nature had intended to give us the opportunity of discovering the isolation of all these systems, by the extension of our survey throughout her domain. If we had been confined to the study of the articulata, we should have discovered the structural distinctness of the sensori-volitional and the reflex systems; but we should have had no clue to the distinction between the afferent fibres, which convey impressions from the periphery to the centre, and the efferent or motor fibres, which convey the motor impulses from the central organs of the muscles. On the other hand, by the exclusive study of the vertebrata, we *have* discovered the latter, but not the former.

"The physiology of the cerebral system," says Dr. Hall, "comprises sensation in all its forms, perception, judgment, volition, and voluntary motion." (§ 106.) "The distinctive character of a voluntary motion is its spontaneous occurrence. Without sensation, without emotion, the individual *wills*, and a voluntary movement succeeds." (§ 112.) We do not consider this to be the really distinctive character of voluntary motions. The exercise of the will is consecutive upon the other processes enumerated by Dr. Hall, and these are at first stimulated by sensation. If we could conceive a being to come into the world endowed with a perfect brain and every mental faculty complete, but all the inlets of sensation closed, there would be no mental *operation:* for it is *sensation* which is the necessary stimulus to any change in the mental condition. The being would remain in a torpid state, like a man in profound sleep, possessing faculties, yet wanting the power of using them; just as the seed buried deep in the soil possesses vitality but does not germinate, because withdrawn from the influence of the requisite stimuli. But when the mind has been once aroused by sensation, it is rendered permanently active; for the sensation does not produce a merely transient change, but is like the spring, by touching which we set a complex machine in continued operation. The sensation is retained and may be reproduced by the act of conception at any remote time, or it may spontaneously recur

to the mind; and thus all the actions to which it gives rise may be in appearance spontaneous, whilst they are in reality as dependent upon sensations, felt or remembered, as are those of a reflex nature. The real distinction appears to us to be this. *Voluntary* motions are executed only as means to a certain end which the mind has in view; in other words, we throw a certain set of muscles into action, in order to accomplish a certain determinate object. In the reflex and instinctive actions, on the other hand, there is no perception of the object, or choice of means. But there is this remarkable peculiarity about voluntary motions, to which we do not think that enough attention has been given. We *will* to perform a certain movement, but we do not determine by the will the muscles which are to execute the movement. We must have the end or purpose of the operation in our mind, to constitute it a voluntary movement; but the immediate influence on the muscles is something different from the will, and approaches more to the character of an intuitive direction. We shall first take for illustration the case of the muscular operation concerned in the production of vocal sounds or musical tones. A little consideration makes us aware that we cannot *voluntarily* utter a given sound or tone, without in the first instance conceiving it (however transiently) in our minds; and the particular operation upon the muscles necessary to produce it is certainly not voluntary, for our *judgment* could not select the precise strain on each which is requisite to produce the combined effect. But an *emotional* cry is performed from the mere instinctive tendency, without any such previous conception. We believe that all persons whose organs of voice are naturally formed can at once execute any tone of which they form a definite conception; for singing out of tune is well known to be due to an imperfect musical ear, which renders that conception weak and indefinite. But whether the intuition is original or acquired in infancy we shall not at present discuss; probably there is some variation in this respect. Now this is rather an extreme case; and we have purposely selected it, because the remoteness of the relation between the muscular action and the effect to be produced, renders the distinction between them sufficiently obvious. But this distinction is not less valid in other cases. All the muscular movements which we call voluntary are under the *immediate* guidance of *sensation*, either received from the muscle itself or from some other source. The case of the woman whose arm was affected with anesthesia, and who could not continue to hold up her child unless she looked at it, is a most satisfactory proof of this; and the case is not a solitary one. The muscular sense being destroyed, even the strong voluntary effort could not keep up contraction in a certain set of muscles without the guidance of the visual sensation. Hence we see that there is something intermediate between the will and the muscles, of which the ordinary theory of voluntary motion does not take cognizance; and this might be inferred also from the fact, that irritation of the hemispheres does not excite muscular movements, though these are immediately shown when the irritating cause operates on the tubercula quadrigemina or on the medulla oblongata.

We return from this digression to the proper functions of the cerebrum, in which Dr. Hall includes the *emotional* actions. These he regards as distinguished from the voluntary, in being *immediately* excited by sensations. "Thus a disgusting object placed before the eyes will induce

vomiting and fainting." But he considers that, though their seat is in the cerebrum, and though they depend upon sensations, they operate through the true spinal system of nerves. We must own ourselves unable to comprehend how the true spinal nerves, which terminate and originate in the true spinal cord, can be the ministers of changes which take place in the brain; and it seems to us that the same line of argument which establishes the distinctness (whether we prefer to call it structural or physiological) of the true spinal or reflex, and of the cerebral or volitional systems, must also establish the existence of the emotional as distinct from either. Dr. Carpenter has brought forward a theory on this subject which we believe to be new, and to be supported by much anatomical evidence. (Human Physiology, §§258-265). He identifies the *emotional* actions with that restricted class of the *instinctive* which involve sensation (the lower instinctive actions being merely reflex), and localizes them in the ganglia of special sense, which constitute nearly the whole cephalic mass of the invertebrata, and which in fishes and reptiles (whose intelligence is very low) bear a very large proportion to the cerebral hemispheres. In mammalia, the greatly-increased size of the hemispheres throws these ganglia, as it were, into the shade, but still they exist; and we find that from the corpora quadrigemina and the neighbouring ganglionic substance, into which we may trace the chief terminations of the optic, olfactory, and auditory nerves, there issues a set of fibres which pass down through the corpora olivaria into the anterior columns of the spinal cord, and thence into the motor roots of the spinal nerves. The small size of these ganglia in man, and the small proportion which their motor fibres bear to those which originate in the cerebral hemispheres, is exactly what we should anticipate from the small proportion which his purely emotional and instinctive actions bear to those which are dictated and guided by his intelligence.

The cerebral system is inactive during sleep; and to it alone is due the sense of fatigue. Its diseases produce the effects which we should anticipate from its normal functions.

"Every derangement of the senses, every form of delirium or of coma, or of perverted imagination or judgment, every act of violence, must be referred to the condition, primary or secondary, of the cerebrum or cerebellum." (§ 120.) When the cerebrum is irritated, delirium ensues. When compressed, coma is induced. When lacerated, we have paralysis of *voluntary* motion. If other phenomena are seen in diseases of the encephalon, they arise from the extension of the influence of these to the true spinal and ganglionic systems, through *irritation* or *pressure*, *counter-irritation*, or *counter-pressure*; points of extreme importance, to be noticed very particularly hereafter." (§ 135.)

The connexion of disordered action of the cerebral system with disordered circulation is thus concisely stated by Dr. Hall:

"Not only undue arterial action and venous congestion induce morbid states of the cerebral functions, but the state of exhaustion from loss of blood, the anemious condition in chlorosis, &c. induce *similar* effects, and present to the physician anxious cases which frequently try his skill in diagnosis. Too great action of the minute arteries, congestion in the veins, an anemious state of the vascular system of the encephalon, alike induce morbidly exalted and impaired conditions of the mental and cerebral functions. Spectra, delirium, insomnia, amaurosis, stupor, coma, violent voluntary actions, or paralysis of the voluntary motions, these are the symptoms which arise out of these morbid conditions of the cerebral system and functions, and these only.

Spasmodic actions depend upon the fact of another system being implicated." (§§ 140-142.)

Dr. Hall's account of the therapeutics of the cerebral system is very brief. He confines himself to the indication of the fact that there are substances, such as green tea, alcohol, opium, &c. which have a specific effect upon the cerebrum; and to the quotation of M. Flourens' researches on this subject.

We next proceed to the true spinal or excito-motory system, of whose functions we shall give Dr. Hall's general account in his own words:

"1. Its anatomy involves a system of incident and reflex nerves connected with the true spinal marrow as their centre, unknown in this special connexion before. 2. Its physiology consists in functions, all of which are performed through this peculiar anatomy. These functions comprise all the acts of ingestion, of retention, of expulsion, and of exclusion in the animal economy; they are those therefore on which depend, 1st, the preservation of the individual, and, 2d, the continuance of the species. All these functions are reflex-spinal functions, of which the idea did not formerly exist. 3. Its principle of action is the *vis nervosa* of Haller, a motor power of which there was previously no application to physiology whatever. 4. Its pathology comprises the whole class of spasmodic diseases and its subdivision into those of, 1st, incident; 2d, centre; 3d, reflex, origin. 5. Its therapeutics are similar to its pathology; the various physical agents, especially severe cold, sometimes acting on the incident nerves, sometimes on the spinal marrow itself, sometimes on the reflex nerves." (p. 39.)

We have on former occasions expressed our want of accordance with Dr. Hall, in regard to the importance which he attaches to the *third* of the preceding principles, and also with respect to the absence of any preceding ideas of reflex action. We shall not therefore now revive these discussions; but simply express our full and hearty assent to the claim which he advances, of having entirely elicited this system, as a system, by his own laborious and persevering researches. The following is Dr. Hall's own tabular view of the *anatomy* of this system:

I. *The Incident or Excitor Branches.*

- i. The Trifacial, arising from
  1. The Eyelashes.
  2. The Alæ Nasi.
  3. The Nostril.
  4. The Fauces.
  5. The Face.
- ii. The Pneumogastric, from
  1. The Pharynx.
  2. The Larynx.
  3. The Bronchia.
  4. The Cardia, Kidney, and Liver.
- iii. The Glosso-pharyngeal?
- iv. The Posterior Spinal, arising from
  1. The General Surface.
  2. The Glans Penis vel Clitoridis.
  3. The Anus.
  4. The Cervix Vesicæ.
  5. The Cervix Uteri.

II. *The True Medulla Oblongata and Medulla Spinalis. The Centre of the System.*

III. *The Reflex or Motor Branches.*

- i. The Trochlearis } Oculi.
- ii. The Abducens } Oculi.
- iii. The Minor Portion of the Fifth
- iv. The Facial, distributed to
  1. The Orbicularis.
  2. The Levator Alæ Nasi.
- v. The Pneumogastric, or its Accessory.
  1. The Pharyngeal.
  2. The Œsophageal & Cardiac.
  3. The Laryngeal.
  4. The Bronchial, &c.
- vi. The Myo-Glossal.
- vii. The Spinal Accessory.
- viii. The Spinal, distributed to
  1. The Diaphragm.
  2. The Intercostal and Abdominal Muscles.
- ix. The Sacral, distributed to
  1. The Sphincters.
  2. The Expulsors, the Ejaculators, the Fallopian Tubes, the Uterus, &c.

The succeeding table conveys at a glance a complete view of the physiology of the true spinal system, according to Dr. Hall.

I. *The Excited Action.*

i. Of the Eyes, the Eyelids, (and of the Iris ?)

ii. Of the Orifices { 1. The Larynx. 2. The Pharynx.

iii. Of the Ingestion.
- 1. Of the Food.
  - *a.* In Suction ;
  - *b.* In Deglutition.
- 2. Of the Air, or Respiration.
- 3. Of the Semen or Conception.

iv. Of Exclusion.

v. Of the Expulsors or of Egestion.
- 1. Of the Fæces.
- 2. Of the Urine.
- 3. Of the Perspiration.
- 4. Of the Semen.
- 5. Of the Fœtus or Parturition.

vi. Of the Sphincters.
- 1. The Cardia.
- 2. The Valvula Coli ?
- 3. The Sphincter Ani.
- 4. The Sphincter Vesicæ.

II. *The Direct Action or Influence.*

i. In the Tone
ii. In the Irritability } of the Muscular System.

We think the last subdivision the most objectionable of the whole ; for it may be doubted whether the spinal system ever exercises the direct influence attributed to it by Dr. Hall. We have expressed ourselves on several former occasions, as quite opposed to the belief that the *irritability* of the muscular system has any relation of dependence upon any part of the nervous system ; and the facts which Dr. Hall regarded as indicating such a dependence have been shown by Dr. John Reid to be susceptible of, and indeed to require, a very different explanation.* And in regard to the *tone* of the muscular system, we think it may be questioned whether this is not dependent upon a reflex, rather than upon a direct action of the spinal cord. The following experiments prove the influence ; its nature remains uncertain :

"Two rabbits were taken: from one the head was removed; from the other also the head was removed, and the spinal marrow was cautiously destroyed by a sharp instrument ; the limbs of the former retained a certain degree of firmness and elasticity ; those of the second were perfectly lax. The difference was most obvious....The limbs and tail of a decapitated turtle possessed a certain degree of firmness or tone, recoiled on being drawn from their position, and moved with energy on the application of a stimulus. On withdrawing the spinal marrow gently out of its canal, all these phenomena ceased. The limbs were no longer obedient to stimuli, and became perfectly flaccid, having lost all their resilience. The sphincter lost its circular form and its contracted state, becoming lax, flaccid, and shapeless. The tail was flaccid, and unmoved on the application of stimuli."

It would be an interesting experiment, to divide the *posterior* roots of

* See Edinburgh Monthly Journal, May, 1841 ; and Brit. and For. Med. Rev., vol. XII. p. 277.

all the spinal nerves, instead of removing the cord. If, as we rather anticipate would be the case, the tone of the muscles were then lost, it would be evident that this state of moderate contraction is the result of a reflex action of the cord, excited by some impression conveyed from the muscle; and not a direct influence originating in the cord itself. We have thrown out this hint, merely for the purpose of simplifying the system; because it appears to us more likely to operate on *one* general principle, than in two distinct methods.

For a most luminous and interesting account of the various subordinate divisions of the reflex function, and of the nerves concerned in each, we must refer our readers to Dr. Hall's work; as it would not be consistent with our plan to go through these in detail. There are few physiological principles which are more susceptible of varied and important applications than those which he has here developed; and we earnestly recommend all our readers, who have not already done so, to make themselves well acquainted with them. There is no department of pathology which is capable of being so much improved by a good physiology as is that of the nervous system; and Dr. Hall's researches have not only pointed the way, but have carried us far towards our ultimate goal.

We must commence this part of the subject, by observing that the tables already quoted do *not* express the *whole* anatomy and physiology of the spinal system, but the most important aspects under which it is to be viewed, as ministering to the *organic* functions in man. Its operations appear to be in him more limited to the supply of the conditions requisite for the performance of these, than they are in the lower animals, with whose locomotive actions it often seems intimately connected. Thus it is probable that the regular movements of the wings of birds and insects are rather of a reflex than of a voluntary character; indeed they must be, if we admit that the cerebrum only is the centre of volition, since they will continue after its removal. The locomotive actions of man are under the complete control of his will; and those muscles of his general system which are not connected with the organic functions are only incidentally affected by reflex agency. But they must be all supplied by the spinal system of nerves; since we see that, in disordered conditions of that system, they are as strongly affected as any. Yet the groups of muscles connected with respiration, deglutition, and egestion, are the most liable to suffer from its disorders, as will be seen from the following table:

PATHOLOGY OF THE TRUE SPINAL SYSTEM.

I. *Diseases of the Incident Nerves.*

I. 1. Dental 2. Gastric 3. Intestinal	irritation in infants.	1. The crowing inspiration. 2. Strabismus, spasm of the fingers and toes, strangury, tenesmus, &c. 3. Convulsion. 4. Paralysis?
II. 1. Gastric 2. Intestinal 3. Uterine	irritation in adults.	1. Hysteria. 2. Asthma. 3. Vomiting, hiccup, &c. 4 Epilepsy. 5. Puerperal convulsion.

III. Traumatic tetanus, hydrophobia, &c.

II. *Diseases of the Spinal Marrow itself.*

i. Inflammation and other diseases.
ii. Diseases of the vertebræ and membranes.
iii. Counter-pressure, &c. in Diseases within the cranium.
iv. Centric epilepsy, tetanus, &c.
v. Convulsions from loss of blood, &c.

III. *Diseases of the Reflex or Motor Nerves.*

i. Spasm.
  1. Spasmodic tic.
  2. Torticollis.
  3. Contracted limbs, &c.
ii. Paralysis.

In epilepsy, tetanus, hydrophobia, hysteria, and the crowing inspiration of children, the symptoms are all of the same character; and the chief difference consists in the parts affected. Thus in *epilepsy*, besides the general convulsion, the expulsors are chiefly excited; and there is frequently at the same time spasmodic closure of the glottis, so that, in spite of violent expiratory efforts, the air is retained within the chest, the face becomes livid, and death may take place during the fit. *Tetanus* is characterized by the continued spasm of the muscles of the back, neck, and jaw. The muscles of deglutition are specially affected; and death usually results from the *fixation* of the respiratory muscles, so that asphyxia takes place. In *hydrophobia*, there is laryngeal as well as pharyngeal spasm; and death results from the closure of the glottis. In this disease, the emotional system of nerves seems to be more affected than in the preceding. The patient expresses a peculiar horror at the sight or sound of fluids; and the spasmodic action is brought on by *sensations*, more than by internal impressions. In that proteiform and perplexing disease, *hysteria*, almost all the varieties of spasmodic action are exhibited, even by the same patient at different times. Dr. Hall considers it a well-marked character of difference between epilepsy and hysteria, that, in the latter, however much the larynx may be affected, it is never closed; in epilepsy it is closed: in the former we have heaving, sighing, inspiration; in the latter violent ineffectual efforts at expiration, (§ 1679.) We have for some time had under our observation, however, an aggravated case of hysteria, which has exhibited this epileptic symptom in a very alarming degree; the face having often become quite livid, before the spasm of the glottis yielded. Dr. Hall might say that this symptom was indicative of epilepsy; but the case has nothing else in common with that disease, and has put on by turns all those variations which are so characteristic of aggravated hysteric disorder. It is one, too, in which the *emotional* character of this disease has peculiarly manifested itself. Dr. Hall very justly says that "hysteria seems to single out and affect every organ, every function which belongs to the true spinal system." Like the emotions, it also affects the action of the heart, the secretions, especially that of the kidney, &c. The *crowing inspiration* or croup-like convulsion of children bears a strong resemblance to epilepsy in the mode in which the muscles are affected. Its chief symptom is, as its name imports, a partial impediment to the inspiratory action; but as soon as this impediment increases, so as nearly to close the larynx, strabismus and general convulsions come on, with violent expiratory movements, and not unfrequently an affection of the sphincters.

This reference of all convulsive diseases to the spinal (including the

emotional) system of nerves alone, we believe to be one of the most important advances which the pathology of the nervous system has yet made; and the idea of it is due entirely to Dr. Hall. Not unfrequently the source of these diseases may lie within the cranium, or may be even in the brain itself; but it cannot act upon the muscular system, so as to produce convulsions, unless it affect, in some of the methods already indicated by Dr. Hall, either the tubercula quadrigemina, (the centre of Dr. Carpenter's emotional system,) or the medulla oblongata, or some other part of the spinal nerve. The experiments of Magendie and others, who have shown that no irritation of the cerebrum proper can directly produce muscular motions, form the basis of this view; which is borne out by a number of other physiological and pathological phenomena. A frequent source of convulsive action from cerebral disorder is undoubtedly pressure or irritation of the nerves of the dura mater, which is supplied by the fifth pair, and which is thus connected with the spinal system; for Dr. Hall has found that on pinching the dura mater of an animal just killed, violent convulsive movements were excited. It remains for intelligent practitioners to reobserve, on Dr. Hall's improved plan, the phenomena of all forms of nervous disease; and we shall be happy if, by this notice of Dr. Hall's views, we can excite more general attention to the subject. For the application of his principles to each particular form of disease, our readers must consult the work itself; which ought to have a place in every medical library. The diagrams with which it is illustrated are very ingenious; and will, to many, afford considerable aid in the comprehension of the text.

We have now the less pleasing task of laying before our readers an account of Dr. Arnold's brief treatise. The object of the writer (who is not *the* Arnold of neurological fame, but a brother possessing a much less extensive renown,) is evidently to upset the whole doctrine of the reflex function, as propounded by Dr. Marshall Hall; for he labours throughout to prove that what is new in that system is not true, and that what is true is not new. With this view, he brings together the facts and opinions bearing on the question, which had been promulgated by various older physiologists; and intimates that Dr. M. Hall and Professor Müller might have spared themselves much trouble, if they had been better acquainted with the labours of their predecessors. As we have fully gone over this ground on a former occasion, (vol. V, pp. 523-40,) and as we do not meet in Dr. Arnold's resumè, with any facts of importance which had been overlooked by us, (indeed we think he might have profited much by consulting *our* retrospect,) we shall dwell but lightly on this part of his inquiry; directing our attention rather to the arguments he adduces against the system as it now stands.

We cannot but remark, throughout this disquisition, a very loose mode of reasoning, both when the author is attacking the position of his opponents, and when he is endeavouring to establish his own. And though there is considerable ingenuity in many of his objections, yet they are often brought to bear, rather against his own inferences from the reflex doctrines, inferences which its chief supporters would probably not acknowledge, than against the fundamental dogmas of the system itself, as laid down by its author. Of this we shall presently meet with more than one example. There appears to us, also, to be a great deficiency

of precision and clearness in the use of abstract terms; and this fault, we may hint, is common to most German writers on physiology. They get on very well so long as their language applies to tangible things; but so soon as they quit the palpable they become obscure. We suspect that this fault has a good deal to do with the dreamy mysticism in which some of the most applauded among the German school of philosophy delight to indulge. We have often thought it a curious anomaly that men on the one hand so laborious in the accumulation of physical facts and in the investigation of their causes, and on the other so prone to indulge in metaphysical reasoning of the most abstruse kind, should be so deficient in the power of conceiving definitely, and applying correctly, the simple principles by which groups of phenomena are at once expressed, by which the investigation is conducted to new truths, and which alone can be relied on as guides in the application of physiology to practice. Yet so it unquestionably is; for though it is the fashion to cry up German physiology as immeasurably in advance of British,* we think it would not be difficult to show that in a truly *scientific* view, we are, and long have been, greatly in advance of our continental neighbours; our physiological facts being more completely reduced under the dominion of principles; and these principles being at once more definite in their character, and more precise in their character, than those which have held their sway in Germany. To draw an illustration from the present subject, which affords many such. From the time of Whytt, the doctrine taught in the University of Edinburgh respecting the sympathetic motions has been, that they are dependent upon the cerebro-spinal system; and that they result from the conveyance of a stimulus from the peripheral extremities of certain nerves to the central organs, where it excites an impulse which is transmitted downwards to the muscles. Dr. M. Hall has recently shown that no other part of the central organs is required for this purpose than the spinal cord; and he has also shown that sensation is not a necessary link in the chain of actions, as maintained by some. Now, with the exception of a few stray writers, like Prochaska, whose more correct opinions never seem to have excited any general attention, the almost universal opinion of German physiologists down to the time of Müller appears to have been, that the sympathetic movements are dependent on the sympathetic system of nerves, a doctrine which, as we need hardly tell our readers, is now quite exploded. But instead of receiving the simple and definite conceptions now presented to them by Dr. M. Hall, many of the German physiologists, and our author among the rest, flounder about in a slough of unmeaning, because indefinite, generalities; which after all seem to us to come much nearer to the doctrines they reject than their authors imagine.

The difficulty under which the physiologists of the German school labour in this matter is chiefly one of words; and as a great deal hinges on the meaning attached by them to the terms *sensation* (empfindung) and *perception*, we shall briefly examine it. Among British metaphysicians and physiologists the meaning of the former term has long been definitely fixed. They are all agreed, we believe, in regarding consciousness as a necessary element in sensation. Dr. Brown (in arguing against the doctrine of Dr. Reid, that consciousness is a distinct attribute of the mind)

* See recent articles in the Lancet.

thus expresses himself. "If, immediately after the first sensation, we imagine the sentient principle to be extinguished, what are we to call that feeling which filled and constituted the brief moment of life? It was a simple sensation and nothing more; and if only we say that the sensation has existed, whether we say or do not say that the mind is conscious of the sensation, we shall convey precisely the same meaning; the consciousness of the sensation being in that case only a tautological expression of the sensation itself." And Mr. Mill, the most recent and certainly one of the ablest of metaphysical writers, thus expresses the same view. In justifying the assertion that sensations are as truly states of mind as thoughts, emotions, and volitions, he says: "It is usual, indeed, to speak of sensations as states of body, not of mind. But this is the common confusion of giving one and the same name to a phenomenon and to the proximate cause or conditions of the phenomenon. The immediate antecedent of a sensation is a state of body, but the sensation itself is a state of mind. If the word mind means anything it means that which feels. If we allow ourselves to use language implying that the body feels, there is no reason against being consistent in that language and saying that the body also thinks."* The change which we call sensation is, in fact, neither more nor less than the consciousness of an impression. If we wish to describe a change taking place in the central organs of the nervous system, of which the individual does not necessarily experience consciousness, we cannot therefore employ the term *sensation* without contradicting its received acceptation. Yet Dr. Arnold accuses Dr. M. Hall of inconsistency in making the term sensation imply consciousness; and his chief argument seems to be, that Plato maintained that sensations are dependent upon the organs of sense, having special relations with the peculiar structure of these organs, so that neither of them can minister to the sensation receivable through the other; an argument of which we fail to perceive the applicability. Dr. Arnold, however, thinks that it enables him to refute many of the doctrines of the reflex theory. Yet, as will presently appear, the idea which he would substitute is essentially the same, the difference being chiefly in the language in which it is expressed.

In regard to the term *perception*, there is more latitude among scientific writers. Some employ it to designate a process distinct from sensation and consequent upon it; this process being the formation of that general notion respecting the nature of the object causing the sensation, which it is certain the sensation itself does not convey, and which is, in man at least, a complex process, involving acts of memory and association and comparison, although often, through repeated experience, performed in an incredibly short period of time. But some writers have employed it in a much looser sense, almost identifying it with sensation; whilst others have employed it in a still lower meaning, implying by it the simple reception of an impression, not even involving consciousness. This was the sense in which it was used by Glisson, who describes irritability (or what we now term vitality) as a *perceptio*, and speaks of sensation as a *perceptio perceptionis*, a definition which, interpreted as the author meant it to be, comes very near we believe to our own, the consciousness of an impression. It is in the same sense that the term is

* System of Logic, vol. ii. p. 497.

employed by the late ingenious Dr. Fletcher, who even extends it to the actions of different kinds of inorganic matter upon each other. "The thing acted on," he remarks, "must, even in this last case (that of inorganic matter), have *perceived* the influence of the thing acting, or it would not have obeyed this influence; the organized tissues in general must, in the second case (that of the ordinary phenomena of organic life,) have *perceived* the influence of the stimuli which they obey, or they would never have manifested this obedience; and it is only in another kind of *perception*, enjoyed by some only of the organized tissues, that sensation appears to consist, a perception so far elevated above that in which mere irritation (which always precedes it) consists, that a certain degree of consciousness attends it. *The being manifesting it not only perceives, but perceives that he is perceiving.*" This passage serves as a commentary on many expressions of Dr. Arnold and other German writers, which would otherwise be obscure. It shows that we are often to understand by the term *perception*, as used by them, merely the reception of an impression, not in the least involving consciousness. We shall hereafter see that Dr. Arnold attributes to the spinal cord the power of *perceiving* the impressions of external stimuli, and of *reacting* in accordance with them. We should like to know in what respect the property which he thus attributes to the spinal cord differs from the *excitabilité* of Flourens or the *reflex function* of Dr. M. Hall.

With these preliminary cautions we shall now follow Dr. Arnold through his examination, which consists of ten chapters. In the first he sets forth the *reflex theory of Dr. M. Hall and Prof. Müller.* The two theories, however, have many points of dissimilarity; and we shall confine ourselves for the present to that of Dr. M. Hall, reserving for a future occasion some critical remarks on that of Müller. Dr. M. Hall may fairly complain that his theory is not fairly stated by his antagonist; for Arnold has evidently drawn his chief materials from Dr. Hall's *first* memoir, and adopts positions which Dr. Hall has abandoned, besides omitting much important evidence subsequently adduced. Hence he commits the error of stating that Dr. Hall recognizes the four following species of muscular motion: 1, voluntary motion, having its origin in the brain; 2, respiratory motion, having its origin in the medulla oblongata; 3, involuntary motion, resulting from the application of stimuli to the muscles themselves; and, 4, reflex action, performed through the spinal cord and its nerves. This is the classification adopted by Dr. Hall in his first memoir; the authority of Sir C. Bell (it would seem) having led him to consider the respiratory movements under a distinct category. But before the publication of his second memoir, he had satisfied himself that they belong to the same class with the other reflex movements, with which he therefore associates them. Among the chief omissions in the representation of Dr. Hall's theory is that of due reference to the cases in which accident or disease has furnished the means of unequivocally proving, by reference to the human subject, that sensation is *not* a necessary link in the chain by which reflex actions are performed. Here again we find an absence of that reference to Dr. Hall's subsequent writings which his critic ought certainly to have made, before passing judgment upon opinions which were evidently crude, and intended by their author to be tested by subsequent research. We have ourselves always regarded these

cases as furnishing more satisfactory proof of the truth of that part of Dr. Hall's reflex theory which respects the absence of sensation, than any experiments on animals can do; for the inferences from these *must* be open, until they are thus confirmed, to the objection that we cannot be sure that consciousness and sensibility do not exist in the spinal cord, so long as the reflex movements can be excited through it.

The second chapter professes to give the *Views of several physiologists of modern times on the reflex theory.* Of these Volkmann is the first quoted; but as a summary of his opinions, with some critical remarks upon them, has been given in a former Volume (vol. VI. pp. 211-18), we need not here go over the ground again. The views of Nasse are of a similar character: Carus objects to the term reflex function, as implying a peculiar property of the spinal cord; and also to the assumption (as he regards it) of a system of excito-motory nerves distinct from the sensori-volitional; but he admits that the spinal cord contains, not merely primitive nervous fibres, but also a pulpy substance, which, like the gray matter of the brain or of the ganglia of the sympathetic, has the power of receiving impressions and of reacting in accordance with them. To this he attributes the movements which are performed by parts whose communication with the brain is interrupted. We confess ourselves at a loss to see in what essential respect this doctrine can be said to differ from that of Dr. Hall. Other criticisms, very much to the same effect, are extracted from the writings of Valentin, Kürschner the translator of Dr. Hall's memoirs, Prof. F. Arnold, and Klencke. Mr. Grainger's observations are very slightly alluded to; but more attention is given to the criticisms of Dr. Griffin, (Medical Gazette, vol. xxiv.) who particularly objects to that portion of Dr. Hall's views which respects the absence of sensation as a necessary link in the production of reflex movements. We have ourselves carefully weighed Dr. Griffin's arguments, but we fail to perceive their cogency. There appears to us to be much looseness in the manner in which the term sensation is employed; and in several portions of the paper we fancy that Dr. Griffin's ideas do not differ greatly from our own; but that he expresses them in different, and what seems to us incorrect, language.

In the third chapter our author takes the trouble to adduce several instances of the *Early use of the term reflex*, for the sake of proving that there is no novelty in Dr. Hall's application of it. We are not aware that he ever claimed any. No one knows better than Dr. Hall that a reflex function had been attributed to the central organs of the nervous system by Haller, Unzer, Prochaska, and many later physiologists; but he claims as his own the limitation of this function to the *true* spinal cord.

The title of the fourth chapter is the *Early knowledge of the facts on which the reflex-theory is founded.* We find in it nothing of any importance which our former historical summary did not include, and shall therefore pass it over without further notice. Nearly the same may be said of the fifth chapter, entitled *Reflex theories of physiologists before Marshall Hall.* An important fact, however, is mentioned respecting Haller, which we had overlooked, namely, that he expressly distinguishes the movements resulting from muscular irritability from those which take place when the head has been cut off or the brain removed; these last

continuing to manifest themselves as long as the spinal cord and medulla oblongata remain uninjured. (Elementa Physiologiæ, vol. iv. p. 337.) It is evident, then, that Haller was well aware of the dependence of the reflex movements upon the spinal cord alone; but that he could have had no clear idea as to the nature of the reflex action of this organ is evident, from the fact of his unwillingness to admit a distinction between the afferent and efferent (in common language, the sensory and motor) nervous fibres. It would appear, however, that Unzer came nearer the truth; and that in his Physiology, published in 1771, he expressed nearly the same opinions as those put forth not long afterwards by Prochaska, who is not quoted in Dr. Arnold's historical summary. The ideas of Whytt, Sir G. Blane, Legallois, and Burdach are successively adduced in support of the author's position that there is nothing new in the doctrine of the reflex function; but Flourens, who in our opinion approached it most nearly, is omitted. Referring those of our readers who may feel interested in the subject to the retrospective view of it which we formerly gave, we may here repeat that it cannot be questioned that the reflex theory, limited to the expression of it given by Dr. Hall in his first memoir, contained little that could be regarded as a decided advance upon the doctrines of previous physiologists, although none of them had ever exhibited them in the same combined form, or developed their extensive application. But it is on his conception of a system of nervous fibres appertaining exclusively to the *true spinal cord*, and ministering to the *reflex actions alone* (unless we admit them to be the instruments of the emotions also), physiologically distinct therefore from those which are concerned in sensation and voluntary motion, that Dr. Hall has rested his claim as a discoverer; and this claim we think it impossible to impugn. The only question is with respect to the validity of the conception; and on this we have already expressed our opinion.

The sixth chapter is on the *Distinction between the various kinds of muscular motion, according to Marshall Hall and John Müller.* The author's criticism upon Dr. Hall appears to us to be here chiefly of a verbal character. He objects, and with reason, to Dr. Hall's use of the term *involuntary* as designating the class of movements which depend upon the immediate application of the stimuli to the muscular fibre; since a large number of movements, including all those of the reflex system, depend upon other conditions. Dr. Hall obviously intended to refer to the movements of those muscles which have never anything else than an *involuntary action*,—the heart, for instance, and the muscular coat of the alimentary canal. The other muscles of the body are voluntary or involuntary in their action, according as the stimulus is conveyed to them by the sensori-volitional, or the excito-motor system of nerves. With the criticisms upon Müller's classification of muscular movements, we have at present nothing to do.

In the seventh chapter are considered the *Signification of the term reflexion*, and the nature of the *operation of the spinal cord concerned in reflex motion.* This part, also, of Dr. Arnold's criticism is rather verbal than philosophical. Dr. M. Hall has never attempted to specify the precise action which the spinal cord performs, when it gives rise to a motion in obedience to the stimulus of an external impression; he has merely employed the term reflex-function, as a general designation

for that peculiar power in the organ, to which are due a large number of phenomena agreeing in their essential conditions; and we cannot see what objection can be taken to this. In his later writings we think that he has laid himself open to criticism of this kind, in his employment of the term *vis nervosa*, as if it designated a known or appreciable entity. Dr. Arnold, presuming that the advocates of the reflex-theory imply by the term *reflexion* somewhat the same action on the nervous principle as do physical philosophers in regard to light, thus defines it: "We understand by it a *throwing back* of the nervous principle in the spinal cord, without at the same time admitting the existence of a change similar to sensation in the brain, or an effort in the spinal cord analogous to the will." We do not see why it was necessary to go in search of such a definition, when Dr. Hall has on so many occasions expressed his idea of the reflex function with the utmost perspicuity. Dr. Arnold then finds fault with the advocates of the reflex-theory, for not explaining what takes place in the spinal cord when motions are excited through its agency; but we do not see what right he has to do so, until he has explained what takes place in the brain when a sensation is produced there, or a voluntary motion originated. Certainly it behoves the advocates of the new system to be very guarded in the language they employ, so that they may not appear to assume that which they do not claim to know; whilst, on the other hand, they do not leave unexplained any phenomena for which their principles will account. We do not see how any charges of this kind can be fairly brought against Dr. M. Hall; but certainly Dr. Arnold makes out a strong case of inconsistency against Professor Müller. Into this, however, we need not now enter.

The title of the eighth chapter is *Sensation and its seat*, and also its *connexion with reflex motions*. Dr. Arnold commences with the argument for the possibility of sensation without consciousness, on which we have already made some observations. He then goes on to show that Dr. M. Hall's experiments, on which that physiologist originally rested his assertion that sensation is not necessarily involved in reflex actions, are not conclusive; and on this head, as we before observed, we agree with him. But Dr. Arnold does not refer to those other and more convincing proofs which Dr. Hall's second memoir contained. And he considers that conclusion drawn from Dr. Hall's experiments respecting the absence of sensibility in parts of the body cut off from connexion with the brain by section of the spinal cord is true only when the term sensation is restricted to consciousness united with perception (Wahrnehmung.) We believe that we are here to understand this last word in the higher sense in which the term perception is employed in this country; and to regard our author as meaning that the spinal cord is sufficient for what *we* term sensation, and that communication with the brain is only required for the mental changes consequent upon it. If this be his view, it is entirely contradicted by the cases of injury of the spinal cord in the human subject, to which we have already so frequently alluded. But if Dr. Arnold uses the term in its lower sense, he then says nothing different from Dr. M. Hall, namely, that when communication with the brain is cut off the consciousness of impressions made upon the parts of the body thus separated is abolished.

The ninth chapter contains a critical view of the opinions of Dr. M.

Hall, and Professor Müller on the *physiology of the spinal cord, so far as it stands in relation to the reflex theory.* Here, as elsewhere, we shall restrict ourselves to the comments upon the views of Dr. Hall. As already stated, the organ commonly designated as the spinal cord is believed by Dr. Hall to be composed of two parts: the intravertebral cord, which connects the nerves with the cerebrum; and the true spinal cord, the axis or centre of the excito-motor system of nerves. He confesses his inability, however, to isolate these parts anatomically in vertebrated animals; but he thinks it probable that they may be found distinct in the invertebrata; and he adds, "but if the existence of a *distinct* anatomy of the excito-motory system be doubtful, that of the *blended* anatomy, and that of the distinct physiology, pathology, and therapeutics of that system, are perfectly obvious." Dr. Hall has never claimed for himself the discovery of the *structural distinctness* of the two systems; but has constantly put forth his arguments in proof of their *physiological* distinctness. Now we cannot but regard this distinction as more verbal than real. The question after all comes to this. Do the same nervous fibres minister to the reflex function and to sensori-volitional changes; or are these performed by different sets of fibres? If they *are* performed by the *same*, then the two systems cannot be regarded as physiologically distinct; and many of Dr. Hall's arguments in favour of the reflex-function fall to the ground. If the two sets of actions are performed by *different* fibres, then the systems *are* structurally distinct; though the two classes of fibres may be everywhere bound up in the same sheath, and may not be distinguishable at their central origins or terminations. It is not getting out of the difficulty, in which the advocates of the reflex-function are placed, by the want of a clear demonstration of the excito-motor system in the spinal cord of vertebrata, to say that such a demonstration is not required, since only a *physiological* distinctness is claimed; and we do not think that Dr. Hall has done wisely in leaving the question in this state. He certainly lays himself open, thereby, to the criticism of Dr. Arnold; who asserts that all the facts adduced by Dr. Hall may be explained upon the property of the spinal cord long known to physiologists; and upon the supposition, that the conveyance of sensory impressions and of those destined to excite reflex actions, on the one hand, and the transmission of voluntary and reflex motor impulses on the other, are accomplished by the same nerves. The following are his principal arguments, on which we shall comment as we proceed.

1. The skin, which is the organ for the reception of sensory impressions of such variety and acuteness, is also the organ through which the stimuli that excite reflex actions make their strongest impression. Hence it may be concluded that the nerves which go to the skin are both sensory and excitor.

2. If we admit the existence of distinct sensory and excitor nerves, we must admit that both kinds are distributed even in the minutest ramifications; since every portion of the skin is capable of receiving and transmitting both sensory and excito-motor impressions. And in the same manner we must admit that the minutest twigs supplying the muscles contain a cerebral fibre and a spinal fibre.

We own ourselves unable to see the force of the objections adduced in these arguments. There are very obvious reasons why the general

exterior surface of the body should be endowed alike with the capability of receiving sensory impressions and the stimuli to excito-motor impulses; and there can hence be drawn, therefore, not the shadow of an argument against the distinctness of the two sets of nerves. If we *universally* found sensibility and the power of exciting reflex actions associated in the same degree, there might be then more ground for the assumption that they act through the same nerves; but this is by no means the case. For whilst irritation of the *trunk* of a sensory nerve occasions acute pain, it does not excite reflexions in anything like the same degree; and sensibility is often abolished by disease, when the reflex actions are unusually excitable. Knowing that the minutest ramifications of the cutaneous and muscular nerves almost invariably (perhaps invariably,) contain two or more ultimate fibrillæ, we cannot see any force in the second of Dr. Arnold's arguments; and shall therefore at once pass on to the next.

3. When the posterior roots of the nerves going to the posterior extremities have been divided, not only is sensibility abolished in these limbs, but the excitability to reflex actions; and when nux vomica has been given to the animal, no tetanic spasms occur in these parts. In support of this last assertion some interesting experiments are detailed.

As it is universally believed, by the advocates of the reflex doctrine, that the excitor fibres are bound up with the sensory in the posterior roots of the spinal nerves, we cannot see the least force in the first part of this argument; since, as Dr. M. Hall has frequently shown, the reflex actions cannot be excited if the continuity of the nervous circle, consisting of the excitor nerve, the spinal cord, and the motor-nerve, be anywhere interrupted. The absence of the tetanic spasms which succeed the administration of nux vomica from limbs whose sensory nerves have been divided, is an interesting fact, but one not in the least militating against the doctrine. It shows that the effect of the poison is to produce a high degree of *excitability* of the spinal cord; but that the convulsive action is immediately dependent upon some stimulus, (perhaps of the slightest possible nature,) communicated through the excitor nerves. Hence in this artificial tetanus, the spasms are not of central but of peripheral origin.

4. The next argument, or rather series of arguments, is drawn from other effects of nux vomica. Dr. Arnold considers that the fact of its exalting at the same time the sensibility and the excitability is a proof that the excitor and sensory fibres are the same; and that the following points should be kept in view in reasoning upon it. A. The increased susceptibility to irritation, from the action of nux vomica, still exists after the removal of the head or brain, so that convulsive movements or rigid spasm occur, especially in amphibia; and this happens even if the nux vomica be not applied until some time after the removal of the brain. It hence appears that the medulla oblongata and spinal cord are capable of being specifically acted on by this irritant, quite independently of the brain. B. After the injury or partial removal of the medulla oblongata, however, the (so called) reflex motions are much weaker, and sooner come to an end, than after mere decapitation of the animals. The same thing occurs with respect to the action of nux vomica. It appears from Dr. Arnold's experiments on frogs, that

the removal of one lateral half of the medulla oblongata does not suspend the excitability of the spinal cord by nux vomica, but diminishes its duration; the removal of a small portion of the upper extremity of the medulla oblongata seems to delay the occurrence of the tetanic spasms occasioned by the nux vomica, and renders the convulsive movements less marked (especially in the posterior extremities) as well as more transient; whilst after the division of the medulla oblongata transversely, the tetanic spasms either do not present themselves at all, or but very slightly. c. After the complete removal of the medulla oblongata, the reflex motions diminish more evidently, both as to energy and duration, than after mere injury of that part. This is also the case with the tetanus occasioned by the application of nux vomica previous to the removal of the medulla oblongata. d. The medulla oblongata, which is the channel through which the sensations of a large part of the body are produced, is the point on which the effects of nux vomica are chiefly manifested. If the nux vomica be not applied until after the complete removal of the medulla oblongata, no convulsive movements, according to Dr. Arnold, are produced by it; whilst if the removal be delayed until after the nux vomica has been applied, and the tetanic state resulting from it has set in, the tetanus then continues.

From these facts it might be concluded, that the spinal cord has no independent powers of its own; and that it reacts only so far as it has received a *charge* from the medulla oblongata. Such a view, however, as Dr. Arnold admits, could be only partially correct; since reflex motions can be excited in portions of the spinal cord that have been long completely separated from the medulla oblongata; and there are various agents which are capable of increasing its excitability, so as to give rise to violent spasmodic actions, after the complete removal of the medulla oblongata. It is curious that strychnine, when employed in its isolated state, should be one of these. We must avow ourselves at a loss to comprehend what bearing these experiments have upon the question of the distinctness of the excito-motor system of nerves; they only point to what is certainly a curious fact,—that the influence of nux vomica is primarily exerted upon the medulla oblongata, and that it extends from this to the rest of the spinal cord.

In the last chapter, entitled "Facts which the observation of nature presents in regard to the so called reflex-function, and inferences from these," we have a summary of Arnold's own researches and conclusions, of which we shall give a full translation, as well in justice to the author, as with the desire to show how the subject is treated among learned physiologists of the German school:

"Unprejudiced observation of the so-called reflex-motions in decapitated animals, presents the following results.

"1. If the convulsive movements, which often follow the decapitation, are over, or if the animal has recovered itself from the paralytic state which frequently succeeds the operation, the subsequent movements are such as result from the application of external stimuli. It is true that, in decapitated frogs, spontaneous (selbständige) movements sometimes occur; but they only take place when the frogs find themselves in unwonted positions, and they result from their endeavours to attain their ordinary position, or one more convenient. Such movements, however, are only spontaneous in appearance; for they are nothing else than the reaction of the bodies played upon.

"2. The motions of decapitated animals, which result from the action of external stimuli, bear the character of *adaptiveness* in themselves, and, when in combination, they indicate internal combination and harmonization. We may generally see in these movements, that their object is to remove the source of the external irritation; and frequently for the attainment of this object a powerful movement is executed. If, for example, a decapitated frog be laid hold of in the back with the forceps, one of the hind-legs is violently moved to the place, and grasps the forceps. Frequently an unmistakeable purpose shows itself in the movement, that of withdrawing itself completely from the stimulus. The animal extends the irritated limb, and the frog jumps away, when the stimulus operates with sufficient power.

"3. The so-called reflex motions are more energetic in decapitated than in undecapitated amphibia. It frequently happens that, in undecapitated frogs, no external stimuli produce motions, when for instance the animals perceive that they cannot immediately withdraw themselves from the stimulus; but after cutting off the head, or removing the brain, determinate and adaptive motions generally follow the action of an irritant on the skin, as soon as the animal has recovered its tranquillity, and its irritability has become re-established.

"4. The liveliness of the movements is increased by those agents which suspend or repress the activity of the brain. If opium be given to an undecapitated frog, it is the result of my experiments that, together with the state of stupor, which prevents the exercise of the will and the free selection of its movements, there is an increased susceptibility in the skin to the action of external stimuli, and consequently a greater liveliness in the so-called reflex motions; there even follow tetanic symptoms, like those resulting from the operation of nux vomica. But in decapitated amphibia, or in those from which the brain has been removed, opium in similar doses no longer increases the irritability of the skin or the movements excited by external stimuli.

"5. Such influences as possess a specific effect in stimulating the medulla oblongata and spinal cord, nux vomica for instance, exalt the sensibility in uninjured animals, and in the same way heighten in decapitated animals the susceptibility of irritation, especially in the skin, and hence aggravate the movements which follow external stimuli, the so-called reflex actions.

"6. The character of the movements which present themselves in decapitated animals depends chiefly upon the state of the spinal cord induced before their decapitation, by influences derived from the brain or the medulla oblongata. They are also modified, in some degree, as to direction, strength, activity, and duration, by the strength or peculiarity of the irritating cause. Thus it is well known that many animals, especially birds, will continue to run, after decapitation, if their heads be cut off whilst they are running. [The facts respecting the different action of nux vomica, according as it is introduced before or after the removal of the medulla oblongata, are here again introduced; and it is admitted that the circumstance of tetanus being induced by strychnine applied after this operation, is an argument for the independence of the spinal cord.]

"7. The so-called reflex motions do not cease to affect the body generally, though the spinal cord be cut through on one side even to the median line, or though a piece of it be entirely removed from one side. In like manner, a longitudinal division of the spinal cord does not prevent the extension of these motions to all the muscles on both halves of the body, provided that some part of the spinal cord remains undivided on the median line.

"8. Section of the posterior roots of the spinal nerves checks the so-called reflex motions. If the nerves of the posterior extremities be thus partly separated, and the spinal cord itself be divided transversely in the middle of the back, the posterior extremities will remain perfectly motionless, which is not the case if the sensory nerves be left without injury.

"9. The strength and extension of the motions from irritation, or the so-called reflex movements, depends on the degree of excitability and on the strength of the irritating cause. But when the susceptibility to irritants is con-

siderably increased, the motions lose the character of adaptiveness, and present that of spasm.

"10. The so-called reflex actions are chiefly produced by irritations of the skin, conjunctiva, and the mucous membrane of the nose, mouth, and throat. No determinate adaptive movements are produced by irritation of the thoracic or abdominal viscera. Even after irritation of the muscles and nervous trunks, only spasmodic twitchings take place, without any character of adaptiveness or harmonious accordance."

From these facts, Dr. Arnold thinks that he is warranted in drawing the following conclusions:

"1. A power of being influenced* by external irritants has its seat in the spinal cord, in a certain degree independently of the brain and medulla oblongata, *the perception-power of the spinal cord.*

"2. This power in the spinal cord relates not merely to the irritant in general, but also to the nature, the degree, and the locality of it; but the faculty of perception allied with consciousness is wanting.

"3. With the perception-power stands in next relation that faculty of the spinal cord, by which it reacts in accordance with the excitement produced by impressions, so as to perform adaptive, combined and harmonious movements; *the reaction-power of the spinal cord.*

"4. These motions are evidently adaptive and harmonious; but they want the character of freedom. They are not external manifestations of a will.

"5. The spinal cord possesses in a slight degree only the faculty of giving origin to spontaneous motions. If in decapitated animals self-originating motions do occur, they are principally the result of a disposition or excitation, which the spinal cord has received from the brain or medulla oblongata previously to the decapitation.

"6. The degree of the perception-power of the spinal cord depends on a peculiar disposition (Stimmung) of this organ, which is induced in it principally by the medulla oblongata, and which, without it, can be manifested by this organ only in a very slight degree. The same may be said of the rapidity and energy of the motions excited by external irritations.

"7. The disposition thus produced in the spinal cord remains in it for some time, even when it is separated from the brain and medulla oblongata; and even in separated parts of it.

"8. The change which takes place in the spinal cord during the reception of external impressions, and the determination of movements consequent upon these, is analogous to that which takes place in the brain during conscious sensations and voluntary motions; only that clear consciousness (klares Bewusstsein) and freedom of will are wanting to it, whilst the character of adaptiveness and of harmonious accordance appertains to it in the highest degree.

"9. The impressions (Eindrücke) which the central organs receive through their nerves produce in them a disposition corresponding to the quality of the impression; depending both on its own nature and on the nerve by which it is received and conducted to the central organs of the nervous system; whereupon these organs react in a corresponding manner.

"10. A mere transference of the nervous principle from sensory to motor fibres does not take place in the spinal cord. The term *reflex-function* does not indicate what really takes place in the organ during the motions occasioned by external irritations.

"11. With regard to the conducting faculty (Leitungsvermögen) of the spinal cord, observation goes to show that it is in its *totality* that it imparts the disposition it has received, either from the brain and the medulla oblongata on the one hand, or from the nerves on the other. After the arguments formerly

* This we believe to be the real meaning of the original: "Ein Vermögen, aüssere Reize inne zu werden, hat in dem Rückenmarke," &c. The author does not seem to imply what *we* term *sensation;* as appears from the next position.

adduced, it can no longer be admitted that the fibres of the spinal cord conduct impressions in an isolated manner, as do those of the nerves.

"12. It is not the number of the muscles moved that is determined by the central organ; but the object which is to be attained. A *Clavier-theory*,* such as has been introduced of late years, has no facts in favour of it, and many against it."

It will be evident, we think, to any candid reader of the preceding extract, that Dr. Arnold admits the essential principles of the reflex-theory whilst apparently dissenting from it. Many of the objections he adduces bear rather against the ideas he has himself attached to it, than against the theory itself, as propounded by Dr. Hall, and sustained by his followers. Thus we are not aware that any one has spoken of "a mere transference of the nervous principle from sensory to motor fibres" as the entire change to which the spinal cord is subservient in reflex action; or has attempted to explain the operations which really take place there. It must be admitted that some of Dr. Hall's expressions regarding the course of the *vis nervosa* along the incident nerves, and its reflexion by the spinal cord along the motor nerves, do give an appearance of validity to Dr. Arnold's objections; and we have always regretted that Dr. Hall should use language which appeared so capable of misrepresentation, whilst it did not give the least assistance in the explanation of the phenomena to which his theory applies. But Dr. Hall knows, as well as we do, that the true spinal cord has properties peculiar to itself, and that it is something very different from a mere centre of communication between incident and motor fibres. Although it might seem quite possible to explain the *ordinary* reflex movements by such communications, especially since the incident and motor nerves, through which these are executed, are almost invariably connected with the same portion of the ganglionic centre, yet this mode of explanation can scarcely be regarded as applicable to those abnormal convulsive actions in which almost every muscle in the body is thrown into violent action from the effect of a stimulus conveyed through a single excitor nerve. This is obviously the case, for example, in traumatic tetanus, and in hydrophobia, where we can distinctly trace the source of the irritation. In epilepsy and hysteria, the local cause of the convulsive actions is often more obscure, but it is not the less real. But in these disorders the convulsive movements are not limited to particular groups of muscles, such as might be imagined to be connected with the incident nerves that excite them; they affect the whole body, and especially the respiratory system. It is evident that in these cases, as in the artificial tetanus induced by strychnine, the whole spinal cord is in a state of undue irritability, so that any stimulus conveyed to it will react, not through one or two nerves only with which the incident fibres may have the closest relations, but through the gray matter of the cord, which gives to it its peculiar character as a ganglionic centre, and constitutes the medium of communication between the incident and motor *systems* of fibres. We know nothing of the operations of this gray matter, any more than we are acquainted with the precise mode in

* Dr. Arnold here refers to the doctrine propounded by Müller, that the will acts upon the motor nerves through the brain and medulla oblongata, as a performer on a keyed instrument produces what sounds he pleases by striking the appropriate keys.

which changes are propagated along the white fibres; but we have good reason to believe that whilst each *fibre* of the *white* matter is completely isolated from the rest so as not to communicate to it any influence which it is conveying; the ultimate constituents of the *gray* matter are not so isolated but that they are able to propagate to each other (to a greater or less extent, according to circumstances) the changes they are undergoing. Now we will suppose that a certain tract of gray matter intervenes between the termination of a given incident nerve and the origin of the motor nerve by which it ordinarily reacts; the stimulus conveyed by this incident nerve usually produces such a change in the tract of gray matter, that it excites the respondent motion, and that only. But supposing the spinal cord (by which we understand its gray matter or ganglionic substances) to be in a state of unusual irritability, then the operation of this stimulus will spread itself to other parts of the gray matter, perhaps even to a considerable distance, and will produce respondent motions in a great variety of muscles. This hypothesis appears to us to account sufficiently well for observed phenomena to possess a claim to be received as their explanation, until something more definite shall be known respecting the real changes which take place in the nervous system, during its active operations. It will be found, we think, perfectly conformable with all the facts adduced by Dr. Arnold, against what he erroneously believed to be the essential principles of the reflex-theory.

Art. XI.

Ueber die Veränderungen des Scheidentheiles und des unteren Abschnittes der Gebärmutter in der zweiten Hälfte der Schwangerschaft. Von Dr. Fr. H. G. Birnbaum.—*Bonn*, 1841. 8vo, pp. 94.

An Essay on the Changes which the Cervix and Lower Segment of the Uterus undergo in the second half of Pregnancy. By Dr. F. H. G. Birnbaum.—*Bonn*, 1841.

The alterations of the cervix uteri have been long, and with propriety, regarded as affording one of the most valuable signs of the existence of pregnancy. Their importance would, however, be greatly increased if they afforded a means of judging of the date of pregnancy as well as of its existence, and many German writers on obstetric subjects have, since the time of G. W. Stein, described these phenomena as occurring with most unvarying regularity, and as indicating exactly the period of utero-gestation. In England, on the contrary, most authorities speak of these changes as observing no constant order, and caution the inexperienced from attempting to diagnosticate the stage of pregnancy from the condition of the cervix uteri, since, as Smellie observes, "The neck of the womb will in some be felt as long in the eighth as in others at the sixth or seventh month." This discrepancy of opinion on a subject of considerable importance did not give rise to any fresh investigations, but some espoused one side of the question, others a different one, and not a few qualified their statements, so that it was not possible to deduce from them any definite conclusion. Stoltz, indeed, made it the theme of an essay in his "Considérations sur quelques points relatifs à l'art des ac-

couchements," and Kilian briefly notices it in his *Geburtslehre.* The latter differs both from Stoltz and from Stein, and makes some very just remarks on the circumstances which have led others into error; but the grand conclusion to which his observations lead, is that the whole question stands in need of a thorough critical investigation.

To this task Dr. Birnbaum has applied himself, and the tract before us contains the results of his inquiries, which appear to have been conducted with great painstaking and conscientiousness. He kept records of the frequently repeated vaginal examination of fifty-four primiparæ, and compared with them the results obtained in the case of women who had already given birth to children. The changes in the cervix uteri of multiparæ appear to take place with far greater irregularity than in persons who have never given birth to children; and the fissures in the os uteri of the former are a further source of confusion to those who wish to draw any inference as to the stage of pregnancy from an examination per vaginam. But there are circumstances which may lead to mistake, even in the case of women who are pregnant for the first time. Dr. Birnbaum observes in § 3, that sometimes the vagina is attached to the cervix uteri so low down, as to leave only a small portion of the lip of the uterus, perhaps not more than a line in length, projecting into its cavity. This low insertion of the vagina in general exists only at the anterior lip of the uterus, but occasionally its attachment around the posterior part of the cervix uteri presents the same anomaly, in which case a person may readily fall into the error of supposing pregnancy to be very far advanced, when in reality it is not by any means near its termination. In cases of this sort the true condition is at once detected by pushing up the roof of the vagina with the finger, when the cervix uteri will be felt above it long, firm, and but little developed. Another source of error of even more frequent occurrence is furnished by the development of the base of the cervix uteri, which produces an apparent shortening, distinguishable at once from real obliteration of the neck of the womb by passing the finger within its cavity.

The degree of shortening of the cervix uteri indeed appears liable to modifications from so many causes, that it affords but little clue as to the stage of pregnancy. In muscular and athletic women the lower segment of the uterus becomes developed less readily, and the shortening of the cervix uteri takes place at a later period than in females of a more delicate frame, while the firm and unyielding abdominal parietes prevent the uterus from sinking down into the cavity of the pelvis until an advanced period of pregnancy, or even until the commencement of labour. In such persons too the base of the cervix becomes enlarged, and the difference between apparent and real shortening of the neck of the womb is often extremely remarkable. In women of a directly opposite temperament, those in whom there is great laxity of fibre, and who are the subjects of leucorrheal discharges, it likewise often happens that the cervix remains long, cylindrical, very dilatable, and with an open os uteri for many weeks. Often indeed it is scarcely at all shortened till labour actually sets in, when its dilatation occurs in an almost mechanical manner. There are also other causes of a more local nature which modify the changes of the lower segment or neck of the uterus, as "the excessive

distention of the uterus, pendulous abdomen, too great or too slight inclination of the pelvis, contraction or malformation or excessive width of the brim of the pelvis; unnatural presentations of the child, or any cause which keeps the head high above the brim of the pelvis." (Sect. 8, p. 40.)

We must refer our readers to the work itself for an account of the effects produced by each of the above-mentioned causes, while we glance at another point on which great diversity of opinion has existed, namely the degree of opening of the os uteri during pregnancy, and the mode in which this opening is effected. Roederer, the Steins, Montgomery, and Kilian speak of a patulous state of the external os uteri, as occurring at an early stage of pregnancy, while Osiander, Baudelocque, and Stoltz hold a directly opposite opinion. Dr. Birnbaum's observations on this subject are both interesting and important:

"Usually," he says, "we find at an early stage of pregnancy, soon after the lapse of the first half, but at a period so uncertain that it cannot be stated positively, a commencing separation of the lips of the uterus which had been in contact up to that time. First of all a small shallow funnel-shaped depression is formed, which just admits the point of the index-finger, as may be seen by examination with the speculum. This gradually opens higher and higher so as to form a canal, narrowing towards the top and slightly curved in its course. As the opening advances, the tip of the finger reaches the superior aperture of the canal, and is prevented from advancing further only by a thin, almost membranous ring, which contracts firmly on every attempt to pass the finger beyond it. By degrees the edges of this opening yield to the finger and the cavity of the cervix uteri is now found to be a canal of uniform dimensions, at the end of which the membranes of the ovum may be reached." (Sect. 17, pp. 61-2.)

To this rule, however, there are many exceptions, and the opening of the cervix uteri from within outwards, as described by Stoltz, is occasionally observed, while in other cases the external os uteri will at one time be found most dilated, at another the internal, without our being able to assign any reason for the variation. Stoltz laid it down as a rule, that in first pregnancies the neck of the womb becomes effaced from within outwards, and that in subsequent ones the process takes place from without inwards. Dr. Birnbaum's researches have led him to the conclusion that this statement is altogether erroneous; that the obliteration of the cervix uteri from within outwards is always an exception to the usual course of things, and that it is by no means characteristic of first pregnancies. It may indeed be asserted that there exists no characteristic difference between the changes in the uterus of primiparæ and multiparæ with the exception of those which are produced by fissures of the os and cervix, and their cicatrices. The opening of the cervix from within outwards occurs as well in first pregnancies as in the case of women who have had children; in both it is unusual, but it is rarer in the latter.

The alterations in the form of the os uteri are regarded rather as characteristic of pregnancy than as indicating its stage. To one of these changes, the alteration of the form of the orifice from transverse to circular, great importance has been attached by some writers. Dr. Birnbaum examined the os uteri with the speculum in thirty-six women who were pregnant for the first time. In twenty-three the os uteri was a transverse opening, which retained completely the character presented by the unimpregnated uterus in two instances. In nine it was circular,

and in four it approached the round form, but was oval rather than circular. These facts satisfactorily prove that the circular form of the os uteri cannot be regarded as of much value as a sign of pregnancy. Montgomery noticed that the os uteri often *felt* circular, and Dr. Birnbaum says that he has often convinced himself by an examination with the speculum that the orifice of the womb often *feels* circular when its form is in reality transverse. The apparently circular form of the os uteri depends in a considerable degree on the mucus which fills its cavity, and which often differs in quantity, thus producing frequent changes in the form of the mouth of the womb.

The above are the chief points illustrated in this essay; we could have wished that the facts it contains had been better arranged and stated in a more lucid manner. The first sixteen pages are occupied with details of the vaginal examinations on which Dr. Birnbaum grounds his statements; but their results are not thrown into tables or made in any way available to the reader, so that except in so far as they show that the author has not dealt in mere assertions, they might as well have been omitted. Fearing that some of his conclusions may have escaped our notice, we give, in Dr. Birnbaum's own words, the summing-up of his inquiries:

"1. Of all the changes induced by pregnancy, the relaxation of the cervix uteri is the most variable and uncertain. Usually, however, it takes place from without inwards, or from the point to the base, very rarely in the opposite direction, or from the base to the apex.

"2. With the relaxation of the cervix the opening of its canal is intimately connected. This, too, generally takes place from without inwards, very seldom from within outwards; but often it alternates from the one to the other. The mode in which it occurs is not characteristic of first or subsequent pregnancies, but the opening of the cervix from within outwards, which is always a rare phenomenon, appears to be much more unusual in multiparæ than in women who are pregnant for the first time.

"3. The shortening of the cervix uteri is partly apparent, owing to growth and distention, partly real, in which case it results from the disappearance of its middle part. Real shortening of the cervix is most distinctly recognized by thinning of the lower segment of the uterus, and shortening and widening of the canal of the cervix.

"4. The final obliteration of the cervix uteri is produced either by its walls being drawn apart in the progress of development of the lower segment of the uterus, or by a kind of process of inversion by which the internal undilated os uteri is driven through the external. This inversion is possible, though rare during pregnancy, but is by no means unusual in labour if the lower orifice is very lax, and the upper very resistent.

"5. The internal and external os uteri then never pass into each other, but one orifice alone remains, which is in most cases the external os, but in some few instances the internal.

"6. With the exception of the fissures and cicatrices of the os uteri, there exists no invariable difference between the occurrences in first and in subsequent pregnancies. In the former, however, greater and more rapid changes usually occur in the lower segment of the uterus than are observed in persons who have already given birth to children." (pp. 83-4.)

Art. XII.

Pharmacologia; being an extended Inquiry into the Operations of Medicinal Bodies, upon which are founded the Theory and Art of Prescribing. By J. A. Paris, m.d. Cantab. f.r.s. &c. *Ninth Edition, rewritten, in order to incorporate the latest discoveries in Physiology, Chemistry, and Materia Medica.—London*, 1843. 8vo, pp. 622.

The author commences his work with an elegant oration on the Revolutionary History of the Materia Medica; and gives a graphic description of the twin sisters Superstition and Credulity.

"Credulity," he says, "has been justly defined belief without reason. Scepticism is its opposite, reason without belief, and is the natural and invariable consequence of credulity; for it may be generally observed that men who believe without reason are succeeded by others whom no reasoning can convince; a fact which has occasioned many extraordinary and violent revolutions in the materia medica, and will explain the otherwise unaccountable rise and fall of many useless as well as important articles.........By bestowing unworthy and exaggerated praise upon a remedy, we in reality do but damage its reputation, and run the risk of banishing it from practice; for, when the sober practitioner discovers by experience that a medicine falls so far short of the efficacy ascribed to it, he abandons its use in disgust, and is even unwilling to concede to it that degree of merit, to which in truth and justice it may be entitled; the inflated eulogiums bestowed upon the operation of digitalis in pulmonary diseases excited for a time a very unfair impression against its use. It is also well known with what earnestness the profession regarded the expectations raised by Störk, of Vienna, in 1760, with regard to the efficacy of hemlock.........In the earlier editions of this work, I predicted the fate of the cubebs, which had been restored to notice with such extravagant praise and unqualified approbation; who now places any confidence in the specific powers of that substance in the cure of the disease for which it was considered a never-failing remedy? May the advocates for the virtues of iodine, and of other more recently introduced remedies, derive a useful lesson of practical caution from these precepts!" (pp. 38-9.)

We give Dr. Paris full credit for his prophetic spirit, and agree with him that the remedial powers of both of the last-mentioned substances have been grossly exaggerated. We also entirely coincide with him in the observation which follows, and the truth of which we shall plead in justification of the manner in which we shall treat his own work:

"As we are investigating the follies of physic, it will not be foreign to the subject, to observe that the above remarks may, with as much truth and force, be applied to medical writings as to medical substances; nothing is more fatal to the permanent character of an author than the extravagant and unmerited encomiums of the reviewer, superlatively lavished on inferior claims." (p 39.)

Under the head "false theories and absurd conceits" the homœopathists are ably dealt with, and treated according to their deserts. The subjects of the "application and the misapplication of chemical science" "devotion to authority and established rule, &c." are next ably discussed, and are followed by a discussion on the "ambiguity of nomenclature." In this chapter the author has made some rather caustic remarks upon the Edinburgh Pharmacopœia. We are fortunately not an interested party in this controversy, and can therefore regard the dispute without feelings

of prejudice or prepossession. In a former Number we stated the grounds upon which we preferred a national pharmacopœia in the vernacular tongue, to that of one written in a dead language, and we have met with nothing in the arguments of Dr. Paris to shake our belief in the preponderating advantages of the latter. And although we by no means approve of several of the names adopted in the Edinburgh Pharmacopœia, it is almost a self-evident proposition that permanency of name will secure the greatest accuracy in the preparation of prescriptions. In corroboration of this view we may quote the author's own admission :

"In our state of transition from the former to the present pharmacopœia, the practitioner has occasionally fallen into the error of employing the nomenclature of both in the same prescription, indiscriminately mixing up that of 1824 with that of 1836. If he thinks proper to adhere to the term hydrargyri submurias, all the names associated with it ought to be contemporary ; in which case his language, to whichever standard it might refer, would at least, be consistent and intelligible. I have been informed by dispensing chemists that a departure from this rule has been attended with considerable embarrassment, and might lead to serious mistakes." (pp. 467-8.)

It however appears to us that, independently of the merits of the question, the author would have shown more wisdom in withholding controversial matter of this kind, from a work destined for the instruction of students and junior practitioners.

The "progress of physiological botany" is shortly noticed, and the chapter on the "influence of soil, culture, climate, and season," is comprehensive and full of valuable instruction. The introductory part also contains a few sensible remarks upon the "unseasonable collection of vegetable remedies, &c."

The Second Part of the work is devoted to the consideration of "*The Operation of Medicinal Substances, and the Classifications founded on them.*" Much has been written upon the classification of the materia medica, and many arrangements have been proposed, but hitherto no plan free from objections has been brought forward. For practical purposes, a physiological or therapeutical arrangement, or a judicious combination of these two, possesses the greatest advantages, as it places before the practitioner, in one view, all the available remedies employed in the cure of certain diseases, or to excite the action of certain functions. Dr. Paris mentions, with approbation, the classification of Dr. Cullen, but adopts that of the late Dr. Murray, whose admirable work on Materia Medica continued for so many years unrivalled by any contemporary publication on this subject. With additions, the author considers it well calculated to furnish a "frame-work" for the display of those therapeutical distinctions, the knowledge of which is essential for the successful administration of remedies and the full comprehension of his practical doctrines.

STIMULANTS. According to Dr. Paris, the term stimulant is very vague and indefinite, and hence is productive of very injurious opinions and practices, but as his views are peculiar, we shall give his own words :

"In its popular and generally accepted meaning, it [a stimulant] denotes any influence which accelerates the vital movements of the sanguineous and nervous systems, and is thereby supposed to exalt the energies of the body ; but action

is not power. In the first place let it be observed that, with the exception of what have been justly termed 'vital stimuli,' such as food, air, water, sleep, and heat, the whole range of the materia medica does not furnish a single agent which is capable of directly increasing the energies of the body, or of adding to the general stock of vital power........... Our misconception of the term has arisen from a partial and superficial view of the immediate effects of a few limited agents, of which brandy may be taken as their type; it produces a temporary excitement of the arterial and nervous systems, but this is invariably followed by a corresponding depression: all that we have done then is to disturb the balance; we have added nothing to the general amount of power." (pp. 169-70.)

It may be true that the materia medica furnishes few agents capable of directly and permanently increasing the vital powers; but the researches of modern physiologists and the general experience of mankind, fully prove that alcohol and alcoholic liquors are capable of assimilation, or are alimentary bodies, containing a considerable amount of carbon, and applicable either to the support of certain tissues, or for the supply of this element to the lungs and skin, during the processes of respiration and perspiration. How else can be explained the tendency to obesity which is induced in the individuals who indulge in their use? The following passages will further illustrate the author's views:

"There is scarcely a remedy, therefore, that might not properly fall under the designation of a stimulant; thus, congestion may take place in a nervous centre, and the whole system, in consequence of the oppression, exhibit symptoms of declining power; in such a case, venesection is a stimulant. The two most essential processes of animal life are nutrition and excretion, and these are exclusively performed by capillary vessels; suppose the balance of their circulation to be disturbed; mercury, by restoring and equalizing it, might, in certain cases, prove a true stimulant. The body may also exhibit symptoms of languor from intestinal and biliary accumulation; under such circumstances, a purgative assumes the character of a stimulant. The nervous system may be in a condition that repels sleep; a judicious and well-directed narcotic, inasmuch as it affords the means of indirectly giving power to the body, may be correctly considered as a stimulant." (p. 170.)

Although there is much truth in the foregoing remarks, the principles inculcated are by no means very definite, and they afford as much if not more scope for the construction of false and dangerous theories in practice as the ordinary definition he complains of, which, in our belief, is nearer the truth than his own. Dr. Paris considers it a great defect in the classification of Dr. Murray, that no distinct place is assigned to exhilarants, which is one of his additions. With this opinion we do not agree; for those substances possessed of the properties he enumerates are aromatics, the active constituents of which are essential oils, and they differ little in their action from the ordinary vegetable tonics, and when they operate as exhilarants, which is not frequently the case, it is generally by exciting an agreeable impression on the digestive organs.

NARCOTICS. This order of remedies is exceedingly diversified in action, and its members possess so different physiological and therapeutical properties, that no general principles can very safely be deduced from them. Authors in general, however, when describing their effects upon the various organs of the body, make some allusion to opium or its preparations, thereby showing that the simple narcotics are only referred to. Indeed, we consider it nearly impossible to give anything but a very

general description of their effects, such as is given by Dr. Murray. Dr. Paris seems to entertain opinions not more definite:

"In conclusion," he says, "it may be observed that there is probably no class of medicinal bodies, the individuals of which are less disposed to bend and conform to an artificial arrangement; each would seem to have its own particular mode of operation and to affect sensibility in its own peculiar manner, and hence the practitioner will often find that, after the failure of one narcotic, the administration of another will induce sleep or tranquillity." (p. 176.)

For practical purposes, it is very desirable that some author would arrange narcotics into subdivisions, according to their peculiar physiological or therapeutical actions in ordinary doses, and not from their poisonous effects.

Sedatives. There exists much room for dispute on the subject of this class of remedies, as there are no very precise facts upon which a proper theory may be founded. Some authors, however, assert very positively that some substances act primarily as sedatives; that is, never cause excitement of any kind, either in the first or last stages of their operation. An abstract opinion of this kind is of minor importance, but when a certain line of practice is founded upon it, an erroneous treatment may be the result.

"A sedative," according to Dr. Paris, "in whatever dose it may be given, is never followed by the slightest indication of excitement; it directly and primarily depresses the powers of life, whereas a narcotic in small doses never fails to increase the vital force. It therefore appears to me to be practically essential that we should recognize a class of agents so evidently distinct from all others, not for the sake of giving support to a particular theory, but to warn the practitioner against an error of practice, that of combining in the same prescription remedies obviously incompatible, from a belief in the similarity of their operation. Suppose, for instance, our object was to allay irritability by hydrocyanic acid, would it not be inconsistent to combine it with a small dose of opium, or a more considerable one of alcohol or of ammonia?" (pp. 176-7.)

If it were true that the therapeutical virtues of hydrocyanic acid depended on its depressing effects, the reasoning and practice of the author would be correct and conclusive; but as its mode of action in relieving irritability is totally unknown, and, on the most favorable view, purely hypothetical, his deductions cannot be admitted. It must not be imagined, however, that we advocate a combination of hydrocyanic acid with opium, alcohol, or ammonia: on the contrary, we generally exhibit it without the addition of any other medicine, but we have in one or two cases given it in combination with laudanum in cases of vomiting, and we see no reason why a little brandy might not be exhibited occasionally as an adjuvant. Its combination with ammonia does not neutralize its poisonous effects, according to several experimenters, and as stated by Dr. Christison; and why should it counteract its medicinal operation? Is laurel-water less efficacious than hydrocyanic acid because it contains an essential oil? Although, therefore, it may be unsafe to establish a class of medicinal agents upon hypothetical grounds, it may be useful to retain it on account of its practical utility; for though a slight excitement may accompany the primary action of a remedy, its secondary or depressing operation may be equally powerful, as if no stimulation was produced.

ANTISPASMODICS. According to Dr. Paris, antispasmodics are substances supposed to possess the power of allaying the inordinate action of muscular structures. The causes of spasm he considers to be the following: 1, Irritation of the nervous centres; 2, A loss of balance between the nervous and sanguineous system; 3, Irritation in the primæ viæ; 4, Cold; 5, Excessive muscular reaction, excited by over-extension; 6, A laborious effort to expel foreign matter. With the exception of the second, the causes assigned seem to be judicious. It is difficult to comprehend what is meant by a loss of balance between the nervous and sanguineous systems. It is no doubt true that a state of health necessarily implies the mutual cooperation of these two systems, but we have strong objections to such indefinite principles, particularly as no practical rules for ascertaining *the balance* are given, and the student is thus left in the wide field of hypothesis. After stating that narcotics are most important resources in allaying irritation and pain, the author enumerates certain agents which appear to exert a specific control over spasmodic action, independently of any influence upon its exciting causes, such as assafætida, musk, castor, ammonia, camphor, valerian, &c. He does not, however, establish this proposition very clearly, and some of the substances he enumerates have a powerful influence upon the exciting causes; such as ammonia in neutralizing acidity, and in promoting the heat of the body; indeed, all of them have a tendency to excite the calorific functions of the system, by quickening the circulation of the blood, &c. Why is camphor placed among this list, as almost all writers on materia medica consider it a narcotic?

TONICS. The chapter on this class of substances is sound and practical, and some excellent remarks are made upon the injudicious use of tonics in certain affections.

ASTRINGENTS. This article also contains much sound and useful information; but we pass on to the local or special stimulants.

EMETICS. These agents are well defined, and the various views of physiologists are ably discussed. The author here takes credit to himself, and justly, in our opinion, for anticipating the discovery of Magendie, that vascular congestion retards or suspends the operation of the absorbents; Dr. Paris having previously referred to the influence of venesection in accelerating the absorption of mercury.

PURGATIVES. These substances have three different modes of operation:

1. By exciting the peristaltic motion of the intestines.
2. By stimulating the excretory and exhalant vessels of the inner coat of the intestines.
3. By stimulating the neighbouring viscera, as the liver and pancreas.

The author makes a separate classification of Purgatives and Laxatives; the former being special stimulants, and the latter mechanical remedies. He brings forward bran as an example of the latter operation, and the coarse fibrous grasses eaten by the lower animals. A decoction of bran is, however, known to be laxative in its effects, and it does not appear to us that he has adduced sufficient evidence in support of his opinions that any of the purgatives act purely on mechanical principles.

He agrees in opinion with the greater number of writers on the subject, that different purgatives act upon the different parts of the intestinal

canal; as aloes upon the colon and rectum, colocynth upon the whole tube, mercury upon the functions of the liver, saline purgatives in increasing the watery secretions from the mucous surfaces; and, with some show of reason, entertains a liking for the old terms, hydragogues, cholagogues, &c.

"If there be a circumstance in the treatment of disease, which, above every other, is the government of a blind routine, it is the government of the bowels; let the complaint be what it may, the temperament, strength, or circumstances of the patient be ever so different, the first question of the practitioner relates to the bowels, and should they not have acted during the previous twenty-four hours, away he flies to the aid of aloes, colocynth, senna, calomel, &c. &c., to force the reluctant canal to pour forth its contents." (p. 210.)

We entirely agree with the author as to the morbid affection of the profession in this country for purgatives, but see no reason to question the judgment of the practitioner who in every case inquires carefully into the state of the bowels. The doctrines of Dr. Hamilton have often led young practitioners astray in the treatment of some diseases, particularly in typhus and scarlatina. Purgatives are highly beneficial during the early stages of these diseases; but after symptoms of debility begin to manifest themselves, as not unfrequently happens in weak or cachectic individuals, they ought to be used with extreme caution. A regular evacuation of the bowels, is, however, necessary, which may be effected by moderate doses of mild purgatives, or by enemata. An opposite practice, such as is sometimes adopted in the treatment of dothonenteritis is also very injurious to the patient, as the morbid secretions and alimentary matters become a source of irritation, either by sympathy or absorption. The same rule applies more or less to every other disease.

Emmenagogues. The article on remedies of this class is short, but clear and comprehensive. The author has no confidence in the specific action of certain substances on the uterus. Almost all the old specific emmenagogues have been justly consigned to oblivion, but ergot of rye has lately been extolled by some respectable authorities as redeeming the credit of the class. Its action on the uterus during labour is established beyond all dispute, and this action is capable of no other explanation, but that usually applied to diuretics, expectorants, &c., namely, that it exerts a stimulating influence upon the uterus independent of any particular general effect. No other substance is unequivocally known to possess this operation, and hence it is by no means improbable that even in the unimpregnated state, it may have some influence on the uterus, particularly during menorrhagia, when coagulated blood is liable to be formed in its cavity. In our experience, however, it is nearly as uncertain in its operation upon the menstrual secretion as other emmenagogues; but as it is productive of no injury to the constitution, it may, in many cases, be associated with the general treatment.

Diuretics. These remedies are classified by Dr. Paris as follows:

"1. Medicines which act primarily on the urinary organs.

"2. Medicines which act primarily on the absorbents, and secondarily on the kidneys.

"3. Medicines which act primarily on the stomach and primæ viæ, and secondarily on the absorbents." (pp. 214-5.)

The following substances are stated not to undergo any decomposition *in transitu*, namely, potassa, potassæ nitras, oleum terebinthinæ, juniperus communis, cantharides, and potassæ hydriodas; but squills, colchicum, copaiba, broom, &c. are decomposed. Are the experiments conclusive upon which this division is founded particularly regarding the vegetable substances? We strongly suspect that the question is still not correctly determined.

The medicines which act primarily on the absorbents are limited to mercury and iodine; and although it is rather an obscure subject, we are inclined to agree with the author in this view. The first of these agents undoubtedly has a powerful effect in promoting the absorption of several tissues, natural as well as morbid, and during a slight mercurial action, a diminution of weight is observable. The same occurs with iodine when exhibited in large doses, and some authors have accused this medicine of causing the absorption of the mammæ.

Medicines which act primarily on the stomach produce their effects—

1. By diminished arterial action, as in the case of tobacco, digitalis, &c.

2. By increasing the tone of the system. An instructive case is given in illustration, and its application in the acute diseases of old and cachectic individuals ought to be carefully weighed by the practitioner before he abstracts much blood from the system.

3. By producing catharsis. The following is the author's graphic description of the action of this class of remedies:

> "In the whole circle of medicinal operations, there is nothing more wonderful than this, that an impression made on the internal surface of the primæ viæ, by a few particles of matter, should thus convey by magic, as it were, an impulse to the most remote extremities, rousing their absorbents to action; and in cases of œdema, thus awaking the sleeping energies of the vessels, which, like millions of pumps at work, transmit the morbid fluid to the intestines and urinary passages, effecting a detumescence of the hydropic limbs in the course of a few hours, and thus affording a striking illustration of the sympathetic action of medicine, and are instructive examples of the operation of those of the sorbefacient class." (pp. 222-3.)

DIAPHORETICS. Dr. Paris justly condemns the use of stimulating diaphoretics during febrile and inflammatory diseases. Some authors are of opinion that several fevers, such as typhus, may be checked *in limine* by diaphoretics, like ordinary inflammatory diseases. This opinion appears to be totally without foundation; no physician experienced in the treatment of the latter disease has been able to verify this statement. This misconception arises from confounding other fevers or febrile disorders with typhus, which they intimately resemble in their early features.

EXPECTORANTS. The theory of the action of bodies of this class is somewhat obscure. It is highly probable, however, that some expectorants enter the circulation, and in this manner stimulate the bronchial tubes. Dr. Paris enumerates garlic, squills, the different balsams and fetid gums as belonging to this order. Some excellent remarks, worthy of serious application by the practitioner, are made upon atmospheric changes in relation to moisture and dryness.

Passing over SIALOGOGUES and COUNTER-IRRITANTS, which are treated with the author's wonted cleverness, we come to

Chemical remedies. An elaborate and well written account is given of the modern discoveries and opinions regarding digestion, the properties of the saliva, and the qualities of the blood, &c. Dr. Paris seems to hold the same opinions as Dr. Prout, that the generation of lactic acid is the source of many diseases, by imperfect assimilation. On this hypothesis he explains the advantages resulting from the protoxide of iron, as it has a great affinity for this acid; and may thus possibly act by clearing out such remains of this impurity as the emunctories have failed to eject. He lauds the mist. ferri comp. in that form of dyspepsia characterized by "a red tongue and pultaceous evacuations resembling pea-soup." (p. 273.)

Refrigerants. It must be confessed with the author that our notions respecting the action of this class of remedies are by no means well established. Many substances supposed to be refrigerant, from some inherent property of their own, undoubtedly owe this quality to the cold water in which they are exhibited. Acids, also, may owe a portion of their cooling properties to their peculiar effects on the gustatory organs, which effects are propagated by sympathy, to the whole system. Thus a small quantity of a very acid juice excites a shiver or slight tremor over the body, similar to the effects produced by sprinkling the forehead suddenly with very cold water. Dr. Paris demolishes the theory of Dr. Murray respecting the refrigerant operation of vegetable acids, fruit, &c., as is often his wont, in one elegant sentence. "Such," he says, "is the philosophical web which chemical ingenuity has wove for us; the device is beautiful, but the fabric will be found too frail to endure the touch." (p. 277.) The theory of Dr. Crawford respecting animal heat is as summarily confuted: "If the heat of the body depended upon respiration alone, any one might, by a voluntary effort of quick, deep and prolonged respiration, increase it at will," &c., (p. 277;) but, we suspect, some of our chemical friends could find grounds for still adhering to the theory of Crawford, even although our accomplished author had himself condescended to exhibit before them the inefficacy of the most potent respiratory efforts to warm up his corporeal system.

The theory of Liebig is next examined and his logical reasoning is opposed simply by the following assertion:

"But the theory of Liebig assigns the chemical change which converts the organic salts into a carbonate, to the absorption of oxygen in the lungs, whereas it is my belief that the vegetable acid, (e. g. the acetic,) is decomposed in the stomach by the ordinary powers of digestion, and that the alkaline base is there eliminated in the state of carbonate, or at least that it acquires carbonic acid during its transit." (p. 280.)

Antacids. The section on this class presents nothing particularly worthy of notice.

Antidotes. The article on this class is full and instructive, and though we are not prepared to approve of the author's classification of poisons, his remarks regarding the treatment, by emetics, decomposition, and consecutive measures, are exceedingly judicious. Dr. Paris seems still to entertain the opinion that the bichloride of mercury or corrosive sublimate is converted into the chloride or calomel by the agency of albumen; whereas it is now generally stated by chemical authorities that it merely forms an insoluble and inert compound with it, which we believe has been named by Lassaigne the chloro-hydrargyrate of albumen.

"ANTISEPTICS. The term putrefaction with reference to the question before us has been by far too loosely and indiscriminately applied. According to popular acceptation, we understand by it a succession of chemical changes, characterized by a more or less nauseous and offensive smell, by which an organic body, after passing through various stages of softening and attenuation, is ultimately exhaled, leaving only a small and fixed residue of earthy and saline matter. But let it be remembered that, between the incipient motions and those ulterior phenomena to which we have alluded, a number of intermediate changes occur, each of which must at once become final the instant the chemical and actual affinities are brought to a balance. It is therefore to the early links in the chain of decomposition that the attention of the pathologist is to be directed. . . .In those diseases, to which the epithet of *putrid* was formerly given, such as certain fevers, sea-scurvy, &c., the putrescent odour of the discharge, as well as the very rapid manner in which the body, after death, runs through the extreme stages of putrefaction, renders it probable that some of the preliminary decompositions may have commenced previous to the extinction of life. In all our discussions upon this question, it seems desirable that we should adopt some term to express the ascendency of chemical forces consequent upon the decadence or failure of vital power; the term *hyper-chemisis* might denote such a condition, reserving that of putrefaction to express the ulterior stages and consummation of the process." (pp. 308-9.)

The theory of Liebig respecting the operation of contagious matter in generating diseases is next alluded to with approbation, namely, that "a body, the atoms of which are in the act of transformation, (to which he has given the name of the exciter,) may impart its peculiar condition to compounds with which it may happen to communicate." (p. 310.)

The whole phenomena of the contagious fevers are agreeable to this beautiful law; the blood is universally altered; all the secretions are depraved; many of the organs are in a morbid state; and the great similarity in the symptoms, progress, and termination of the cases all tend to prove that the important series of changes which has been produced are the result of a small quantity of transforming matter infused into the system. The author next discusses the means to be employed in preventing the putrefactive process, namely, "by removing the influences necessary to its commencement. . . . . These may be said to be the presence of a certain portion of water, the access of air, a moderate degree of heat, and the contact of some matter, however small in quantity, in which putrefaction has already commenced, or, in the language of Liebig, the atoms of which are in the act of transformation." (p. 313.) Of all the disinfectants, pure air is considered the most efficient; some influence is, however, ascribed to chlorine.

ANTILITHICS. The article on this subject is full of modern information, and is worthy of careful perusal; but as many of the remarks are taken from Dr. Prout's valuable treatise, and as it is an extensive subject, our limits do not allow us to enter upon it. At page 329, paragraph 321, the following sentence occurs: "Nitric acid may be habitually discharged for years before that condition of kidney takes place which appears to dispose its concretion into a calculus." We presume that nitric acid is here a misprint for lithic acid.

ESCHAROTICS. In our opinion escharotics ought to have been a subdivision of epispastics; for the principles of action in rubefacients and blistering substances and escharotics, or those which are supposed to act chemically are nearly allied. This may be deduced from the fact, that the greater number of this order of medicines may be so regulated in

strength as to produce all those different effects. A dilute solution of potass will cause inflammation; one more concentrated, an elevation of the cuticle by a serous fluid; but the solid caustic produces complete decomposition of this and other textures. The application of heat may also be so regulated as to produce nearly the same results. Dr. Paris states that during the employment of caustic potass as an escharotic, ammonia is generated by the combination of the hydrogen and nitrogen, and that it may be detected by inverting over the surface a small jar moistened with hydrochloric acid, when the fumes of hydrochlorate of ammonia become visible.

The observations on Mechanical Remedies and Anthelmintics present nothing novel.

Diluents. The author makes the following observations on the misapplication of this order of remedies:

"Dr. Davy found by experiment that when an animal is bled to death the last portions of blood are of a much lower specific gravity than those which flow at the commencement, in consequence of the former containing more water, which it is to be inferred was derived by the increased activity of the absorbents from the mucous and serous membranes. Since, then, venesection promotes and accelerates absorption (108), it is clear that in inflammatory diseases, where we have recourse to bloodletting in order to diminish the volume of the circulating fluids, we ought not to suffer the patient to indulge in an unrestrained use of liquids, which he eagerly demands to satisfy a thirst which is probably the natural consequence of an increased absorption. In such cases it is often better to administer liquids in small divided doses, which will have the effect of moderating the thirst without overloading the arterial system and bringing back that tension and plenitude which it has been our object to relieve. These views are equally important to the surgeon, for they will guide him in the treatment of patients who have undergone operations, for in some cases it will be desirable to restore as speedily as possible the blood that may have been lost, while in others it may be equally advisable to adopt an opposite practice." (pp. 359-60.)

This line of practice is problematical; for, on the one hand, the appeasing of thirst diminishes febrile excitement and promotes diuresis; while it is highly probable that the very dilution of the blood may tend to resolve inflammatory action, even although the blood-vessels should be redistended to their former caliber, which, in practice, however, we have seen no reason to believe. The following opinion is also questionable:

"It cannot be denied that exorbitant potation has a tendency to produce fat, which may depend upon the vascular distention, and consequently diminished absorption thus occasioned. It has been explained on the supposition that water yields hydrogen by its decomposition; but it has been already stated (26) that we have no reason to suppose that it is ever resolved into its elements in the living system." (p. 360.)

It is contrary to the modern theory of assimilation, and in our opinion contrary to experience, to believe that water containing no alimentary substance can tend to the production of fat except indirectly in the promotion of digestion. The same may be said of condiments.

Alteratives. These are restricted by the author to the preparations of mercury, which "increase the activity of the secreting organs and imperceptibly restore healthy action," &c.

Part III. *The Theory and Art of Prescribing.* The author com-

mences this important part of his work by a comprehensive introduction, in which, after condemning the polypharmacy of the ancients, he animadverts upon the tendency to the opposite extreme, or an inactive simplicity. He alludes, with some point, to the combinations of nature, and the efficiency of some natural products when compared with the proximate principles produced from them. While discussing the objects to be attained by mixing and combining medicinal substances, the author gives the following elegant and powerful passage, to the discomfiture of empirical practitioners:

"Now, let me ask, what constitutes the essential difference between the true physician and his counterfeit, between the philosopher and the empiric? Simply this; that the latter exhibits the same medicine in every disease, however widely each may differ from the other in its symptoms and character, while the former examines, in a spirit of philosophic analysis, all the existing peculiarities of his patient and of his disorder, and being thus led by a sagacious induction to an estimate of his vital powers and to a knowledge of the nature and condition of the peccant organs, he graduates and adapts, with a sound discretion and with a correct judgment of his agents, such means as may be best calculated to control and correct their morbid condition." (p. 374.)

Five objects may be attained in the composition of formulæ; and these we shall notice in their order.

1. "*To promote the action of the basis or principal medicine.*" (p. 376.) The author considers it an important generalization that "a combination of similar remedies will produce a more certain, speedy,' and considerable effect than an equivalent dose of any single one." (p. 377.) That this law may hold in a number of cases we do not presume to question; but it has so many exceptions, in our opinion, that its claims to such a designation may with propriety be questioned. Exhilarants, excitants, or aromatic stimulants, are brought forward among others as examples:

"There are probably no remedies which receive greater mutual benefit by intermixture than the individuals which compose this class; for they not only thus acquire increased force and efficacy, but at the same time they lose much of their acrimony. If, for instance, any one spice, as the dried capsule of the capsicum, be taken into the stomach, it will excite a sense of heat and uneasiness; a similar effect will follow the ingestion of a quantity of black pepper; but if an equivalent quantity of these two stimulants be given in combination with each other, no such sense of pain is produced; but, on the contrary, a pleasant warmth is experienced and a general glow is felt over the whole body; and if a greater number of spices be joined together, the chance of pain and inflammation being produced will be still farther diminished." (p. 379.)

We question *in toto* the accuracy of the above statement; for we ourselves have often taken capsicum in conjunction with black pepper, along with alimentary matters, and never remarked that the hot qualities of either were in any degree abated. Indeed, the acridity of capsicum is with the greatest difficulty disguised by any other substance, whether stimulating or demulcent. Some of the illustrations alluded to are not fair examples of the combination of similar remedies, as in the confectio opii, confectio piperis nigri; for the additions to the primary ingredient are substances that possess different qualities. The composition of the different varieties of snuff is also brought to bear upon this question:

"The local action of stimulants would also appear to fall under the same law; and perhaps the origin of the popular custom, so long observed, of mixing to-

gether the varieties of snuff may thus receive a plausible explanation: certain it is that by such combination the harsh pungency of each will be diminished, and the odour rendered more mellow and agreeable." (p. 380.)

That the practice of mixing various kinds of snuff together is adopted and approved of by manufacturers of this article and connoisseurs in the nicotian art we doubt not; but that a mixture of good, bad, and indifferent kinds will form an agreeable variety is extremely doubtful. It may be generally predicated that two good varieties when mixed together will form an agreeable compound, the precise odour of which however can alone be determined by experiment; but if an inferior kind, or what amounts to the same thing, one not generally esteemed, be compounded with a favorite one, no man would be safe in prophesying a satisfactory result. A similar practice of mixing various qualities together is adopted by spirit-merchants and perfumers; but those which are approved of are the result of experiments nearly as precise as the formulæ of our pharmacopœias. In justice to the author, we must however admit that many of his examples furnish excellent proofs of the advantages resulting from the combinations of emetics, cathartics, diuretics, diaphoretics, &c. He mentions some exceptions, such as the combination of camphor and ammonia during fever, and the use of narcotics on particular occasions. He disapproves of the combination of squills, calomel, and digitalis, because the latter rather tends by its depressing powers upon the circulation to counteract the stimulating effects of the others. Farther and more accurate experiments are necessary to determine this knotty point; for many practitioners of experience are in the habit of exhibiting all the three medicines in combination with advantage.

The action of the basis is also promoted by combining it "with substances of a different nature and which do not exert any chemical influence upon it, but are found by experience to be capable of rendering the stomach or system or any particular organ more susceptible of its action." (p. 387.) In this division are noticed the superior diuretic powers of a combination of squills and calomel, the effects of antimony in quickening the operation of saline purgatives, of opium in increasing the sudorific powers of antimony, the operation of the bitartrate of potass in evacuating mucus, the increased power of the compound powder of jalap, the effects of colchicum in quickening the operation of aloes, &c. The beneficial effects of a combination of bitters with an alkali are also brought forward in illustration of the above law. May not this arise, to a great extent, from the neutralization of the acid which often exists in cases of dyspepsia?

"The relative sweetness of sugar when in different degrees of purity, depends upon the operation of the same law of combination; pure sugar, as Dr. M'Culloch has very truly observed, however paradoxical it may appear, is not so sweet as that which is impure. The sweetness of molasses compared with that of refined sugar is too well known to require more than to be mentioned; the vegetable matter, in this case, increases the effect of the saccharine principle with which it is combined." (pp. 391-2.)

We do not see the force of this reasoning. If molasses were the same kind of sugar as the refined, but disguised with vegetable impurities, the author's conclusion might be admitted; but as molasses is chiefly composed of uncrystallizable sugar its chemical qualities are different from

those of the other, and why should its natural sweetness be not also different? According to Dr. Paris venesection increases the effects of cathartic medicines, calomel, opiates, &c. Purgatives awaken the susceptibility of the body to mercurial impressions, and to the effects of "steel medicines" and to diuretics: change of diet and habits are also *adjuvantia.*

2. "*To connect the operation of the basis by obviating any unpleasant effects it might be likely to occasion, and which would prevent its intended action, and defeat the objects of its exhibition.*" (p. 399.) This article is well worthy of careful perusal, and contains many useful observations; but we must refer the reader to the work itself.

3. "*To obtain the joint operation of two or more medicines.* A. By combining those substances which are calculated to produce the same ultimate effects, although by totally different modes of operation." (p. 409.) This law is illustrated by a combination of the infusion of senna with neutral salts, digitalis, potass as a diuretic, opium and ipecacuan as a diaphoretic, &c. B. "By combining medicines which have entirely different powers, and which are required to obviate different symptoms, or to answer different indications." This section includes a numerous group, from which we shall select a few instances of combination.

Narcotics with mercurials. Five grains of the extract of conium with one of calomel is mentioned as beneficial in chronic rheumatism. The combination of calomel with opium is not here mentioned, a preparation the value of which in resolving inflammations, particularly those of an acute character in serous tissues, is not equalled by any other. The opium serves at least two important purposes; first, by preventing the action of the mercury on the bowels, and, second, by hastening its operation on the system. In what manner the latter operation is produced has not been satisfactorily explained.

Astringents with tonics. "I will take this opportunity to remark," says the author, "that in superseding the preparations of the bark by the salts of quina, we deprive ourselves of any power that may be derived from the astringent principle (tannic acid), and there is reason to believe that its presence in the native combination heightens the tonic virtues of the alkaloid." (p. 414.) Berzelius's opinion is at the same time quoted in proof of the estimated value of the tannic acid as a constituent of bark. We are disposed to concur with this opinion, and feel satisfied that the salts of quina are too often substituted for the bark itself, or for what we consider an excellent preparation of it, a decoction made with diluted sulphuric acid.

Astringents with narcotics. In diarrhea, three valuable indications are obtained by the union of a narcotic, an astringent, and an antacid; the first diminishes the peristaltic motion of the intestines, the second restrains the serous evacuation, and the third neutralizes acidity. The mineral tonics, such as the sulphates of zinc and copper, may also be combined advantageously with opium.

Purgatives with narcotics and antispasmodics. In colica pictonum, opium is often highly beneficial, as it allays the spasm which is the chief obstacle to the action of the purgative. Aloes and assafetida or sagapenum, are useful in flatulent colic. The combination of some aromatic

stimulant with drastic purgatives, is also of great service in preventing griping, particularly in that arising from aloes, colocynth, senna, gamboge, &c.

Diuretics with excitants. The author recommends a combination of squills, ammonia and ether, in cases where the system requires to be excited and upheld; and several formulæ are referred to. In such cases or in those where dropsical accumulations occur in weak or cachectic individuals, we have found a portion of gin or whisky a valuable adjuvant.

Expectorants with excitants. In peripneumonia notha, when the powers of life are ebbing and the lungs oppressed with viscid mucus, ammonia is recommended, to which small doses of opium may be added, if it does not check expectoration. In conclusion, the author justly remarks:

"In the construction of these composite formulæ, considerable experience and judgment are required. In the foregoing section I have endeavoured to point out, generally, the nature of the advantages to be obtained by such combinations, but they are not to be adopted without the nicest discriminations. No written instructions can ever embrace the whole extent of the subject; its leading points, however, may be seized by the student, and made the topics of his own reflections and examinations." (p. 420.)

4. "*To obtain a new and active remedy, not afforded by any single substance.*" (p. 422.) This is a dangerous field of practice and ought not to be adopted, in extemporaneous prescription, without careful examination and experiment. The decomposition of the sulphate of iron with carbonate of soda, resulting in the formation of the proto-carbonate of iron, an entirely new compound, is an exemplification of this law. Formula 90 (p. 604) is also adduced by the author, and it is the following; R. sodæ sesquicarb. ʒij, ferri sulph. gr. iij, magnesiæ carbonat. ʒj, aquæ Oss, acid. sulph. dilut. fʒx. In this case, sulphates of soda and magnesia are formed, holding in solution proto-carbonate of iron with excess of carbonic acid gas. In the development of active principles, the author alludes to a plaster, which is stated to be very efficacious in curing the swelling of the bursæ of the patella, a disease common to housemaids. It is composed of hydrochlorate of ammonia, soap and lead plaster; and the alkali of the soap slowly combines with the acid and liberates the ammonia. (Form. 17, p. 591.) The black and yellow washes are also mentioned as examples.

The increase or diminution of the solubility of substances also influences their operation. Dr. Paris endeavours to account for the varieties of action on the intestinal canal of gamboge, aloes, and colocynth, &c. upon their various degrees of solubility. Although there may be a portion of truth in his remarks, we are inclined to think that further and more accurate experiments are still required to determine these points. Mixtures of different saline purgatives are stated to be more active than an equivalent dose of any single one; as for example, the sulphate of magnesia and tartrate of potass and soda. The author attributes the griping effects of some purgatives to their comparative insolubility. This, a priori, would seem to be the case with resinous purgatives; but the same result is produced by an infusion of senna, buckthorn juice, &c. which proves that it also depends upon the irritating qualities of the agent.

The state of insolubility has a powerful effect on poisonous agents; thus many substances, such as the acetate of lead, are rendered inoperative by being thrown down in an insoluble form.

5. "*To afford an eligible form.*" (p. 441.) "It should ever be our object," says the author, "to accommodate, as far as we are able, the form and flavour of our medicines to the taste and caprice of the patient, whose prejudices should never fall coldly upon the ear of the physician; for such is their influence upon the body, that by a little address they may often be enlisted into our service, while by opposing them they are rendered formidable obstacles to our plan of treatment." (p. 442.) The disagreeable flavour of opium is disguised by storax and saffron. In solution, we have found a little acid and sweet spirits of nitre very serviceable for this purpose. The taste of alkaline solution is rendered less disagreeable by combination with beer or a bitter infusion. (Form. 169, p. 618.) Acids are sheathed by mucilage, and (it might have been added) sugar. Castor oil and balsam of copaiba are less nauseous, when formed into an emulsion by albumen ovi or mucilage. The addition of a little ammonia or aqua potassæ and syrup is very useful in preserving the emulsive form. Some medicines may be exhibited in the effervescent state with bicarbonate of soda and lemon-juice; but care must be taken that no decomposition ensue. Some substances require the addition of some agent to prevent the spontaneous changes to which they are liable. Thus sugar, which is found so useful in the preservation of animal and vegetable substances, has also been found essential in preventing the chemical changes which some mineral agents undergo. Iodide of iron and the proto-carbonate of iron are two remarkable examples in point. It ought to be observed, however, that dilute solutions of sugar are very liable to fermentation, and for this reason syrups should not be introduced into mixtures, which are not to be speedily used, particularly during warm weather. A small portion of an aromatic tincture, when added to mixtures, is very efficacious in retarding this process. Indeed the addition of a portion of alcohol to infusions and decoctions of vegetable substances or salts, containing a vegetable acid, is in many instances absolutely necessary to prevent decomposition.

Prescriptions. The subject of prescription is a very important one, and involves several grave considerations. Simplicity of construction, with a small or moderate amount of ingredients, should ever be regarded as the greatest desideratum; as there is more safety and efficiency in this practice than when there is a multitude of *bases*, *adjuvants*, and *corrigents*, which, like camp-followers, become a dead weight on the energies of the others. There is a tendency, however, among men of small comprehension, to place reliance upon little conceits of their own, which they generally dress up according to their own notions of taste and efficiency, and in this state are palmed upon their patients and sometimes on the profession as infallible prescriptions for the cure of certain forms of diseases. Such receipts ought to be narrowly scrutinised, and if made the subject of experiments, the trials should be made with caution and accuracy, so that their virtues may be corroborated, or their inefficiency exposed.

"'Superflua nusquam non nocent.' Let him cherish," says the learned author, "this maxim in his remembrance, and in forming compounds, always

discard from them every element which has not its mode of action clearly defined, unless indeed, as we shall hereafter explain, a general and paramount experience shall have stamped upon it the authentic seal of experience . . . . . A medicinal formula has been divided into four constituent parts, a plan which will be found to admit of useful application to practice . . . . .

"1. The Basis, or principal ingredient.

"2. The Adjuvans; that which assists and promotes its operation.

"3. The Corrigens; that which corrects its operation.

"4. The Constituents; that which imparts an agreeable form.

"These several elements, however, are not all necessarily present in every formula, since many medicines do not require any addition to promote their operation; and the mild and tractable nature of others renders the introduction of any corrective unnecessary, while some again are in their nature so manageable as not to require the interposition of any vehicle or constituent." (pp. 449-50.)

DOSES OF MEDICINES. "It would appear," says Dr. Paris, "that powerful doses are disposed to produce local rather than general effects. Experience seems to prove that, in this respect, the effect of an internal remedy is analogous to that upon external impression; if violent, it affects more particularly the part to which it is directly applied, as pinching does that of the skin; whereas titillation, which may be said to differ from the former only in degree, acts upon the whole system, and if long continued would even occasion convulsions." (p. 453.)

We are glad to be supported by the authority of our author in the advocacy of moderate doses of medicines, as we are convinced that a prevailing evil in the practice of this country is *over-dosing.* In the case of chronic diseases this evil is carried to a truly frightful amount; and we do not hesitate to say that in a pretty large proportion of cases, the imaginary agency of the medicines in the hands of the homœopathists, leaving nature undisturbed, is less injurious and more successful than the energetic empiricism so often practised. The intercourse in recent times with the continental nations has tended to mitigate considerably this evil; but it yet rages, to a melancholy extent, among the disciples of the pure London school. We must not, however, be misunderstood, as proscribing large doses altogether. In their proper place they are absolutely necessary. The doses of medicines ought to be regulated by age, sex, temperament, habit, diet, profession, climate, and season, nature and duration of disease, time of the day, idiosyncrasy, &c.; but we refer to the work itself for full instructions on these matters.

The next article designated *chemical pharmaceutical errors,* exemplifies the various errors which are or may be committed by those who have an imperfect knowledge of chemistry, and is well worthy of perusal by those who wish to attain accuracy or elegance in writing prescriptions. The blunders committed in this department, even by otherwise highly respectable practitioners, are more frequent than they would have been if chemistry had held its due place in their education; for we hesitate not to say that this branch of medical education has been too lightly appreciated by the great body of practitioners. A most valuable table of *incompatible substances* is appended to this article. It is more copious than any hitherto published, occupying about forty pages of the work, includes many of the medicines found in our British pharmacopœias, and being accompanied with symbols is compressed into small bulk.

ON THE PARTICULAR FORMS OF REMEDIES. *Powders.* The following rule of the author applies generally to this form of medicine:

"I think it may be received as a general rule that extreme pulverization assists the operation of all substances whose active principles are not readily soluble; and that of compound powders whose ingredients require intimate intermixtures, whilst it certainly impairs the virtues of such as contain a volatile principle which is easily dissipated, or extractive matter which is readily oxidized." (p. 545.)

In the prescription of some saline bodies, it ought to be remembered that their mixture with other substances sometimes gives rise to a soft or liquid compound from the water of crystallization being differently arranged. Some are also apt to attract moisture from the atmosphere, and are thus apt to communicate the same tendency to those with which they are mixed. Pulverized squill root is peculiarly liable to this result, and we have seen specimens assume the consistency of diachylon plaster. All such powders ought to be kept in small bottles very tightly corked.

Pills. The preparation of pills is of far more importance than is generally imagined; for medicines are often prescribed in this form, as being more convenient and agreeable to the patient. They ought to possess, among other qualities, a proper consistency, and should not become mouldy or hard, when they require to be kept for some time. Conserve of roses is the excipient in many of the formulæ for pills in the pharmacopœias, and is generally well adapted for preserving them in a soft state, as it has an affinity for moisture. We have also found the addition of a little fixed oil very useful for this purpose, and when soap is not objectionable in a pill containing gum resins, it has a similar effect; as in the compound colocynth or gamboge pill. When pills become very hard, they ought not to be employed without being previously softened, as they are liable to pass through the bowels undissolved.* A convenient method of doing this is to moisten them with spirits or water the day previous to their use.

Inhalations. Remedies in this form are not in general much valued by the profession, and the tar-vapour of Sir A. Crichton as well as the chlorines and iodine of more recent date, are now rarely heard of. By these remarks we do not mean to depreciate the usefulness of these and other substances, in certain bronchial affections; but rather to show that the exaggerated expectations with which their introduction was accompanied, have not been realised. The temperature and hygrometric qualities of the atmosphere are certainly of great importance in many pectoral and other diseases; and were it possible to obtain the other essentials in promoting health as accompaniments of an artificial atmosphere, an important object would be thus obtained. This, we fear, can only be had, with all the requisite advantages, in a climate of nature's own formation. We can, however, by such means impregnate the system thoroughly with mer-

* We had once, now many years since, a ludicrous instance of this in our own practice, in consultation with an apothecary of the old school, whose pathology was also not very modern. Before seeing the patient we were told that it was a case of *gall-stones*, and on inquiry were further informed that though a *vast number* of these bodies had been passed, the patient had never suffered pain. This naturally surprised us, and our surprise was not lessened when the patient produced from the cupboard several pill-boxes, full of the said gall-stones! On even a slight inspection these were discovered to be pills, ("Boerhaave's red pill,") which the old gentleman, a toothless ecclesiastic, had been long in the habit of taking for a cutaneous affection!

cury and other agents, but the effects are often so uncertain as to render it by no means an eligible method.

Blisters. Dr. Paris does not notice the blistering *tissues* and *chartæ* of various chemists, which we think he might have done as an improvement of modern times, more particularly as it has been asserted that they produce no strangury. He is of opinion that blisters should not be kept applied, until vesication has occurred, as this effect generally follows the dressing. That this is a frequent result we do not question, but in the acute and dangerous internal diseases of adults, we are led by experience to believe that the blister is the most efficacious that is retained *in situ* until thorough vesication is produced. The opinions expressed in the following paragraph, are also very doubtful if not decidedly erroneous:

> "The popular practice of copious potations to protect the kidneys from any anticipated irritation is wholly erroneous; and actually counteracts any benefit which might be derived from the action of a blister as an evacuant. (p. 357.) Erroneous views have also been entertained with regard to the effects of a blister bearing an exact proportion to its size. Large blisters, while they are far more efficient as evacuants, do not produce greater pain and constitutional irritation than those of smaller size." (p. 583.)

We are not in the habit of recommending copious potations, with the object of preventing strangury, for we are doubtful of their beneficial agency; but certainly we do not believe that this practice is productive of any very injurious effects, in inflammatory diseases, as we have endeavoured to show in a former part of this article. Although we approve of large blisters in certain affections, yet in our experience—and theory supports us—they always occasion more pain and irritation than small blisters; and we have often seen the system highly excited for a day or two by the former, whereas the latter more rarely produce much inconvenience.

The illustrative formulæ appended to the work are numerous and select, and being arranged under the various orders of medicines, with key-letters, &c. will be found of great use to the junior practitioner.

As we have now given a pretty copious analysis of Dr. Paris's work, we must here take leave of it and its learned and accomplished author. On several important subjects we have had occasion to differ in opinion from Dr. Paris, but from the prominent station of the writer, and the high character of the work, we considered it the more our duty to point out some of his more questionable dogmas and doctrines. The department of "Special Pharmacology" has been altogether omitted in the present edition; and there is sufficient internal evidence that the pharmacologia has been rewritten, and remodelled upon the theories and discoveries of modern times. Many of the chapters still contain a considerable amount of irrelevant, or at least very debateable matter. These portions of the treatise might, with great advantage to the reader, have been condensed, and the rules and principles confined to matters which were thoroughly established. The work, however, as a whole, is one of great erudition and great ability; it contains a large amount of information; it is elegantly and perspicuously written; and it will form an excellent guide to the practitioner, in compounding and prescribing the articles of the materia medica. We therefore recommend it in strong terms to the attention of all our readers.

Art. XIII.

Report of the Committee appointed by the Right Hon. the Governor of Bengal for the establishment of a Fever Hospital, and for inquiring into Local Management and Taxation in Calcutta.—Calcutta, 1839. Folio, pp. 245.

The recent inquiries into the sanatory condition of our own cities, of which due notice has been taken in this Journal, have naturally led us to look at what has been done elsewhere; and in the course of our investigation, we have been much gratified in observing the great attention which has been recently bestowed on the sanatory improvement of the metropolis of British India. The document now before us contains the suggestions of a committee of eminent and highly competent individuals, relative to the measures which they deem conducive to the removal of the causes of endemic disease, and the efficient relief of the poorer classes suffering from its effects.

The length at which we have entered on the kindred subject of Mr. Chadwick's reports, forbids our bestowing more than a cursory notice on that of the Calcutta committee. To the energy and benevolence of Mr. Martin, the late surgeon of the Calcutta Native Hospital, is due the appointment of the committee. The attention and zealous industry evinced by that body in their investigations is beyond all praise, and although few if any of their suggestions have been acted upon by the government, nevertheless their inquiries have at least done this good, that individual benevolence has been excited to acts of munificent charity, and some serious local nuisances have been considerably abated.

Scattered throughout the report, we find numerous proofs of the correct and enlightened views with which Mr. Martin directed the labours of the committee. We are induced to select the following passages in justification of the favorable opinion we have formed of the exertions of this active officer, in forcing on the notice of the government and the public, the measures he deemed essential to the preservation of the general health. On the result of local improvement as affecting the intensity of endemic remittent fever in Bengal, Mr. Martin has the following observations:

"The causes of the present superiority in public health must be of the highest interest and importance, especially to communities living within the tropics; and with all the just confidence in modern medicine, guided by the lights of an improved physiology and those of pathology, I cannot yet agree with those who would ascribe the *whole* of the difference here spoken of to superior modes of medical management, great as these confessedly are. It is not through the advantages of modern improvement in the treatment of mere disease, as contrasted with the more ancient modes, that the public health has been so much amended, as through the great measures of the *prevention of disease* consequent on the progress of the public mind, and of governments, *general knowledge*, leading directly to improved habits of life in communities, improved localities, institutions of police, &c.; it is to the preservative power of knowledge, to the reciprocal actions of the social state, and of political events upon each other, and upon medical science, that the advancement of public health is most indebted and that it will continue to be so, although the circumstances are not sufficiently weighed by some of us, when in our hurry to praise ourselves, we forget what is due to our predecessors, and that these last had frequently to treat a form of

disease which we have never seen, and with whose fatal severity we are, consequently, unacquainted."

Again, when speaking of epidemics, and of the influence of local improvements on their spread and intensity, Mr. Martin observes:

"A history of our local epidemics would be of great value, as enabling us to trace their connexion with changes of climate and condition of the surrounding localities, or with the social condition of the people, both European and native; had such a history existed, it would have helped to an earlier establishment of general principles, and a rational plan of treating our fevers especially; for, though all the epidemics within my personal recollection (and there is scarcely a year we have not one in some form) differ greatly from the ordinary endemics of the country, still, there will be found in most of them, so much of the savour of the soil, if I may be allowed the expression, as to render a knowledge of their history and treatment an object of no mean importance. It is these sweeping epidemics, aggravated by endemial and social influences, that swell out our bills of mortality, and that did so in former times especially, to such a frightful extent.

"Under a system of local improvement calculated to diminish the endemic sources of disease, I am satisfied that the value of European life in this city (Calcutta) would be greatly enhanced.

"From what is here said, however, it must not be supposed that we are without help in the case of epidemics, or that our means of prevention apply only to such diseases as are peculiar to the climate; far otherwise is the fact. Even epidemics are greatly modified by states of locality; and, as stated in another place, *they are found to fasten with peculiar severity, and remain longest, in such localities as are neglected;* in truth endemics are very often the parent stock upon which epidemics are engrafted. Why is it we have not now, as formerly, those terrible epidemic fevers which swept off '800 Europeans, and 50,000 blacks?' The cause is obvious enough to the most ordinary understanding; it is the same that has banished the former malignant intermittents of our city, to which we owe our present comparative exemption; in short, when filth, want, and misery are removed, epidemics will have lost their chief power. It is these that everywhere give such destructive influence to climate, and that gave rise to the observation of Cabanis, 'que l'effet du climat n'est pas le même pour le riche que pour le pauvre.' The impure state of London during the seventy-three years from 1592 to 1666 was such, that the average deaths in each year, from the plagues, amounted to about a fourth part of the whole population; or what would, for the present population, amount to the fearful sum of 375,000 persons per annum! Owing to the modern improvements of London again, only one person died out of 250 inhabitants, or little more than 6000 in the worst year of cholera, the severest plague which has visited London since 1666."

It is unnecessary to prolong this notice, as the details embodied in the report are so strictly local in their nature, that little general benefit could flow from their consideration. We have pleasure in reiterating our sense of the great value of Mr. Martin's labours. We are bound, too, to express our hope that he may find many to follow his example in toiling to improve the public health in the great and populous cities of British India, of which so many are still abandoned to the destructive influence of the jungle and morass, and are at the same time destitute of all efficient means of relief for the sufferers from such unpardonable neglect. Mr. Martin himself has, however, abandoned his old field of exertion, and transferred his services from the metropolis of India to that of England, where, we doubt not, he will find his talents appreciated as they deserve, both by the profession and the government.

ART. XIV.

Ueber die Verjüngung des Menschlichen Lebens und die Mittel und Wege zu ihrer Kultur. Nach physiologischen Untersuchungen in praktischer Anwendung dargestelt von DR. CARL HEINRICH SCHULTZ, &c.—*Berlin*, 1842. 8vo, pp. 445.

On the Rejuvenescence of Man, or Renewal of Human Life, and on the Means and Modes of cultivating it; a practical Treatise, founded on Physiological Researches. By Dr. C. H. SCHULTZ, Professor of Medicine in ordinary at the University of Berlin.—*Berlin*, 1842.

THE idea of a constant renewal of human life, and of the possibility of maintaining perpetual youth, is as old as science itself. It has given origin alike to the study of personal hygiène and to numerous treatises on health and long life. It has occupied the minds of Aristotle and Bacon, of Paracelsus, Sanctorius, Hufeland, Kant, and others. Of the writers on the subject in our own country, Sir John Sinclair has perhaps obtained the highest celebrity.

Both the theory and practice of these authors have been faulty, even of the most celebrated and practical, as Bacon, Hufeland, and Sinclair. People must be treated according to their real and not according to ideal circumstances. Few men have the leisure, like Sanctorius, to weigh themselves for thirty years, or like Cornaro, the opportunity of taking their food and drink by weight and measure. Nor is it possible that the dietetic and hygiènic rules, so precisely and positively laid down by authors, can be acted on. Living in the world we must act with the world; and this same world is no rich valetudinarian ever fearing to transgress.

One of the fundamental principles of physiological science is that the materials of the bodies of animals are continually undergoing renewal; new matter being deposited and the old removed. The process by which the two acts are accomplished is termed by Professor Schultz the *verjüngungs* process, a word which may be translated as the *youthifying* process or the process of becoming young. If this process is completely interrupted, death takes place, and the more effectually it is performed, the stronger is the individual, and the more likely to have life prolonged. Hence the importance of a correct knowledge of its nature and its relations, and of the modes and means by which it may be maintained; or if interrupted, may be restored. This is the object of Professor Schultz's inquiries.

In the present article we purpose giving a pretty full account of the author's views, restricting ourselves, however, almost entirely to an analytical summary of them. If we did not think some of the doctrines very important, and yet more of them ingenious, we would not trouble ourselves or our readers with any detailed exposition of them; but it would occupy much more of our space than we can at present afford to discuss them critically, or to point out the numerous instances where we demur and where we differ. On another occasion we may resume the subject; at present we hope we may claim not merely the attention of our readers to our analysis, but their thanks for the trouble we have taken to gratify them. It is only the *labor ipse voluptas* of a British and Foreign reviewer, that could minister to them in this kind.

The general process comprehended in Professor Schultz's inquiries, is subdivided into two others: *a*, the process of reorganization; *b*, the process of disorganization, and separation of the effete molecules from the body, or *moulting* (Mauser), as our author prefers to express himself. These two are the poles between which the pendulum of vital action swings, the poles of life and death, of renewal and destruction, of revival and effeteness. And as the whole term of life is periodic, so is the alternation of these processes. In short, in the renewing process *we have to trace a current of vital changes through the channel of periodicity.* Illustrations of the processes are drawn from the moulting and other phenomena of insect life, and from the development of plants. The moultings of insects are not confined to the epidermis, but extend to all the mucous membranes; the matter thrown off at each change being the *debris* or effete matter remaining after the renewal of the tissues. These changes in moultings are traced to the point at which the generative organs become fully developed, and ova are deposited; when the *verjüngungs* or renewing process is exhibited in the production of a new animal, and the parent is itself the debris or educt of the final moult. (!) A perennial plant so called is really not a perennial at all, but merely a whole generation of twigs and layers of wood and bark springing annually from the old stem. The inner structure of the tree never changes. No plant can be more than biennial, as this is the utmost period required by any for the development of the blossom and the ripening of the fruit. What remains after the latter process is accomplished is merely the residue of a life or lives ended. An old tree is like a coral reef, a monument of death formed out of the debris of the successive generations which have annually sprung from it. And a strict analogy may be traced between the blooming of plants and their death after fructification, and the development of the generative organs of insects during their successive metamorphoses and their death after the deposition of their ova. In both the development of the individual arrives at the same results and exhibits the same effects. In animals of a higher grade the complete development of the generative organs, and the renewal of the species do not occur contemporaneously with a total cessation of the renewing process in the parent; but the general law is manifest in the moultings of the cutaneous appendages to which they are liable, and which occur contemporaneously with changes in the ovaria and testes. The substances moulted are but the debris left by the renewing process, and the changes in both the skin and generative organs are only indicative of the moult and the renewal occurring throughout every part of the system of the individual. The mucous membrane of the lungs and alimentary canal, and of the genito-urinary apparatus, moults as well as the skin. The teeth in many fishes and reptiles are a product of and fixed into the mucous membrane, and are thrown off when the general renewing process takes place, just as are the cutaneous appendages. Indeed the teeth generally have an important relation to the periodic renewals and changes in the individual.

Such are our author's views as regards the renewing process in general; and now we come to more special changes, namely, to the death and renewal of the microscopic structures, and especially of microscopic constituents of the blood. Our author's inquiries as respects the latter are

very curious, very novel, and, if they prove to be correct, will be of the highest value in practical medicine.

Professor Schultz observes that the attention of physiologists has been concentrated rather upon the chemical and mechanical composition of the blood than on its organic properties. A knowledge of its vital phenomena cannot, however, be attained either by dynamics or chemistry, but only by an organic analysis. This analysis Professor Schultz has already undertaken and given the results to the public.* In the present work these results are farther developed and carried out to practice.

"We have shown that the organic constituent of the blood, namely, the blood-vesicles, the so-called blood-corpuscles, are by no means unchangeable and fixed constituents as they have hitherto been considered; but are being continually re-formed and decomposed; that both in the embryo and during digestion they continually originate anew and displace the old; and that when arrived at the climax of development they begin to dissolve and die, their debris being invariably separated from the body as the new vesicles form. In the blood of an animal, or man, blood-vesicles may be found in all stages of development between their first origin and dissolution. I believe I am the first who has given a history of the development of the blood, and shown in what the vital activity of the blood exists. These researches have rendered us better acquainted both with the true organization of the so-called blood-corpuscles as well as of the plastic constituent of the blood. We can also better understand the part taken by these constituents as well in the formation of the blood itself as in the formation of the body. We can distinguish between their vital manifestations and their chemical properties. We can penetrate into the connexion between their vital activity and other functions; and, lastly, we can study the pathological changes which are dependent on, or co-existent with, abnormal vital action in these organized constituents." (p. 37.)

Composition of the blood. According to our author's researches there are only two organized constituents in the blood; namely, the plasma and the vesicles. Neither fibrine nor serum exists as such in living blood. The latter is formed as a chemical product during coagulation; the former is a product of the vital power during the death of the blood. The plasma corresponds to the liquor sanguinis; it may be separated from the blood by two or three methods. The best is the following: let the blood flow directly into a tall glass cylinder, and when full close it quickly so as perfectly to exclude the air. By this means the plasma is kept longer fluid, and time is given for the vesicles to sink to the bottom of the cylinder, leaving the plasma supernatant. Or the blood may be collected, by means of a funnel, into a piece of intestine previously well cleaned, and closed at one end. If the piece of intestine be made perfectly full and the open end closed tight with strong twine and then hung up, the vesicles will separate from the plasma, sinking to the most dependent part, and the intestine can be bound with thread at the point of separation between the two.

Composition and properties of the blood-vesicles. The vesicles of vertebrate animals in their perfect condition consist of an outer mem-

* 1. System der Circulation in seiner Entwickelung durch die Thierreiche und im Menschen.—Stuttgard, 1836. With Plates.

2. Der Lebensprocess der Pfortader systeme. Hufeland's Journal der pract. Heilk. Feb. 1837.

3. Ueber die gehemmte und die gesteigerte Auflösung der verbrauchten Blutbläschen. —Ibid. March, 1838.

brane, containing an elastic fluid in which a granule floats. This membranous cell is at first colourless, but afterwards becomes coloured by the colouring matter entering into its tissue and rendering it opaque. If fresh vesicles be placed in brine or a weak saline solution and water be added gradually, the colouring matter will be dissolved out of the vesicles and they then become thin, transparent, and almost invisible. But their texture is not altered, for Professor Schultz has found that by adding tincture of iodine to the solution the vesicles become dark-brown. (p. 40.)

In making experiments on these vesicles various interesting phenomena may be observed. 1st. The same quantity of water has a different effect upon different vesicles. Some become quite colourless, others only imperfect, others are unchanged. This difference depends upon the quantity of colour contained originally in the membrane of the vesicle; those having a larger quantity requiring a larger quantity of water to abstract it all. The quantity of colouring matter in the vesicles varies in different classes of animals. In the vesicles of the blood of fishes it is the least; in birds and mammalia the greatest. In the vesicles of all embryos there is less than in adult animals. In all the quantity is in relation to the stage of development of the vesicle. 2d. The perfectly developed vesicles of the vertebrata and of man are flat so long as they contain their colouring matter. When this is removed by water they then assume a globular form, and the inner part of the vesicle and its contained granule become more distinct. 3. The vesicles are remarkably elastic, particularly in their colourless state. 4. They possess an organic contractility during life, easily demonstrable by certain stimulants, as cold, neutral salts, and alcohol. This contractility is very obvious when vesicles deprived of colouring matter are brought into contact with salt; they then become flat again, and are also contracted into a variety of forms. In the perfect vesicles it is less obvious. When the vesicles die they are no longer contractile, and become quite flaccid. In this state they lose the capacity of dilatation, so that it cannot be by the dynamic property of endosmose that the living vesicle is filled. This, according to our author, is the basis of the vital power of the blood. The excitability imparted during respiration is founded upon it. It is extraordinarily great in the young and newly-formed vesicles found in the embryo and the lymphatics. The more active the contraction of the membrane the less readily is the colouring matter withdrawn from it, and this explains why it has been generally supposed that the colouring matter is not soluble in saline solutions of a certain strength, the salt acting as a stimulus to contractility, and consequently rendering the separation of the colouring matter less easy. In living blood the vesicles are in a state of natural contraction from the oxygen contained in them, and from the salts held in solution in the plasma.

With reference to the statement that the vesicles contain oxygen gas, Professor Schultz observes that there has hitherto been considerable uncertainty as to the presence of gases in the blood. He demonstrates and collects it thus. He takes as large a bottle as he can get, provides it with an air-tight stopper, and fills it full of blood fresh drawn from a horse until it runs over. The stopper is then thrust in, and the blood set aside to cool. As this process goes on a vacuum is formed at the upper

part of the bottle, and little globules of gas are seen collecting from all parts of the mass into the vacuum. The chemical analysis of this gas shows that it is oxygen gas if arterial blood has been used for the experiment; carbonic-acid gas if venous blood. Professor Schultz infers from these researches that the oxygen gas in the lungs is absorbed into their interior by the vesicles. The capacity to absorb the gas depends upon the degree of excitability in the vesicles; those with large granules have the greatest separating power. In general the smaller the granule and the more the membrane is coloured the less the contractility, and *vice versâ;* large granules with almost colourless membranes have the greatest contractility. These are found in the greatest number in the rose-coloured lymph in the thoracic duct.

Origin, development, and death of the blood-vesicles. The first origin of the vesicles is seen in the lymphatics. The lymph contains naked granules of different sizes, the so-called lymph-globules. The largest of these are perfectly soluble in ether; it is obvious also, from their smooth shining appearance, that they are fat-globules. They also reappear when the ether is evaporated. The larger lymph-globules become metamorphosed into the smaller, and round these a filamentous vesicle is seen to be developed, which is at first perfectly globular, colourless, and transparent. In others in a more advanced stage it is seen that the membrane begins to be coloured, and in these contractility is developed. The lymph-globule is closely shut up within the membrane, so that the blood vesicles are in reality formed in the lymph, and their granules are fully-developed lymph-globules. When the vesicles thus formed pass into the current of the circulation they are quickly subject to further changes. The more frequently they pass through the lungs and are subject to the influence of oxygen gas, the more the size of the granules diminishes, until at last they disappear altogether from the vesicle. And in proportion as this change takes place the vesicle becomes more deeply coloured and less contractile until they are quite dark and without contractility, becoming *pari passu* specifically heavier than the plasma. By means of the latter property the vesicles of different sizes may be separated from each other, as when blood is taken into a glass cylinder, the oldest being the most deeply coloured and the heaviest sink to the bottom, and the youngest and least coloured are at the top.

When the blood-vesicle has become incapable of being further acted on by oxygen gas, it is useless. The colouring matter must be removed, and the residuum, or film be excreted or reorganized. According to Professor Schultz it is in the liver these changes take place. The absence of valves in the vena portæ, and the slow motion of the blood, are favorable to the precipitation of the old heavy useless vesicles from the general current of the circulation, and the more fluid plasma readily extracts the colouring matter of the flaccid films. On a chemical analysis of the portal blood, the plasma is found to be less in quantity and more fluid, and containing more colouring matter than that of venous blood. This must necessarily be the fact, because the vesicles part with their colouring matter the more readily as they become less contractile. So that in the vena portæ two things take place: 1. The old useless vesicles are taken out of the circulation. 2. The debris, or dead films of these vesicles are separated from the blood.

Function of the blood-vesicles. According to Professor Schultz's researches, the blood-vesicles have no direct connexion with the nutrition of the body, but are the true respiratory organs of the blood, and subservient to the completion of the process of assimilation. By the absorption of vital air, the granular substance is transformed into plasma, and the colouring matter is the residue of the transforming processes. It is by no means necessary that the respiration of the blood should be performed in lungs or gills: any surface may suffice to bring the vesicles in contact with the atmosphere, and in some animals the whole surface of the body is a respiring organ.

Nature and origin of the plasma. The plasma is the colourless organized plastic fluid in which the vesicles float. It is the coagulating portion of the blood. Coagulation takes place more quickly and perfectly in proportion as the vesicles are deposited. This process is not chemical, as is the coagulation of albumen, but a vital act developed in the death of the blood. The plasma is formed during the development of the granules of the vesicles with the aid of respiration. It is the true formative and nutritive material in the blood, and supplies all the structures, passing through the parietes of the vessels into the parenchyma of organs, which the vesicles never do. Consequently, there is less plasma in the returning venous blood than in arterial. The plasma takes up matters passing into the circulation. Indigo colours it green; but the blood-vesicles remain unchanged. The vesicles carrying oxygen excite the muscular and nervous system, the plasma is directed rather to the vegetative system.

The following are some of the pathological inferences Prof. Schultz deduces from these researches.

If the old deeply-coloured vesicles are not excreted from the circulation as new ones form, the blood assumes a darker tint, and the portal system is congested, because the old vesicles accumulate in the liver. And as such vesicles cannot undergo the necessary changes in the lungs, and carry oxygen into the system, a necessity for increased respiration is excited, and asthma and dyspnea are developed. The natural excretion of the vesicles may be altered by a want of tone and contractility in the containing membrane of the vesicle, as in chlorosis; or in a more saline or aqueous state of the blood, as in dropsies: in both diseases the formation of plasma is interrupted. The plasma may be also diminished in quantity from an imperfect development of the granules, itself dependent on imperfect digestion and assimilation; as in scrofula and scorbutus. In the lymphatic temperament the blood-vesicles develop themselves slowly, and the blood contains a large proportion of old vesicles. In the sanguineous temperament they run their course more quickly. Naturally the plasma is colourless and transparent, but in man and horses suffering from phthisis it is opaque and quite milky. Vegetable substances, as indigo, act upon the plasma: iodine has a special action on the membrane of the vesicles, staining it permanently of a brown colour, hardening it, and destroying its contractility. Neutral salts act on both the vesicles and plasma: narcotics on the vesicles only, destroying their contractility.

Let us now revert to the general views of our author. The last change the blood-vesicles undergoes is a *moulting*. Each vesicle has a cycle of

life precisely as each individual animal; and every vesicle has equally its birth, course of development, and death. The dead debris of each also must, like the moulted appendages of the skin in insects and vertebrata, be thrown off. The blood purifies itself from them, and the liver is the organ in which their exit from the circulation is made.

This process of depuration is not a purifying of the blood from foreign matters, but a self-purification from the moulted refuse thrown off during its own development. It is a true moulting of the blood itself, and as necessarily connected with the renewal and rejuvenescence of the blood, as is the cutaneous moulting of animals with contemporaneous renewal of other organs. It is a renewal in virtue of the periodicity of the vital processes carried on in the blood, and altogether independent of the renewal of unassimilated matters derived from without. It is not gaseous matters that are here excreted, but the fixed or solid residues of the coloured membranes of the vesicles which have passed through their cycles of existence. As the apple falls from the tree when it is ripe, as the leaf is loosened when dead, as the uterus expels the fœtus when at its full development, so the blood is moulted from those old vesicles that have fully completed their development. Since the vascular system has no direct emunctories through which the moulted debris may be thrown off, the circulation through the liver pours them out into the intestines as bile. The term vena portæ is prophetic, as that vein is a porta, a gate, in more than one sense.

We cannot follow our author through his researches into the functions of the liver, and the composition of the bile, made with reference to his peculiar views. His proof of the correctness of the latter are drawn from comparative anatomy and physiology and from direct experiments. The composition of the blood in the vena portæ is proved to differ from common venous blood; its colouring particles are darker, and are less affected by oxygen or neutral salts; it contains less plasma, and its plasma contains colouring matter; it has less albumen, and $1\frac{1}{2}$ and $2\frac{1}{2}$ per cent. respectively of fibrine more than venous and arterial blood. It also contains a greasy dark-brown fat, not found in either. It is nevertheless more aqueous, and its motion through the veins is slower, evidently from the diminished quantity of the plasma, since by the plasma the motion of the blood in the vessels is maintained. Professor Schultz observes that the colouring matter obtained from venous, arterial, and portal blood respectively, has peculiar qualities, and that the colouring matter of the portal blood is closely allied in chemical composition to the bile. Portal blood, however, is not wholly composed of moulted venous blood, as the lymphatics take up a residue from the chyle.

Renewal of the muscular and nervous structures. We readily agreed with Professor Schultz in his assumption that the muscles and nerves are renewed, and looked anxiously for some account of the microscopic changes they undergo during the process of renewal. In this we have been disappointed. As the muscles and nerves moult like all other organs, or in other words cast off the animal matter rendered effete by subserving to functional activity, there must be an organ or organs by which the debris are thrown off. These, according to Professor Schultz, are the kidneys and the perspiratory organs of the skin; and various facts are brought forward to substantiate this opinion. There are the

close relations between rheumatism and other muscular affections and the skin; and between the nervous system and the urinary secretion. The changes in the latter are very obvious in many affections: as for example, in all spasmodic affections, in diseases of the spinal cord, and in many hysterical disorders. It is not, however, every constituent of the urine which can be considered as the moulted matter of the nervous system, but exclusively urea and uric acid, and their modifications and compounds. These only are affected by increased or diminished activity in the nervous system, and the process of neurine renewal. The great similarity between the chemical composition of these substances and of the albuminoid matter of the nerves renders it also probable that the former are simply the result of a metamorphosis of the latter; just as the purpuric and cyanuric acids, and the urate of ammonia are only metamorphoses of urea and of uric acid.

The symptoms following the retention of urine in the system are such as might be expected on the supposition that it is the moulted matter of the nervous system. The critical urine of intermittent fever and of gout is explained as an illustration; as also the great diminution in the secretion of urea during the presence of spasmodic diseases. Nysten showed experimentally that in these cases there is only 1 per cent. instead of 3½ per cent. of urea in the urine. Professor Schultz refers to Dr. Addison's researches on the connexion between cerebral affections and those diseases of the kidney first clearly exhibited by Dr. Bright. He might with propriety have referred to the published opinions of Dr. Holland, Dr. Copland, and Dr. Laycock: to the two former for arguments as to the connexion between gout and certain constituents of the urinary secretion, and to the last mentioned for researches into the connexion between lesions of the urinary system analogous to the gout, and many anomalous nervous affections of females. (See Br. and For. Med. Rev. vol. XII. p. 73.) Pathological facts of this kind are numerous, and were well known to the old humoral pathologists. What the modern humoral pathology wants is a microscopic analysis of the vital changes in the blood, and those occurring during muscular and nervous action, and in the renewal of the muscle and nerve.

Renewal in relation with the respiratory process. Professor Schultz refers to his published Elements of Physiology for a demonstration of his opinion that the final cause of respiration is to revive the blood-vesicles after their circulation through the system and to exalt their development. It is not the lungs, but the blood which respires by means of the lungs; and it is the blood-vesicles which inspire and expire as the blood-lungs. The air taken into the lungs acts upon the granules of the vesicles, forming them into plasma, the true nutritious and life-exciting constituent of the blood. The life of the blood is at its climax during this metamorphosis. It is by a vital tonic power that the blood-vesicles draw in the air. This power commences in the lymphatic system, when the membrane or film first forms round the lymph-globule, and by which it is enabled to develop itself further, and act upon external agents just as the ovum when vivified is enabled to act on external agents. The cycle of life of the blood-vessels is from the alimentary canal through the lymphatics, and the venous system to the lungs; from the lungs through the arterial system to the portal system; and from thence through the

liver to the alimentary canal, where the process of vitalization recommences. Thus the blood is ever renewing, ever revitalizing itself.

Pathology of crises. Health consists in a balance between the two processes of reorganization and disorganization; disease in a preponderance of the latter over the former. The moulting of diseases is the return to due vital action; the renewal of health by the regeneration of the effete parts in the suffering organ. When this regeneration is normal, there are crises in the evolutions of structures: of this kind are the moulting of birds, the renewal of the hair in mammalia, the casting of the skin and scales in reptiles, the metamorphoses of embryos and insects. A crisis considered as an excreting process is only the termination of the moulting process. This analogy between the stages of disease and the natural processes going on in the system is carried out into details and the scholastic theories of crises, as coction and the like, are explained.

Relations of healthy and morbid renewal. Just as the natural processes lead to diseases so the morbid moultings or renewals lead to health. The natural exuviation and renewal may be interrupted in various ways. Thus, the interruption of the blood-moulting in the portal system may induce abdominal infarctions; interruption of the moulting of the skin may bring on pulmonary or hypochondriacal affections; of the lungs, may induce atrophy; of the alimentary canal, diseases of the brain and nervous system. Therapeutics consists in an artificial excitement of the natural exuviation. For example, artificial excitement of the exuviation and excretion of the blood-vesicles in the portal system may regenerate the whole digestive and nutritive processes. Diseases themselves often induce this favorable exaltation of the process. The hair, nails, and epidermis are thrown off, the epithelium of the alimentary canal removed, the effete debris of the blood-vesicles thoroughly expurgated from the portal system through the liver, and so the whole machine becomes in some degree new formed. Thus nature, in spite of science, renews the body after her own fashion.

Some phenomena resulting from interruption of the natural processes may be mentioned in illustration of our author's views. 1. The vital renewal may be imperfect, and the new formation, just as the embryo, may be an abortion, or may be imperfectly developed. This occurs in scrofula, in which the constituents of the lymph are arrested in the first stage of development, giving rise to irritation and an inflamed condition of the lymphatic glands. In chlorosis, the blood-vesicles are arrested in the lymph-stage, and remain pale, their membranes being as irritable as in fishes. The formation of the plasma is consequently imperfect, and there is a want of energy in the whole of the formative process. In persons of a phlegmatic temperament the muscles are arrested in the cellular stage of organization: in children of the nervous temperament the substance of the brain and nerves is not fully developed, the albuminous basis being imperfectly assimilated. 2. The vital action may be excessive, and a high degree of plasticity result, as in tonic plethora.

Cultivation of the renewing process. This is the heading of the third division of Professor Schultz's volume. The general principles which should guide us are first laid down. As might be inferred *a priori*, it is a fundamental rule that the equilibrium between reparation and destruc-

tion be maintained. Another general rule is that this equilibrium be maintained consonantly with the natural periodicity inherent in the system. The cause of this periodicity of vital change is exoteric.

"Excitement and repose, exhaustion and restoration, increase and diminution of functional activity, assimilation and resorption, all alternated in periodic change, determine the general laws of vitality. It is not an undefined general principle that is in operation, but an actually operating antithesis of vital functions with which we have to do. The periods are to be determined less by telluric periods, as the times of the day, than by the periodic activity occurring in the organism itself." (p. 111.)

With respect to the periods so often referred to by our author we may observe, that henceforth the first duty of an inquirer into the subject must be to determine the length of the periods. The fundamental idea of number once fairly introduced into physiology will be like a mental telescope or microscope to the philosophical inquirer. It will enlarge his sphere of intellectual vision, and give precision of outline to the general facts he may contemplate. The determination of the exact periods of moulting in insects, or of incubation in oviparous vertebrates, considered as stages of development, would be worth many volumes of vague statements and ingenious speculations.

Cultivation of the process of assimilation. We pass with a feeling of satisfaction from the preceding speculations to the practical rules respecting diet and regimen, and the proper culture of the digestive organs laid down by our author, and shall notice a few of the most remarkable of his observations and experiments.

1. The stomach should be well filled when eating, so as to press against the diaphragm, and abdominal muscles. This rule is inferred from experiments instituted by Professor Schultz leading to the conclusion that the rotatory motion of the stomach is essential to good digestion, and that this motion is most energetic when the viscus is well supported by the diaphragm and abdominal parietes.

2. The stomach should be perfectly empty before food is again introduced, because according to the laws of periodicity, a period of repose should always follow a period of activity. The reaction of hunger may be taken as a general indication of the necessity of a further supply of food: not always, however, for it is sometimes morbid, just as the perception of sounds, colours, &c. If the stomach be well filled, the digestive process requires from six to eight hours for its completion.

3. Suppers are not to be recommended, particularly in diseases of the large intestines. Professor Schultz adheres to his old original idea that in adults the cæcum performs the function of a second stomach, digesting the alimentary matters which from their hardness or other circumstances are not sufficiently acted on in that viscus. This cecal digestion is in every respect analogous to the ventricular process, requiring like it the proper digesting fluid, and particularly the bile, for its full completion. It is carried on principally in the evening; and since the liver cannot serve two masters, if the stomach be filled at night, the digestive process in the large intestine is interrupted.

4. The sensation of faintness in those subject to indigestion is increased by frequent eating, and cured most readily by fasting. This sensation, according to Professor Schultz is dependent upon an imperfect

chylification, and consequent on this is imperfect sanguification, and this imperfect chylification is induced by interrupting the ventricular and cecal digestion before the completion of the process, which too frequent ingestion of food will do.

5. Food should be properly cooked, for gross feeding and gross minds go together.

6. Professor Schultz contends, contrary to general opinion, that boiled meats are more digestible and nutritious than baked or roasted, and refers to his work *De Alimentorum Concoctione* for experiments in proof. For similar reasons he asserts that the invertebrate animals and cold-blooded fishes and reptiles are less digestible than warm-blooded animals. Fowls are more digestible than beef or mutton, and so indeed is pork. But much depends upon the mode of cooking and the age of the animal. Fat is very digestible, and not so liable to become rancid as is generally thought; it is also exceedingly nutritious. A case of consumption, according to Professor Schultz, was cured by the methodical use of goose-fat as an article of diet, although a great part of the lungs was destroyed. Cheese ranks after flesh in point of digestion: it has also the property of promoting the assimilation of other substances. Our author recommends it to be taken with oysters. Milk and eggs (which contain the milk of birds without the sugar) are not commendable as articles of diet.

We must pass over some judicious remarks on water and manufactured drinks, to make room for Professor Schultz's experiments respecting the physiological and pathological action of alcohol.

Action of alcohol on the blood. "Since physiological researches have shown that alcohol taken into the stomach is in great measure absorbed from the alimentary canal, and carried into the circulation unchanged, physicians have directed their attention exclusively to its action on the mass of the blood. The special changes excited by alcohol on the individual constituents of the blood have not yet been ascertained. It has been observed generally that the blood becomes of a more venous character, and contains more carbon and hydrogen, not however specifying which part of the blood suffered the change, whether the blood-vesicles or the plasma...... The chemical changes have certainly been inquired into, but without a knowledge of the fact that it is in the organic constituents and microscopic elements of the blood the true nature of the changes is to be found...... If a little spirit of wine be added to fresh blood a dark tint becomes perceptible to the naked eye. But upon closer examination it will be found that it has not really become darker.... If it be examined with a microscope it is found that the colouring matter has changed its locality: that it gradually passes from the vesicles into the plasma, and that the former appear at last as perfectly transparent, colourless bodies swimming about in the fluid of the plasma, or, if coagulated blood have been used, in the serum. The relations of the two constituents of the blood are in fact reversed, and instead of coloured vesicles swimming about in the colourless plasma, we have a uniformly red-coloured plasma and colourless vesicles. The colouring matter is chemically held in solution in the plasma, and consequently the darker shade of that fluid is only *mechanical* (?) When spirit of wine is added to fresh coagulable blood the red plasma changes into a gelatinous matter of the consistence of thick milk, no formation of clot or serum subsequently taking place." (p. 171.)

Professor Schultz contends that the analogy between the action of diluted acids and alcohol on the blood is more apparent than real. The

acids coagulate the albumen, but the colouring matter is not acted on by the alcohol, unless it be mixed with the blood in large proportions when the albumen is coagulated. The decolorization of the vesicles by alcohol does not take place immediately but gradually, and is more or less perfect according to the quantity used. If the contraction in the vesicles excited by the alcohol be kept up continuously, the vesicles contract to the size of a point, and at last disappear altogether, and the blood exhibits the appearance of a uniform transparent red fluid, without perceptible red globules. The newly-formed young vesicles contract the most energetically; the older and more deeply coloured resist its action longer. The vesicles that have become perfectly colourless and quite contracted exhibit only chemical properties, and have lost all vitality and excitability. The action of narcotics on the blood is very different from that of alcohol. Narcotics destroy the contractility of the vesicles so that they are paralysed, and remain expanded, and the accumulation of the colouring matter takes place in the vesicles only.

The action of alcohol then on the blood is not chemical but vital. It acts as a stimulant to an increased and unnatural contraction of the vesicles by which they are deprived of colouring matter, and hurried on to the last stage of development or death. Now, since the activity of the respiratory process depends upon the vital activity of the vesicles, it is a necessary inference that less oxygen will be absorbed and less carbonic acid gas excreted in proportion as the vesicles are devitalized; and this is the true explanation of the venous character peculiar to the blood of drunkards. The formation of plasma, the true nutritive material of the blood, is interrupted *pari passu* as the respiratory process is interrupted, and the plasma itself becomes an irritant to the circulatory and secreting organs from the presence of the colouring matter of the vesicles. So that while on the one hand congestion is taking place in the capillaries, there is an unnatural irritation of the secreting organs on the other; the necessary result being interruption of function.

Action of alcohol on the spleen and liver. The explanation of the enlargement of the spleen observed in drunkards is connected with a theory of our author's respecting the functions of the spleen, in virtue of which that organ is considered a part of the lymphatic system, and subservient to the depuration of the blood. The digestive process being interrupted by the change in the organic constituents of the blood, the food is imperfectly assimilated, and raw nutritive matter carried with the chyle into the circulation. The office of the lymphatic system in the spleen being to further assimilate this raw material, that organ becomes overfilled with blood in proportion as the unassimilated constituents of the chyle increase.

Professor Schultz observes with respect to the liver that there is a double series of phenomena. First, the lymphatic system of the liver is placed in the same abnormal position as that of the spleen: and secondly, there is portal congestion. The change in the biliary system is connected with the general changes in the blood already referred to. It is not simply the effete carbonaceous colouring matter which is transferred from the vesicles to the plasma, but the arterial oxygenizable colouring matter also, and the latter is as unfit for the formation of bile as the blood of the hepatic artery itself.

Professor Schultz connects the pathology of delirium tremens or drunkard's madness, with the physiological changes excited by alcohol in the blood. Unnaturally stimulant plasma on the one hand, and the disorganized vesicles on the other lead to a state of asthenic irritation, a subinflammatory and congestive condition of the brain and spinal cord. It is not an exhaustion of these organs from excessive stimulation that is the cause of the delirium: it is rather a qualitative disorganization and destruction of the exciting process induced by the change in the exciting powers of the blood.

Professor Schultz questions very much the correctness of the principles of total abstinence so enthusiastically advocated by the "tee-totallers." The practice of taking stimulants is so generally diffused, and it is of so great antiquity that it may be considered instinctive. If men eschew brandy, they will chew opium; or if they eschew opium, they will chew betel, or tipple with wine. The tee-totallers, Professor Schultz thinks, may possibly do more harm than good, or to use his own expressive phrase, may lead us out of the rain to the spout, and German spouts *are* water-spouts in a raining day. He thinks it a better plan to render spirituous drinks less injurious by assimilating them to wine; in fact, by encouraging the manufacture of made wines, for our author argues that wine is comparatively innocuous from its peculiar composition. He gives the following recipes for two kinds of made wines; we should call them punch.

Citron wine. The juice of twelve citrons: two quarts of spirit, and four quarts water: one pound of sugar. Mix together, and set aside for several weeks until clarified and ether is formed.

Essig-ether wine. Four ounces of wine-vinegar: two quarts of spirit at 80: four quarts of water: two pounds of honey, or one pound of sugar. Set aside until ether is formed.

Culture of the digestive process. Professor Schultz thinks that late are to be preferred to early dinners, and commends the English and French habits as being superior to the German, and closely resembling the ancient Roman custom in this respect. An early dinner involves the necessity of a supper, and supper (as has been already shown) interferes injuriously with the cecal digestion, especially in adults. The Romans had five legitimate meals: breakfast (jentaculum), lunch (prandium), tea (merenda), and supper (cœna); the fifth being a jollification after it (commissatio). But only one of the five was a substantial, physiological meal. The *jentaculum* was taken by children alone. The *prandium* was only a snack, and as for the *merenda* and *commissatio*, the one was exclusively taken by slaves and labourers, and the other by the drunken sots who in modern phrase are termed jolly good fellows. But was the Roman cœna a five-o'clock, or a six-o'clock, or an eight-o'clock dinner? and if an eight o'clock dinner, how can Professor Schultz lay blame to his countrymen for dining at half-past one, and supping at half-past eight? Probably, a dinner at five or six o'clock is preferable to any other; at a later hour, it would be a supper, at least physiologically; at an earlier, a *bona fide* supper would be necessary. A six-o'clock dinner gives the stomach time to be prepared for a substantial jentaculum at nine next morning:—a true Roman prandium, an Abernethy biscuit and a glass of wine and water at one will keep it from grumbling until six. The

choice between a commissatio and converzatione is of course a matter of good taste and morals, except to the invalid.

New experiments on the physiology of digestion. Considering how much health and long life are dependent upon a due culture of the assimilating powers, Professor Schultz instituted a series of experiments on the physiology of digestion. He observes very justly, that the generally received opinions respecting the agency of the gastric juice are very unsatisfactory. They give no good account of mastication or rumination, or of the part the saliva takes in digestion, although digestion is performed in the mouth in many amphibia, and other animals have salivary glands in the stomach itself. Before relating the new experiments of our author, we shall give the results of those already published in his work *De Alimentorum concoctione:*

"1. That, excepting during digestion the secretions of the stomach are always alkaline; they are even alkaline at the commencement of digestion in many animals, and in those which fast long, as the hybernants, the contents of the intestinal canal are alkaline also.

"2. That the degree of acidity in the chyme in the different divisions of the stomach corresponds closely with the stage of digestion, is altogether different as the food is different, and becomes less as digestion is prolonged.

"3. That there is much less acid formed in the stomach during digestion, than is requisite to chemical action on the food. Chyme from vegetable food requires from one to one and a half per cent. of carbonate of soda for saturation; chyme from cheese requires from 1 to $1\frac{3}{4}$ per cent.; from flesh 2 to $2\frac{1}{2}$ per cent. All the chyme in the stomach of a calf, amounting to about four ounces, was saturated by less than half a drachm of the carbonate. From forty to sixty grains at the most will saturate any quantity that may be found in the stomach of a dog.

"4. That the stomach like all other mucous membranes secretes mucus only, but is specially adapted for absorption of fluids: consequently, the stomachs of fasting men and animals are quite empty.

"5. That there is a great quantity of saliva being constantly secreted and also swallowed, even when digestion is not going on; that in animals furnished with a stomach having a horny lining, the saliva thus swallowed collects in considerable quantities: that in animals in which the mucous membrane of the stomach is naked, the saliva becomes concentrated by the absorption of the watery portion, leaving the fixed constituents in the stomach. Many ounces of this concentrated saliva may be found in the empty stomach of the horse. The parotid gland of a single horse secreted more that 100 ounces of saliva in twenty-four hours.

"6. That, consequently, all fluids found in the stomach, or extracted from the mucous membrane necessarily contain salivary matter, or rather saliva in a concentrated state; and also, that all gastric juices which have been collected or prepared for experiments contain the salivary constituents.

"To these statements may be added—*a.* That, as Spallangani well knew, no food can be digested artificially without saliva. *b.* That the chyme is not a chemical solution but an organic compound formed by the transformation of the constituents of the food; as for example, starch being transformed into sugar, sugar into acids: changes which never take place in the common experiments on artificial digestion. *c.* That the acid of the stomach is a product of digestion: that the alkaline contents of the stomach are first saturated before free acid is present, and that the latter during fasting gradually disappears, and an alkaline condition is restored; the period required for the completion of the changes varying in length from six to twelve hours. *d.* Permanent acidity is only observed in diseased states of the stomach, as in heart-burn, in which the digestion is impaired, although according to the chemical theory it ought

to be energetic. Fermentation and the formation of carbonic acid gas, which never take place in healthy digestion, result from this diseased condition. *e.* The ingestion of acids during digestion interrupts that process under all circumstances.

"From all these statements it follows that the fluid which physiologists have termed the gastric juice, is only a medley of various things taken into the stomach, and not, with the exception of the mucus, secreted by it. The acid is a chemical product from the food produced as the latter is continually agitated with the chyme. But above all, the acid gastric juice obtained by experimentalists from healthy men and dogs contained acid chyme. *There is, in fact, no such thing as acid gastric juice, but only sour chyme.*" (pp. 187-190.)

The flat contradiction here given generally to the inferences from Dr. Beaumont's experiments on the Canadian voyageur is followed up in a note by a special reference to those experiments. This contradiction is, however, pushed to its utmost limit when our author declares, in the very teeth of Dr. Beaumont's conclusions, that the saliva is the main agent in the digestive process. As this view has been advocated from experiments by at least one physician in England, we refer to Dr. Wright, and as it is also founded by Professor Schultz on carefully-made experiments, we shall notice it in detail as far as our crowded volume will allow.

New experiments on the digesting power of the saliva. Prof. Schultz first prepared the saliva used in these experiments by concentrating it, for some, to the consistence of thin syrup; for others, to a state of perfect dryness. In the latter state it was reduced to a white friable mass, containing nevertheless the true digesting element; and formed when rubbed up with five or six parts of water a milky fluid, having the same proportion of fixed constituents as the less concentrated saliva, but constituting a fluid less effectual than the latter. The following are the results of these experiments:

"1. The transformation of potato-meal and of flour into sugar, by digestion in saliva at blood heat, was effected more rapidly in the concentrated than in the unconcentrated saliva. Transformation commenced in half an hour, and was completed in from two to three hours with an adequate supply of saliva.

"2. The mass thus acted upon by concentrated saliva acquired equally with unconcentrated saliva the power of changing unboiled starch into sugar. Free acid is formed in a small quantity only, just as in the digestion of boiled starch in the stomach.

"3. Cheese digested with concentrated saliva is acted upon almost immediately, and changed in from two to three hours into a chyme-like mass.

"4. Cooked muscular fibre colliquesces in like manner, the fibrils melting away as I have figured in my previous work *De Aliment. Concoct.*, p. 7, and Fig. 1-8. The reduction, however, is not so rapid as that of cheese. This point appears to me of considerable importance, and is one not noticed in the accounts of the earlier attempts at artificial digestion. It must be observed that large portions of flesh are only chymified on the surface, so that the chyme thus formed must be removed by moving the portion under experiment, so that the subjacent parts may be duly exposed to the action of the digestive fluid. Minute pieces colliquesce much sooner than large. The concentrated saliva of the horse (which is very alkaline) digests flesh and cheese more readily, if the alkali it contains be previously neutralized either wholly or in part. On the other hand, the transformation of vegetable matters, as from starch to sugar, is more quickly effected by strong alkaline saliva.

"5. The saliva of the dog is less alkaline, and acts on animal fibre like the neutralized saliva of the horse,

"6. The digestion of animal fibre in the saliva of a horse is facilitated by mixing vegetable matters with it, as boiled starch." (p. 191-3.)

The rules for promoting digestion may be easily gathered from the preceding experiments. In the first place, the food must be well masticated. This rule every physician commends to his dyspeptic patients. But thorough mastication demands the exercise of good teeth and active jaws. The masseters rarely fail; the teeth often. The cause of dental decay is well hinted at by our author. Physiologically, the teeth are products of the epidermis, and their healthy condition is as much influenced by that of the mucous membrane of the alimentary canal, as the cuticle and hair are by the condition of the cutaneous organs. To brush and scrub the teeth is well enough when they are sound and the digestive powers are good; but when decay begins to take place attention must be directed to the digestive organs, if the progress of that decay is to be arrested. The first consideration will be not to slip the food down imperfectly masticated for fear of exciting toothach, but to comminute the food more minutely before putting it into the mouth, and then mixing it well with the saliva. It is exceedingly probable that the decayed teeth for which the Germans and Americans are nationally notorious, are dependent in a great degree on the too rapid ingestion of their food, so common with all classes of these people.

The secretion of saliva continues after completion of the meals, sometimes to an extraordinary amount. (De Alim. Concoct., p. 57.) To swallow the saliva thus secreted is the best, because natural, remedy against morbid acidity. If the post-prandial secretion be defective, sweetmeats are its best excitants.

In delicate persons the *temperature* of the stomach may be too low for proper chymification, and warm drinks should be taken. Too great a quantity also of acid may be formed towards the end of the process. If this be the case, soda-water is commendable, or the carbonate of soda, or "fluid magnesia," previously diluted, or a flow of saliva may be excited, and the product swallowed.

Culture of chylification. A few words under this head must suffice. The chyme passing into the duodenum is further transformed into the elements of chyle, namely albumen and fat. There the bile has a duty to perform with the chyme analogous to that of the saliva on the raw material. It de-oxydizes the oxydized constituents of the chyme;—this process failing, saccharine matter is taken up into the circulation from the bowels, as occurs in diabetes mellitus. Professor Schultz thus ascertained by actual experiments that one part of chyme requires two parts of bile to de-oxydate it; consequently, if the chyme passing into the duodenum after each digestion be estimated at six ounces, twelve ounces of bile must be excreted: now, as the gall-bladder can contain from one to two ounces only, the rest must come from the liver direct. So that the gall-bladder is only a receptacle for the bile secreted in the intervals of digestion.

Moulting of the intestinal canal. At the time we were amusing ourselves with Professor Schultz's queer title, Mr. Goodsir's observations on digestion and the absorption of chyle came under our notice, (see Br. and For. Med. Rev. vol. XIV, p. 566, and Dr. Carpenter's most interesting Report in last Volume). The curious fact stated by Mr.

Goodsir, namely, that there is a multitude of cells continually forming on the surface of the villi for a specific purpose, whose life is but of a very brief duration, and whose debris fall away into the chyme, seemed singularly in harmony with Professor Schultz's researches on the life and death of the blood-vesicles: and we must confess that we turned with no little eagerness to see whether in the section now under notice Professor Schultz took cognizance of these moulting vesicles of the villi, discovered by Mr. Goodsir. His general views are as follows:

"Hitherto the formation of fæces in the intestines has been looked upon as merely a mechanical separation of the indigestible portion of the food. Nevertheless, the vital process of chylification, and the moulting of the alimentary apparatus which commences in the mouth and salivary glands, and attains to its highest intensity at the lower end of the intestines, are closely connected with it. The alkaline salts of the saliva are neutralized by the acid in the stomach; the salivary matter appears for the most part to become ammonia, which also being neutralized in the stomach and large intestine, is carried downwards; and thus we can explain how it is that so large a quantity of acetate of ammonia and the nitrates are found in the excrements. The bile is precipitated by the acid of the chyme, and carried to the excrement as the so-called acid of the bile; and the intestinal mucus is no other thing than the moulting of the layer of the mucous membrane which becomes attached to the surface of the feces in the lower portion of the canal. This moulting is a necessary condition for the renewal of the intestinal mucous membrane, and its interruption is followed by cessation of all the functions; since the muscular layer becomes stretched, and the movements of the intestines interrupted. The secretion of a fat in the lower portion of the large intestines rich in carbon and smelling strongly is analogous to the biliary secretion. The uninterrupted progress of the intestinal moulting specially favours the process of renewal carried on during the formation of chyle, and its disturbance can cripple the whole vital process of assimilation. There are three things here to be specially considered in dietetics. 1. The reciprocal decomposition of the chyme and the bile. 2. The separation of the chyle from the products moulted during the process of its formation. 3. The purifying from mucus of the whole canal." (p. 200-1.)

According to Mr. Goodsir's observations great numbers of epithelium cells are being continually formed and moulted (in Schultzian phrase) during chylification. When the gut contains no chyme, the development of new vesicles ceases. During the interval of repose, the epithelium is renewed. We can only find room for one of the dietetic maxims Professor Schultz founds upon his researches: that one is this—If the stomach and cæcum be excited on the one hand into action, they must not on the other be deprived of their periodic refreshment and repose.

Culture of the renewal of the blood. New experiments on the formation of blood. This is an attractive title for all who may be half-past fifty; for the blood, as Professor Schultz observes, is really the fountain of life. How many sexagenarians sigh and audibly express their wish to have young blood in their veins!

We pass over the history of the youthifying process by transfusion of blood, to the more practical subject of the culture of the blood in its formation. This brings us back to the chyle and to digestion. The products of that process handed over to the lymphatic system for the renewal of the blood are albumen and fat. It has been ascertained by experiments of Majendie and others, that a variety of food is necessary to pro-

long life; but the microscope had not been used for the purpose of ascertaining the changes induced in the organic constituents of the blood during these researches. Professor Schultz therefore made some experiments himself. Three dogs, fed with pure olive oil, all died within seven days: one on the 5th, one on the 6th, and the other was killed when in a dying state on the same day. On examining the latter, it was found that the oil had only been partly chymified. Numerous mucous cells were in the chyme found in the stomach. There was a small quantity of milk-white chyle in the lacteals and thoracic duct without any admixture, however, of the red vesicles. Examined under the microscope it was found to consist principally of distinct, perfectly round fat-globules. The plasma seemed imperfectly formed, and its coagulating power was weak. The plasma of the blood in the arteries was deeply reddened; the vesicles slightly coloured, deprived of all turgescence, and collapsed in a remarkable manner, with angular, folded, and wrinkled forms, without granules, like dead vesicles many days old. There was a great number of oil-globules in the blood and bile, resembling those found in the lymph. It was evident also that oily deposits in the parenchyma of organs had taken place. The liver was impregnated with microscopic globules of oil, as was also the spleen. But the lungs were the most remarkably altered. They were collapsed, bloodless, and of a bright red colour. Oil-globules were deposited in the tissue connecting the bronchial ramifications with the blood-vessels in such quantities, that nothing but cells full of them were apparent, the bronchial cells being compressed by them: so that it is probable little of the oil was chymified, the remainder being absorbed by the veins, and mixed mechanically with the blood. Professor Schultz traces the whole series of phenomena described, and comes to the conclusion that no vesicles were formed round the lymph-globules. In consequence of this, the regular metamorphosis of the blood-constituents, and the development of the plasma were at once hindered.

Three dogs were fed with raw potato-flour. One of them died on the 10th day; the other two were killed on the 11th, when just dying. With the exception of the deposit of oil in organs, the changes observed, especially in the constituents of the blood, were as described above.

We have some details of experiments instituted to ascertain the amount of fat produced during chylification. The general result is that nearly one third of the chyle is transformed into fat.

Formation of lymph-globules and vesicles. The formation of lymph-globules is the first act of the vital process after the chyle has passed into the lymphatic vessels. A separation of the constituents of the lymph takes place into the thicker globules and the thin plasma of the lymph, the former having fat as their basis, the latter albumen. The formation of vesicles round the lymph-globules is the second act of the vital process. We have already alluded to this, and as Professor Schultz adds nothing of importance to his previous statements, we pass on to consider the next subject.

Variations in the organic constituents of the blood in different classes of animals. The blood-vesicles are most contractile in the carnivora, and are large but less numerous than in the herbivora. The quantity of

colouring matter differs very much in different classes: in fact, this extreme variation has given origin to the division into white and red blooded. All animals have, however, red blood: it is the habitat of the colouring matter which makes the difference: *all invertebrata have white vesicles and coloured plasma*; *all vertebrata have coloured vesicles and white plasma*. This principle is uniformly applicable, for even the red-blooded invertebrates have white blood-vesicles.

Ripening of the blood. The ideas of Professor Schultz respecting the action of the lymphatic glands on the blood are curious. It will be obvious that the perfection of the blood depends on the respiratory powers of the blood-vesicles. The degree of organization in the organized constituents of the blood, is closely connected with the structure of the respiratory organs, and the mode in which the air is brought into contact with the circulating fluid. In the embryo the complete formation of the blood only commences with respiration. The great end of respiration is the organization of the plasma through the metamorphosis of the granules in the vesicles: at the same time the membranes of the vesicles attain their full growth and highest stage of development, as may be seen in the embryos of fishes, but more particularly of the amphibia: but on the other hand, if the organic constituents are imperfect the results of respiration are imperfect. In the water-breathing of fishes the development of the vesicles attains only the stage of lymph-development in the warm-blooded animals, particularly the mammalia. It is worthy notice, however, that animals breathing by gills have no lymphatic glands, and that the placental respiration of the fœtal state is precisely analogous to the permanent respiration through water, the blood carrying the oxygen in the one case, as the water in the other. Now, the lymphatic glands act after birth as placentæ, and so are subservient to the formation of lymph; but in all cases the development of the vesicles remains at an inferior stage, whether it be in the lymphatics in fœtuses, or in fishes. The embryos of birds present a remarkable exception, since the air is applied through the allantoid directly to the organic constituents, just as it is after birth.

The perfection of the blood may be prevented in various ways. If the plasma be too alkaline, or the contrary, its capacity for coagulation is diminished; it is too fluid, and the fatty matter of the lymph-globules and of the granules is not properly transformed. The plasma is, nevertheless, alkaline in health. Two drops of acetous acid neutralize one drachm of serum. The serum of a person labouring under diabetes required from a half to one minim only; of a scrofulous person only one drop to four drachms; and in another Professor Schultz found the serum quite neutral. In fact, the incapacity to coagulate was found to be in proportion to the want of alkalinity. The cause of this is the excessive formation of acid in the alimentary canal, which passes into the plasma, and neutralizes the alkali. The imperfect formation of granules depends upon an excess of carbonaceous food over the nitrogenized. Too much acid in the alimentary canal will have a similar effect by rendering the bile incapable of completing chylification, and so hindering the formation of fatty matter. Food in which nitrogen is deficient gives rise to imperfectly formed vesicles like those of the embryo. These

absorb oxygen imperfectly, and remain light-coloured from the resulting deficiency in colouring matter. Such vesicles are also weak, and incontractile, and cannot give out the carbon they take up in the system; and for a similar reason they readily give out their colouring matter to the plasma, thus rendering the latter fluid an irritant to the capillaries, and giving rise to the so-called inflammatory diathesis. When with this want of contractility of the vesicles, the granules are rapidly and imperfectly developed, and the plasma remains albuminous and less fit for the formation of fibrine, we have the state which constitutes the hemorrhagic diathesis. In all those cases, the evolution of animal heat is less than natural. As a preponderance of vegetable food is followed by a too rapid development of the granules and debility of the vesicles, so a preponderance of animal food gives rise to a predominance of the vesicles over the granules; oxygen being absorbed more freely than is required, an inflammatory excitement is developed in the intestines, and in the capillaries of both the muscular and nervous systems. This condition is observable in gout and the gouty diathesis. Further, it is necessary that the organic constituents of the blood pass through their embryo state just as the embryo itself, before they can be perfectly developed: and as the lymphatic glands are the gills and placentæ of the system, if these perform their functions imperfectly, as in scrofulous constitutions, a deposit from the unripe blood takes place. Hence the development of chlorosis and phthisis.

Such are some of the general views of our author: for a more particular detail we must refer our readers to his book, promising them at the same time much amusement and some instruction from the perusal. Numerous practical rules in dietetics are laid down, as flowing from these principles. In referring to the culture of the blood, Professor Schultz states the curious general principle, that the blood-vesicles must be considered as true animal leaves. Like leaves, they have a period of birth, perfection, and decay, and just as the former may exist for a month, or a year, or many years, so may the blood-vesicles. The blood-vesicles of the amphibia have the slowest development and the longest life; those of birds, mammals and men, have the quickest development and shortest life. Under this head a new pathology of the blood is developed, and the causes of chlorotic, venous, melanotic, and bilious blood explained. Menstruation and hemorrhoidal flux are shown to be identical, and in close relation with, and complementary to the portal system. They carry off, in fact, the moulted effete vesicles, the sordes of the blood. Professor Schultz's views are supported by microscopic researches into the nature of these constitutional discharges. These we subjoin.

Microscopic analysis of hemorrhoidal blood. Only a small quantity, about a teaspoonful, could be collected. It was not reddened by oxygen or common salt and did not coagulate. The vesicles generally were filled with colouring matter, although a great portion of the latter was present also in the serous plasma. The dark red vesicles were for the most part without granules, were scarcely transparent even at the margins, which were collapsed, and fallen in, and did not contract on the addition of salt to the blood. There were a few freshly-formed vesicles here and there, but, altogether, the fluid strongly resembles portal blood.

Analysis of the menstrual discharge. A teaspoonful collected from a healthy female was neither a bright nor a dark red, but rather of a light dull red. The vesicles were heavy, and formed a precipitate which when the discharge was spread out on a plate formed to the naked eye a red cheese-like coagulum. Observed through the microscope this apparent coagulum was found to be composed of perfect vesicles heaped together, yet isolated, and not adherent to each other, as easily happens to the vesicles in living arterial and venous blood. Saline solutions reddened it a little, but almost imperceptibly. The vesicles swam about in a serous fluid not coagulable like the plasma, and somewhat reddened by colouring matter. They differed in colour, some were very small and colourless, others (the heaviest) were impregnated with colouring matter, and these were also the largest. The latter were also found to wrinkle when placed in a saline solution. In all, with the exception of some perfectly formed, unchanged vesicles, the granules are either altogether wanting or remarkably small. In the small, colourless, round vesicles, the membrane is drawn thickly round these little granules, so that they present the appearance of lymph-globules. The addition of iodine shows their real nature. From these and other less remarkable appearances, Professor Schultz is of opinion that the menstrual secretion is analogous to portal blood.

Physiology and pathology of the water-cure. Professor Schultz instituted experiments on horses and oxen, to ascertain the changes induced in the blood by water-drinking, and by mechanical dilution. We have not room for the details, but the general results are that estimating the mass of blood in a man at thirty pounds, 17·36 ounces of water may be added to the circulation by drinking it; and that in proportion as the blood becomes more watery a separation of the colouring matter from the vesicles is more extensive and its solution in the plasma more facile. In the water-cure three things are to be considered, namely, 1, the water itself, as water; 2, its temperature; and 3, its chemical constituents. Professor Schultz shows that the temperature is the main agent in the Priessnitz method, the alternation of cold and heat acting upon the skin, and inducing reaction and violent diaphoresis. In young constitutions and those in whom the organs are not structurally diseased, the diaphoretic method of Gräffenberg may be beneficial, but it cannot be considered otherwise than as a cruel and dangerous remedy for weak, elderly people. Perspirations induced by vigorous exercise, medicines being administered as auxiliary thereto, are from our experience exceedingly serviceable in those diseases in which the water-cure is found most beneficial, namely, chronic visceral disease in gouty subjects past middle age, or in individuals who have lived fast. When the separation of the colouring matter from the vesicles is indicated, means less irksome than the water-cure may be adopted. This indication is readily fulfilled, Professor Schultz asserts, by drinking acidulated drinks, in fact by the *acid*-cure. Melanotic, bilious cachexies have yielded to the ingestion of two quarts of acidulated lemonade daily: one quart to be taken in the forenoon, and another in the afternoon.

We shall now close this notice of Professor Schultz's work, with the observation that our limited space has not permitted us to do full justice

to the new and very curious views it contains. We heartily commend it to the perusal of all who take an interest in the progress of scientific medicine. It is not free from many and great faults. Distant, very distant analogies are allowed to have the force of first principles: facts are mixed up in such a way with inferences that we have often found it impossible to separate the one from the other, and in the chapter on the renewal of the soul-life, downright assumptions and fanciful speculations are made the basis of grave psychical gymnastics and dietetics. Nevertheless, the book is a good book and a praiseworthy, and full of truths. Even if less interesting, it would be valuable as an antagonism to the Liebig school of bio-chemical philosophy.

Art. XV.

Solution du Problème de la Population et de la Subsistance, &c. Par Charles Loudon, Docteur en Médecine, &c.—*Paris*, 1842. 8vo, pp. 336.

A Solution of the Problem of Population and Subsistence; in a Series of Letters to a Physician. By Charles Loudon, m.d. &c.—*Paris*, 1842.

In this publication Dr. Loudon discusses a plan calculated, as he thinks, to prevent population increasing beyond the means of subsistence. It is interesting to the profession, as it professes to be based on certain principles of physiology.

The first four letters are occupied with Malthusian doctrines, and the last two with desultory politico-economical discussions of our poor laws, colonial government, and emigration, Mr. Doubleday's theory of the census, a systematic plan for the division of labour, and the opinion of St. Clement on the exact period of the birth of our Saviour. The remaining letters resolve the problem.

On a previous occasion we observed that the desire of rising in the world, while it checked population, gave rise to the greatest moral evils. The desire to rise does not annihilate the sexual desire; it only substitutes the concubine or prostitute for the chaste wife; and drunkenness, debauchery, and all the crime attendant on loose morals take the place of the quiet virtues of domestic life. Dr. Loudon is of opinion that the number of prostitutes in Great Britain has been much exaggerated; he estimates it at from 60,000 to 70,000, and supposes there is an equal number of females of doubtful character. The mean duration of a prostitute's career is six or seven years; so that the annual demand for virtuous females in Great Britain to recruit the ranks of prostitution is at least 8000 or 9000, or about one every hour. He estimates the annual number of bastards at 40,000; but these are not in general the children of prostitutes, but of virtuous females who have been seduced. As a secondary evil connected with prostitution, Dr. Loudon notices the venereal disease, and quotes the opinions of Richerand and Ricord to the effect that scarcely one per cent. of the men in the middle and higher classes escape infection. Adultery is another evil originating from the moral check alluded to; for men who marry late, take wives much

younger than themselves, and hence unhappy matches, since the original disparity in years increases in a geometric ratio as the ages of the parties advance. The sale of young females to old debauchees is another evil cognate with the preceding. Dr. Loudon heard of two instances of such sales in Paris; the prices were £600 and £1000. Other results of this moral check of Malthus are mentioned, and it appears that it is no check at all except upon morals and good order. The licentiousness and debauchery consequent upon the delay of marriage in the higher and middle classes spread with all the rapidity of evil example to the lower, and boys of seventeen have their concubines of sixteen years of age. So far we entirely agree with Dr. Loudon.

Dr. Loudon thinks the laws of nature and of Providence are alike, and that Malthus, not being either a physician or physiologist, could not possibly trace their identity. This our author proposes to do, and so solve his problem. It is a law of nature, according to Dr. Loudon, that man should be monogamous, and that the age of complete puberty ought to mark the period of marriage. This he fixes physiologically at the age of twenty-one years, and shows that marriage at this age has been always accompanied by an improvement in morals. The sexual feelings, however, are strongly developed anteriorly to this period; and to restrain these Dr. Loudon proposes the moral check of affiancing the parties for two or three years previously to marriage; a regular and legal ceremony being performed when the engagement is entered into. We can see nothing objectionable in these plans except their impracticability. It is evident the moral check on marriage at so early an age will still operate; and bachelors still dread the burden of a wife and family, in spite of Dr. Loudon's eloquent arguments in favour of a virtuous union. Our author is prepared for this objection, and insists that if the law of Providence or of nature be followed in one period of procreation, it must be followed in another: and that if it be a law that marriage take place at the age of twenty-one years, it is an equally stringent law that lactation shall be triennial. By this means the number of children will be diminished because pregnancy does not occur during lactation; and in addition, a more healthy state of both mother and offspring will be secured, all the objections to the contrary being set aside; for prolonged suckling neither debilitates the mother nor spoils the beauty of her bust, nor renders the children puny.

These laws being demonstrated, Dr. Loudon calls upon medical practitioners to assist in applying the true, because physiological, check upon population, and so become the greatest benefactors of their species. They are to point out to mothers, as eloquently as possible, the importance and necessity of triennial lactation, and will thus not only do much to prevent prostitution, debauchery, adultery, the sale of virgin chastity, and pinching poverty, in the present generation; but also control that frightful increase of population, which threatens to render it necessary at no far distant period, either to castrate and spay the young folk, or devour the old; a sad dilemma, indeed.

Dr. Loudon will see that we differ from him in his views with respect to the expected increase of population. We think no one measure adequate to control its progress, but we are certain that that general ame-

lioration of man's state termed civilization, and which is the result of numerous concurrent causes, will balance population and subsistence. Independently of these considerations, Dr. Loudon has left out the very key-stone of his plan. He has not proved that lactation, and especially triennial lactation, prevents the recurrence of conception. Dr. Loudon's staple in this matter is not statistics. We have physiological inferences and literary quotations to satiety; but Deville Carystias with his hypothetical sevens is no authority, and the second Book of Maccabees is at least physiologically apocryphal. We would even venture to question whether the opinions of "Nurse" in Romeo and Juliet are to be considered of weight, although Dr. Dickson would think them conclusive. Dr. Loudon refers to the only statistical inquiries within his reach, and these are certainly opposed to his views; we refer to the inquiries of Mr. Roberton of Manchester, who found that fifty per cent. of the females belonging to the labouring classes in that town became pregnant during lactation. Dr. Loudon explains away this opposing statement very ingeniously, by observing that the females of the class alluded to are away from their children during the whole day, and only return at night to find them asleep. In fact, they do not properly, that is to say, physiologically, suckle their children; consequently, conception may readily take place. In natural lactation, according to Dr. Loudon, the infant ought to have the breast every two or three hours. There lately appeared in the Dublin Medical Press, some statistical inquiries made by Dr. Laycock of York on this subject, which corroborate Mr. Roberton's deductions, although the individuals of whom the inquiries were made were taken from different classes of society. It appears that 209 conceptions took place during 766 lactations, 27 per cent.; and that of 135 married females, 76 or 56 per cent., became pregnant while suckling, according to the highest estimate, and 33·9 per cent. according to the lowest. We refer to the paper itself. That there is some antagonism between the mammæ and ovaria or uterus is probable; if, however, this be fully conceded, it is obvious that the balance between these organs is readily disturbed in favour of the uterus or ovaria, whenever anything occurs to interrupt lactation even in a slight degree. These interruptions are of necessity continually occurring in civilized life, especially among the labouring classes, and rendering the law inoperative. What then becomes of Dr. Loudon's plan to affiance at an early age and marry at twenty-one?

Having differed so much from Dr. Loudon's views we must do him the justice to say, that this attempt to elucidate political economy by the laws of physiology, and to improve the morals of the people through medical doctrines and precepts, is exceedingly meritorious, and we cordially wish Dr. Loudon success in similar future undertakings. The style of the work exhibits haste and carelessness; but Dr. Loudon appears to have been limited to time by a wish to get out his publication previously to the discussion of the poor-laws during last session.

ART. XVI.

A System of Clinical Medicine. By R. J. GRAVES, M.D. M.R.I.A., Physician to the Meath Hospital, Dublin, &c. &c.—*Dublin*, 1843. 8vo, pp. 937.

IN giving an account of the contents of this bulky volume it is unnecessary that we should enter into any lengthened introduction. The name and high reputation of its distinguished author are known to all our readers, and much of the materials of which it is composed has been already for some time before the public. The form in which they now appear will be found to differ chiefly in the bringing together and comparing of what had been before delivered at different times, in the suppression of some redundancies, and in the correction or illustration of the remarks from subsequent experience and accumulated facts. It is well known that from time to time several admirable clinical lectures by Dr. Graves, printed, as we are told, from notes taken by a shorthand writer, have been published in the London Medical Gazette and other periodicals. The first part of the work before us consists of these lectures or a selection from them arranged and corrected as we have stated above; the second part consists of detached essays upon various subjects here collected together from the Dublin Hospital Reports and the Dublin Medical Journal. It may admit of some question whether the author has done wisely in adhering so closely to the form in which his observations were originally given to the public, and it must be admitted that the work would have taken a more philosophical and systematic cast had a different plan been adopted. Still there is something in the, we had almost said desultory, nature of the clinical mode of conveying instruction which tends to bring the subjects of observation strongly before the mind and to impress them on the memory; and we must allow that Dr. Graves has in this manner succeeded in giving a graphic force to the views which he is desirous of illustrating, investing them with an air of reality, and adding much to the conviction of their truth and fidelity.

The first three lectures are introductory, and embrace the consideration of subjects which, however interesting in themselves and profitable perhaps to the student when delivered, might, we think, have been advantageously dispensed with in the circumstances under which the volume before us appears. The main value of the work is derived from its practical tendency, and we feel assured that few of those who consult its pages will be disposed to enter upon the theoretical questions propounded in the lectures referred to. The doctrines laid down in these introductory lectures are moreover such as many will be found to differ from, and although on the present occasion we intend to confine our remarks mainly to the far more important practical questions with which the succeeding portions are occupied, we are compelled in the outset to indicate our dissent from some of the opinions advanced by Dr. Graves, lest by our silence we should be thought to give them an approval which we cannot altogether bestow.

Briefly to allude to one point alone—we are by no means advocates for requiring from the candidate for medical diplomas and degrees a knowledge of the minutiæ of every science which bears in the remotest degree

upon the theory and practice of medicine. We are quite aware that for the practical purposes of attendance upon and relief of the sick, it is especially the accomplished physician, taking the word in its most extended sense, who will be the real agent of good, rather than the chemist, the botanist, the natural philosopher, or even than the anatomist or physiologist; but we are not on that account to undervalue or set aside chemistry, natural history and physics, anatomy and physiology. To enter into this question would carry us far beyond the space which we can here devote to it, and we must leave this contest about the outworks, satisfied with guarding our opinions from misconception, and our silence from being construed into assent.

FEVER. Lectures four to nineteen inclusive are devoted to this subject, and contain an exposition of the author's views upon several important points connected with the practical management of patients suffering under different forms of idiopathic fever. We must, however, in the outset guard the younger readers of Dr. Graves's work against too exclusive an adoption of certain of the special points of practice therein advocated. Fever is a very indefinite term, and there is perhaps no disease, assuming its various types to constitute varieties of one and the same general affection, which presents greater variety of aspect or requires a greater and corresponding variety of treatment. In giving this caution we are merely expressing what Dr. Graves himself would have urged had his lectures been addressed to those circumstanced as are the general community of medical practitioners. The typhoid fever of Dublin and the adjacent neighbourhood seems to be what is now known and described under the term maculated or spotted fever; in which, whatever other organs may be affected, the brain and cerebral system are often severely implicated. To this form of fever the observations of Dr. Graves more particularly apply, and in this form his opinions, we believe, will be found a valuable guide to practice. But we need do no more than appeal to the experience of the numerous intelligent practitioners allocated in the metropolis and other large towns, or scattered throughout the provinces, on this side of the channel, to show that the varieties of fever are as numerous perhaps as the localities in which it prevails. In many extensive districts of this country maculated typhus is unknown; in others it is of comparatively infrequent occurrence. In Liverpool and Glasgow and other large towns of the western side of the island the maculated form will probably be found prevailing, but we much question whether the central, eastern, and southern portions of the kingdom would not show other types of fever, in which the abdominal and pulmonary organs are much more affected than the brain, and the disease for the most part presents neither maculæ on the skin nor follicular disease in the intestinal mucous membrane. With these preliminary remarks, we proceed to draw attention to the treatment proposed and adopted by Dr. Graves in the severer forms of maculated fever.

The main features of his practice seem to be close and careful watching of symptoms, with the view not only of obviating the tendency to fatal or otherwise injurious effects, but also of anticipating and suppressing the outbreak of those local actions which are commonly observed to be the precursors and often also the immediate causes of mischief. Hence we have impressed upon us at the outset the advantage and indeed necessity

of frequent visiting of fever patients by the physician, and of careful watching and nursing by competent assistants and experienced nurses. Directions are given to be on the watch for the slightest irregularity or discrepancy in the mode in which the different functions are performed, and especially those more immediately connected with the brain and nervous system.

It is sufficiently evident from the general tenor of the observations made by Dr. Graves that the chief danger in the fever which he has been accustomed to treat in the Meath hospital and in the city and neighbourhood of Dublin, arises from the affection of the brain. Yet we find him coming to very opposite conclusions respecting the nature of this state to those drawn by Dr. Clutterbuck and his followers:

"I spoke at my last lecture," he says, "of a man named Cassels, who died in the fever-ward with symptoms of cerebral excitement, and stated that I regretted having omitted to leech his head, and prescribe tartar emetic in the form of enema. Since that time we have had an opportunity of examining his body, and the results of the dissection are well worthy your attentive consideration. He was a young man of robust habit and apparently good constitution, and laboured under the ordinary form of maculated typhus. Shortly after his admission he was attacked with delirium, which was soon afterwards followed by coma and death. Now, suppose you were called to see a patient, not labouring under typhus, but exhibiting a similar train of symptoms, that is to say, violent delirium, accompanied by flushing of the face, suffusion of the eyes, headach, and a tendency to get out of bed, in fact, a state of furious excitement, requiring the restraint of a strait-waistcoat,—what idea would you be likely to form of the condition of the brain? If a patient of this kind had no typhoid symptoms, you would certainly say that he was labouring under meningitis or cerebritis; and if the case proved fatal, you would naturally expect to find lesions of the brain fully sufficient to account for all his symptoms. And you would in all probability find extensive thickening of the membranes of the brain, with subarachnoid effusion, or you would discover softening, increased vascularity, and suppuration of the encephalic mass. But here, a man in fever exhibits all the symptoms of cerebral inflammation; the cerebral affection runs on to a fatal termination with great rapidity; he dies comatose. And what do we find on dissection? Doubtful signs of congestion, and no distinct evidence of inflammation; a slight opacity of the arachnoid at the base of the brain, and about a teaspoonful of clear subarachnoid effusion. Now this is a point to which I would earnestly call the attention of every inquiring student. A patient, during the course of typhus, is seized with symptoms which are generally regarded as characteristic of congestion and inflammation of the brain; he dies, to all appearance in consequence of the intensity and violence of these symptoms, and on dissection little or no trace of cerebral disease is found. In the case under consideration the symptoms present were strongly indicative of congestion, if not of inflammation; and had the man been free from typhoid symptoms, you would expect to find decided traces of inflammatory mischief. This seems to prove that in the production of cerebral symptoms in typhus, some cause not to be recognized by the production of cerebral lesions, or in other words something beside mere congestion or inflammation, exists. I have now examined a great number of cases of this description, and the examination has brought home to me a strong conviction, that the delirium of fever depends upon something more than mere inflammation or congestion." (pp. 97-8.)

Now in these views of Dr. Graves we do not hesitate to express our decided concurrence, and we might appeal to the results of the practice founded upon either supposition as well as to the appearances on dissection to establish their correctness. Still in those cases, where the cerebral

system is deeply implicated, the most frequent form of fever, as it seems, in Dublin, though of comparatively rare occurrence in some districts of this country, the danger to be apprehended is mainly from this source, and every precaution must be employed to guard against its occurrence, and every curative measure adopted to obviate its effects.

"What I wish to impress upon you is, that you should always anticipate the cerebral symptoms in fever. Never allow the cerebral symptoms to explode—watch the first scintillæ of cerebral excitement—repress the commencing mischief, and do not permit your patient to be overtaken by formidable inflammation of the brain. Every writer will tell you that when the patient's face is flushed, his eyes suffused, and when he complains of headach and intolerance of light, you should leech and blister his head, give him purgatives, tartar emetic, James's powder, and the medicines calculated to bring down cerebral excitement; but a careful and observant practitioner will anticipate all these symptoms, although there is as yet no particular flushing of the face, headach, or suffusion of the eyes; and though the patient is still quite rational, he will recognize threatening disease of the brain, and take proper steps to prevent its increase. Watch the functions of the brain attentively, and they will inform you in almost every case, of the approach of cerebral symptoms. You will find in patients who are about to have cerebral symptoms, a degree of restless anxiety, and a higher degree of energy than accords with their condition; and they either do not sleep at all, or their sleep is broken by startings and incoherent expressions. When you speak to a person in this state, he answers in a perfectly rational manner; he will tell that he has little or no headach; and were you to be led away by a hasty review of his symptoms, you would be very likely to overlook the state of his brain. If you inquire closely, you will find that he scarcely ever sleeps or even dozes, that he is irritable, excitable, frequently incoherent, and muttering to himself. Under such circumstances, although there is no remarkable heat of scalp, suffusion of the eye, or headach, I am frequently led to suspect the supervention of cerebral symptoms, particularly about the ninth or tenth day of the fever, (for it is generally about this period that cerebral symptoms begin to manifest themselves;) and whenever I observe these premonitory indications, I never hesitate in taking proper measures to anticipate the evil." (p. 86.)

"There is, on the other hand, an opposite state of the patient, which in like manner informs me that danger to the brain is at hand. In this case, the patient is almost continually sleeping. When you enter his chamber in the morning, and ask how he does, his attendant generally tells you that he has passed the night most favorably, and that he has slept without almost ever waking since your visit on the preceding afternoon. If he awakens to take drink, he quickly drops asleep again, and when you arouse him he looks rather heavy; there is some slight suffusion of the tunica adnata, and some appreciable congestion about the external parts of the face and head. Persons in this state, though apparently doing well, and even where they have been properly treated in the beginning, about the ninth or tenth day begin to rave, and exhibit undoubted proofs of congestion and excitement of the brain." (p. 87.)

In either of these states, Dr. Graves advises the immediate application of a blister to the shaven scalp, and considers that by this means he has the whole external surface of the head pouring out serum, or even suppurating, at the period when formidable cerebral affection would otherwise have set in. We believe that in the fever commonly observed in this country a less severe form of the same remedy, or an application which is less repugnant to the prejudices of the friends may suffice, but in this the practitioner must be guided by the character of the prevailing fever, as modified by local or epidemic influences.

A valuable symptom in indicating the existence of insidious disease of the brain in fever is the occurrence of vomiting. Nausea, vomiting, and diarrhœa are, however, equally accompaniments of those forms of fever in which the gastric mucous membrane is implicated. It becomes of importance therefore to be able early to detect the difference between the vomiting which arises from either cause. Dr. Graves expresses the opinion that vomiting and purging setting in at the commencement of fever usually indicate cerebral affection. This view is in accordance with what is observed in injuries of the brain from other causes, and there are few who have been called upon to witness a severe epidemic of scarlet fever, who can have failed to remark that those cases in which vomiting came on early, proved to be of the most serious description, in consequence of the head affection. In the epidemic which has recently been so fatal in some parts of England, there has been frequent occasion to verify the correctness of this observation.

Cerebral vomiting, as it has been already stated, commonly sets in

"At a very early period of the disease, perhaps on the first or second day, and is seldom accompanied by the red and furred tongue, the bitter taste of the mouth, the burning thirst, and the epigastric tenderness, which belong to gastro-enteric inflammation. There is also another source of diagnosis, but of a less valuable kind; and this is founded on the results of treatment. Gastro-enteric vomiting and diarrhœa are relieved by leeching the belly; but I need not tell you that this mode of treatment can have no effect on the vomiting and purging produced by cerebral disease. There is also another means of distinguishing; the vomiting and diarrhœa which results from gastro-enteric inflammation is never accompanied by such copious discharges of bile as that which depends on disease of the brain. In diarrhœa from derangement of the brain, the quantity of bile passed is very remarkable; and it is equally curious, that when vomiting follows derangement of the cerebral circulation in ordinary cases, and without fever, bile is thrown up in very large quantities. This is frequently observed in persons who become sick from swinging or sailing. In such instances, a larger quantity of bile is vomited than could occur from mere gastric irritation. Now in the commencement of cerebral disease, where congestion or inflammation is present, one of the first symptoms is copious vomiting, and purging of a bilious character." (pp. 94-5.)

There is room, however, for caution against coming to too hasty a decision where the discharges are of a bilious character, since many cases of what are termed bilious fever and bilious indigestion are characterized by early vomiting, and copious flow of bile; and where the vomiting in fever is of a bilious character, there will be a bitter taste in the mouth, and the tongue will be found to be furred pretty much the same as when the symptoms in question are dependent upon gastro-enteric disease. In short, cerebral irritation of whatever description, very generally gives rise to vomiting and purging, which may or may not implicate the secretions of the liver. A similar effect also occurs from gastric irritation, and the chief points of distinction between them are the presence of pain at the epigastrium, and the peculiar red furred tongue which attend the latter.

Dr. Graves points out a peculiar state of the respiration connected with cerebral excitement in fever which when present affords perhaps a symptom of less ambiguous character.

"Now there is one symptom connected with cerebral excitement in fever, which is well worthy of your notice, as its existence is often sufficient of itself

to give timely intimation of the approach of irritation or inflammation of the brain. This is the state of the respiratory function. In fever, the breathing will often announce the approach of cerebral symptoms for days before their actual occurrence. When, in cases of typhus, you find the patient's breathing permanently irregular, and interrupted by frequent sighing, when it goes on for one or two minutes at one rate, and then for a quarter or half a minute at another rate, you may rely upon it that sooner or later, an affection of the brain will make its appearance. You will frequently observe the same kind of breathing preceding attacks of apoplexy and paralysis, and indeed it was the occurrence of this symptom, in these and other cases in which the functions of the brain were deranged, that first drew my attention to this kind of breathing. The first time it engaged my attention was in a remarkable case of an apoplectic nature, which I sat up a whole night to watch. On recollection, I found that I had frequently observed an analogous state of the respiratory function in fever, on several occasions, although its connexion with excitement of the brain had not struck me before. I speak here of irregularity of breathing, independent of any pectoral affection. But when the patient breathes in a permanently irregular manner, at one time at a certain rate, and at another at a different rate, when his respiration is suspicious [suspirious?] and heaving without any disease of the chest or great debility, you will have some grounds to suspect the existence of cerebral derangement. I am in the habit of calling this kind of breathing *cerebral respiration*, because my experience has told me that it is almost invariably connected with oppression and congestion of the brain. To recapitulate. When you find a patient in fever lying constantly awake, or when, on the contrary, you find him continually slumbering, when there is a certain quickness of manner and irritability, and when the cerebral respiration has been noticed for some time, without any concurrent debility or pulmonary disease, under such circumstances, you may, in cases of maculated typhus, predict the approach of cerebral symptoms; and the period about which they generally manifest themselves, is the eighth, ninth, or tenth day." (pp. 87-8.)

We have before urged the importance of anticipating the outbreak of the cerebral affection, and given at some length the directions which Dr. Graves lays down for effecting this purpose. It now remains to notice the remedial measures which he recommends when cerebral excitement is actually developed and threatens the safety of the patient. Tartarized antimony has been frequently employed in fever and in some epidemics with excellent effect. In others, it has on the contrary seemed to prove not only inefficacious but decidedly prejudicial. In these cases however, the remedy has been usually administered in a different stage of the fever and with different views. It is Dr. Graves's peculiar merit that he has exhibited the tartar emetic with success under circumstances of almost hopeless nature, and that he has been led to the practice by what we believe to be a correct view of the pathological state under which patients so circumstanced suffer. It appears sufficiently evident that the same or similar symptoms may arise in very opposite states, and that the resemblance is often so great, that careless, superficial, or biassed observation may readily lead into error. Who is not aware, that giddiness even to falling, is not unfrequently owing as much to a state of debility as to one of plethora, requiring also a diametrically opposite mode of treatment for its relief? The writings of the late Dr. Gooch afford evidence of a similar character in regard to other affections. Dr. Graves extends this principle to fever, and shows that delirium, sleeplessness, and other symptoms usually considered to be indicative of vascular disturbance in the brain, are not necessarily dependent upon inflammation,

and require for their management a corresponding mode of treatment.

The author's claims to originality in proposing the peculiar treatment by which he endeavours to meet this condition have, it seems, been disputed. However otherwise he may feel himself aggrieved, Dr. Graves has some cause for satisfaction with the testimony thus afforded to the usefulness of the treatment proposed. Where a discovery of whatever nature is of no value, few are disposed to say much about it, and the reputation for that which is of doubtful or injurious character is generally left in the indisputed possession of those to whom it may belong. Our present object, however, is to endeavour to appreciate the merits of the practice followed by Dr. Graves, not to assign to him the precise degree of originality which he is entitled to claim, for proposing and carrying it into effect.—Now, when we meet with a fever patient who on the nineteenth day of the fever has had no sleep for many days and nights, and is found to be in a state of mental incoherence, raving, tremor, and subsultus, with a failing pulse, we must allow that there is cause for the most serious alarm. Let us see what Dr. Graves proposes under such circumstances:

"I remember well the time," he observes, "when a patient so situated would have been again purged, his head would have been shaved, a few leeches applied to the temples, and a blister to the nape of the neck, while perhaps wine and musk would have been exhibited internally. How many persons have I seen so treated by the most eminent physicians, and how unsuccessful was the practice! To have talked of giving opium under such circumstances, and when the marks of cerebral congestion were so evident, would have been regarded as absurd; my experience on former occasions, however, determined me to give opium, and, as the danger was imminent, I gave it boldly. To the eight-ounce mixture, with four grains of tartar emetic, we added one drachm and a half of laudanum; of this he took one ounce every second hour, from eight in the evening until he had taken five doses. This produced copious sweating; the skin became cooler, he raved less, but still no sleep; at four on the following morning, his pulse became 70, and respirations tranquil; he got twenty drops of Battley, and at half-past five in the morning, twenty-five drops more. He had now taken within a short time, about one drachm of laudanum, and forty-five drops of Battley, combined with nearly three grains of tartar emetic. He was tranquil, but did not close his eyes, and muttered occasionally; subsultus less. His pupils now became more and more contracted, his eyes less expressive and duller, and when I came at eight in the morning, he was evidently deeply narcotised, although not yet asleep. I thought that all was lost; but still, observing the respiration to be tranquil, and the pulse regular, I indulged a faint hope that sleep might still supervene. His eyes now became still more inexpressive, the lids gradually closed, his breathing became prolonged and deep, and at half-past eight he was buried in a profound and tranquil sleep, which continued for nine hours, when he awoke, spoke rationally, said he had no pain in the head, took some drink, and fell asleep again. Next morning not a single symptom of fever remained." (pp. 150-1.)

The condition to which several of the fever patients treated by Dr. Graves were reduced seems to be analogous in many respects to that which occurs in delirium tremens. It is here that the combination of tartar emetic and opium is so strenuously recommended, and seems to have proved in the cases related so beneficial in its effects. In the earlier stage of fever, when inflammatory or congestive symptoms make their appearance at the outset of the attack, Dr. Graves is disposed to

meet them with the lancet, leeches, purgatives, cold applications, and tartar emetic.

"Here," he observes, "the lancet and tartar emetic are our best opiates, our best restoratives of tranquillity and sleep. As the fever progresses, and when we have arrived at a more advanced stage of the disease, when maculæ makes its [make their] appearance on the skin, and symptoms of general debility announcing the typhoid type begin to predominate, then we must proceed with more caution, even though our patient is totally deprived of sleep and is violently delerious. The lancet cannot now be resorted to; leeches, indeed, may be applied, but their effects must be carefully watched, as the patient will not bear copious depletion of any sort; tartar emetic may, nevertheless, still be given boldly, and will be found to answer our expectations. But if we have to contend with want of sleep and delirium at a still more advanced period of fever, we now often recognize that very combination of symptoms, the union of general debility, and cerebral congestion, which in certain varieties of delirium tremens we have seen so successfully treated with tartar emetic and opium; who will refuse to acknowledge the similarity between these cases of fever, delirium, and many varieties of delirium tremens? Are there not in both, the same tremor and subsultus of the extremities; the same trembling of the tongue when the patient endeavours to put it out; the same starting and sleeplessness; the same rambling, delirium, or incoherence, combined nevertheless with the power of answering rationally when spoken to; the same character of the mental wandering, for in both they are extremely apt to rave as if employed in their ordinary occupations, and as if surrounded with their usual associates; in short, can any greater resemblance exist between two diseases arising from the operation of remote causes so different? We need not, therefore, be surprised, at finding the same treatment applicable to both." (pp. 155-6.)

Tartarized antimony, as we have before observed, has been frequently employed in fever cases; the peculiarity of the treatment pursued by Dr. Graves, arises from the manner in which this remedy is given. In the earlier stages the doses are larger than those commonly employed by other practitioners, and are repeated at shorter intervals; in the later stages the antimonial is given in combination with opium, as we have just shown. We have one observation to make upon this method of treating fever cases. It may admit of question whether the later symptoms simulating those of a congestive or inflammatory condition of the brain are not occasionally owing to the early treatment of the attack being conducted too much on the antiphlogistic plan? May not the *nimia diligentia medici* in the use of antimonials in the early stage, sometimes bear its part in giving rise to the apparent cerebral excitement developed in the future progress of the disease? It is well known that the active employment of depletion and antimonials in cases of disease purely inflammatory or in inflammation as it occurs in certain conditions of the system, will often give rise to an irritative action of the vascular and nervous systems and determination to the diseased part, apparently calling for the continuance or repetition of the same measures, but to be met only by others of a very different character. There seems no sufficient reason why something of this kind may not also arise in severe fever; but however this may be, we are ourselves disposed to be sparing in the employment of antimonials wherever fever assumes or threatens to assume the typhoid type. We do not make these observations in disparagement of the treatment pursued by Dr. Graves. The fever which he has been accustomed to treat may, and probably does require a different method of management to

that required in the fever we are accustomed to witness. There may possibly be more tendency to an inflammatory type at its commencement, and the local complications may at this period be of a more active and threatening character. To meet the almost desperate circumstances of the latter stage which have been above detailed, the use of tartar emetic and opium in the manner recommended should be had recourse to wherever they occur, as we know of no means more likely to prove of service in such cases, and certainly of none which come to us so strongly recommended.

These observations on fever have extended to a greater length than we had intended, but before concluding them we must briefly notice one or two other points of some practical importance.

No one can question the utility of cold lotions when the head is much affected; but, as Dr. Graves elsewhere observes, there is a vast difference between a thing being done and its being well done. In the very imperfect manner in which cold lotions are generally employed, the effect is, we firmly believe, worse than useless, the congestive state of the brain being often augmented instead of relieved by them. Strongly impressed with the mischiefs resulting from the partial employment of cold applications to the head, Dr. Graves recommends lotions of warm vinegar and water instead, and we can ourselves testify to the great relief and advantage which may be thus obtained. In cases where it is desired to obtain the counter-irritation of a blister without its debilitating effects, a circumstance which often arises in the latter stage of fever, and in cases where there is much debility, Dr. Graves recommends that the blister should be kept on from four to six hours only, neither removing the cuticle nor letting out the effused serum when the blister is removed, but dressing it at once with spermaceti ointment. This method of blistering is especially applicable when the chest is affected, either in the advanced stage of fever, or in some forms of pulmonary disease, and especially among children. The pain and local irritation is much less, there is little or no irritative fever, and less risk of affecting the urinary organs, while in children the danger of inducing troublesome sloughing ulceration is avoided. The practice is not new, but deserves more attention in general practice than it now receives.

The employment of wine in fever requires much consideration, both as to the time when, and the quantity in which it is advisable to give it. Dr. Stokes has recently attempted to derive indications for its use from the characters of the cardiac sounds and impulse. Without entering upon a criticism of the theoretical views advanced by Dr. Stokes, we may briefly state that Dr. Graves, admitting the fact that the heart in many cases of typhus becomes debilitated, and that this debility is manifested by a corresponding change in the nature of the sounds and impulse afforded by the organ, fully agrees with Dr. Stokes, "*that in the diminished impulse, and in the feebleness or extinction of the first sound, we have a new, direct, and important indication for the use of wine in typhus fever.*"

In one of the lectures devoted to the subject of fever, the eighteenth, we have an account of an epidemic occurring in Dublin in the year 1826, attended with jaundice, which Dr. Graves terms yellow fever. A comparison is instituted between this epidemic and the yellow fever of

Gibraltar and the south of Spain of the year 1828. There appears to have been some essential differences between the two, and the propriety of applying the term yellow fever to the Dublin epidemic or rather to those cases of it in which jaundice formed one of the complications is very questionable.

Scarlet fever. This is not the subject which follows next in the order of succession in the work before us, but as it may be more conveniently considered in connexion with the preceding observations on general fever, and as the method of arrangement followed by the author, if indeed any methodical arrangement at all have been adopted by him, is not easily made out, we shall not hesitate to transpose the subjects as may seem most fitting or most convenient for our purposes. The thirty-fourth lecture, in which the account of scarlet fever finds its location, was delivered during the session of 1834-5, and the observations chiefly refer, we presume, to the scarlet fever then or for some years before prevailing. To these however are appended letters from various practitioners, dated 1842; a wider range must therefore be allowed, embracing other epidemics of scarlet fever in addition to those to which they are more immediately applicable. We are at some loss to characterize the features peculiar, if any such there be, to each epidemic, and we do not always find a clue to assist us in so doing, a somewhat singular omission, after the remarks with which Dr. Graves has prefaced his account. The scarlet fever of the year 1834 assumed a much more serious and fatal character than what had been before observed during the present century, and Dr. Graves describes three forms under which the more severe cases presented themselves. In the first of these there was, with the usual fever and sore throat, violent congestion of the cerebral organs at the outset, with convulsions and apoplectic coma on the first or second day, attended in the worst cases with general and intense cutaneous efflorescence. In this form when the tendency to the head was early and strongly manifested, the patient was seldom saved, though sometimes very active depletive measures seemed to relieve the brain and the result was then favorable. Cases of this form we remember to have observed on this side the channel in the summer of 1832, during the prevalence of the epidemic cholera, and in more than one instance a fatal termination was produced by the first shock of the attack, within a few hours of the sinking under the disease. In one case, occurring in a fine young woman, the whole surface of the body after death was of an intense purple red; in another, that of a child, the attack was almost immediately fatal, some very imperfect traces only of eruption having transiently appeared.

In a second form of the disease noticed by Dr. Graves there were the usual symptoms of severe eruptive fever, attended from the onset with severe headach and spinal pains and great irritability of the stomach and bowels; nausea, vomiting, and bowel complaint being among the very first symptoms. Recently-secreted bile was thrown up in large quantities, and abundance of bilious and mucous discharges with some fecal matter passed from the bowels. These discharges from the stomach and intestinal canal afforded not the slightest relief either to the fever or the headach, nor did they seem to interfere with the formation and progress of the eruption; and during their continuance there was not the slightest

tenderness of the epigastrium nor of any part of the abdomen, the belly, on the contrary, becoming fuller and soft. It is impossible not to recognize in this irritable state of the stomach and abdominal viscera the characteristics of cerebral irritation and congestion before pointed out under the head of Fever. This form was also attended with much disturbance of the circulatory system, the pulse being rapid and the temperature of the surface elevated. Depletion was ill borne in cases of this description, and Dr. Graves draws the inference that

"The derangement of the brain and nerves depended on something more than the violence of the circulation, and originated in something altogether different from mere cerebral inflammation or congestion. What that something was," he continues, "I cannot even conjecture; but it was probably the result of an *intense poisoning of the system by the animal miasma of the scarlet fever.* Every tissue of the body seemed, if I may use the expression, equally sick, equally overwhelmed, and it is probable that the capillary circulation in every organ was simultaneously deranged. It was not gangrene of the throat which proved fatal, *for in this form it never occurred;* it was not inflammation of any internal viscus, for such was not found on post-mortem examination of the fatal cases; but it was a general disease of every part." (p. 506.)

Cases very similar to the above we have had occasion to witness during the past year, and in one of these the symptoms were almost such as might, under other circumstances, have led to the suspicion of poisoning.

The remarks of Dr. Graves on another state of things occurring in many patients, which he says required to be carefully distinguished from that just described, and where the disease was evidently attended with an inflammatory state of the constitution demanding energetic measures, but ill agree with the views respecting the epidemic constitution with which he commences the subject under consideration. In such cases the early symptoms were severe and the throat very sore, but the eruption was not quite so perfect in its appearance nor was the pulse so rapid. Bleeding and leeching were in such instances borne well and followed by almost immediate relief.

The third form described by Dr. Graves was ushered in by the usual fever together with sore throat and slight headach, and followed by a regular and moderate amount of eruption; but when all danger seemed to be over and convalescence apparently commencing, about the eighth or ninth day, slight restlessness and morning fever were observed, which were followed by an irritable appearance of the nostrils, return of sore throat, heat of skin, sudden and great prostration of strength, and painful swellings in the region of the parotid and submaxillary glands. These swellings rapidly increased and were attended with ill-conditioned viscid fetid secretions from the nostrils, mouth, and throat, the mucous lining of these parts being inflamed and tumefied. Under such circumstances the fever rapidly increased and assumed the typhoid character, the fatal termination following after much suffering. It is remarkable that in not any one of the three forms of the disease here noticed, formidable as they were, does the sore throat appear to have been of primary or essential importance.

A good illustration is afforded by scarlet fever of a general proposition or law laid down by Dr. Graves, "that in both acute and chronic diseases *a constitutional affection may display its existence by only one or*

two of the numerous symptoms which usually accompany it." "This occurrence," he observes, "seems more frequent in the case of diseases produced by contagion and morbid animal or vegetable poisons, than in the case of maladies generated by causes developed in the system itself." (p. 538.) The "scarlatina simplex, nulla comitante cynanche" of Cullen, described also by Sydenham, and the "rubeola sine catarrho" of Bateman, are examples familiar to all, while the sore throat so frequently observed to prevail during epidemics of scarlet fever is probably another illustration of the same principle. It will be recollected that the desquamation of the cuticle occurring at the close of scarlet fever has been attributed to the effects of the inflammatory process going on or supposed to be going on in the skin during the appearance of the efflorescence, but Dr. Graves shows that the desquamation occasionally takes place where there has been no eruption, and he also mentions a case in which a young lady, who after a constant attendance on her sisters during their illness from scarlet fever, but who had herself apparently resisted the infection, was subsequently attacked with the peculiar anasarca so well known as one of the sequelæ of scarlatina.

Some remarks are made on the occurrence of albuminous urine in the dropsy connected with scarlatina, which is considered by Dr. Graves, in accordance with the opinion expressed by Dr. Blackall, as indicating merely a peculiar inflammatory condition of the whole system unconnected with degeneration of the kidneys. In support of this view he gives the particulars of a case in which, after death, the kidneys were found to be perfectly healthy, although the urine had been highly albuminous throughout and the dropsical swelling extreme. Dr. Graves therefore considers this state of urine and the morbid condition of the kidney with which it is so commonly connected to arise from different causes often co-existing in chronic dropsy but not necessarily dependent the one upon the other. He consequently regards albuminous urine as a sign of Bright's kidney, but not as its result. In the anasarca following scarlatina Dr. Graves highly recommends the employment of the hydriodate of potash in addition to the usual means, a remedy the good effects of which in dropsical swellings, we may add, are not confined to the forms here spoken of.

INFLUENZA. We do not perceive that Dr. Graves has added much to our knowledge of the causes of those epidemic visitations of catarrhal fever, so many of which have occurred within the last twelve or fourteen years. The exclusion of vicissitudes of temperature, peculiarities of situation, supposed moist or dry states of the atmosphere, the prevalence of certain winds, with many more presumed sources of epidemic disease, form the chief agency in the generation of influenza, is no more than what has before been recognized by others, and after all affords but a negative characteristic of its etiology. To say that "it is probable that influenza may depend chiefly on telluric influence, upon some agency connected with variations in the physical conditions which operate on the external surface of our planet," is only another mode of expressing our entire ignorance, which it would be better at once to acknowledge in so many words and without attempting to involve it in the language of useless and groundless conjecture. But whatever may be the cause to which influenza owes its origin, there can be no doubt that from the

severity and fatality of some of its recent visitations as well as from their wide spread and almost universal prevalence, the best mode of management of those suffering under this affection becomes a subject for serious consideration.

Notwithstanding the expectations formed by some practitioners from depletive measures, which expectations, by the way, seem to have been more sanguine amongst our countrymen on the opposite side of the channel than they were on this, there is little doubt that the general impression now is that bleeding, leeching, &c. in the greater number of cases, and unfortunately also in the more severe ones, were too often worse than useless. Dr. Graves, although he thinks that in some instances venesection was of great service even when the pulse was natural, expressly states that in others it could not be borne to the smallest amount when the pulse was hard and wiry, and remarks that he never saw any disease in which the pulse formed so bad a guide as to the propriety of venesection as the present epidemic, (that of 1837.) Upon the whole he seems to consider the disturbance of the circulation in influenza to be connected with irritation of the nervous system rather than with any inflammatory state of the constitution in general, an opinion which agrees we suspect with that of most well-informed and reflecting physicians. He subsequently observes that bleeding, unless employed within the first twelve or twenty-four hours, is likely to do as much or more harm than good; and that on the second or third day, except to relieve congestion of the lungs, it seems inadmissible. Leeching over the hollow of the neck just above the sternum, aperients, confinement to bed, and sudorifics, are the remedies which he chiefly recommends when the first twenty-four hours have passed over, adding opiates in combination with nitre and tartarized antimony after the antiphlogistic remedies have been persevered in for a day or two. Blisters, according to the experience of Dr. Graves, afforded no relief either to the dyspnœa or to the pulmonary symptoms in general. Fomenting the trachea and chest with hot water seemed to be more serviceable, a practice which is also applicable to many other diseases where the air-passages are affected.

There is no novelty, it will be observed, in this mode of treatment, and little difference from that generally followed in this country, and we have been induced to give the foregoing brief summary of the practice of Dr. Graves in this affection, only because in diseases of such extensive prevalence as influenza it is desirable to be acquainted with the methods of treatment adopted by those practitioners whose means of observation and sound practical judgment render their opinions upon almost any subject worthy of notice.

Bronchitis. The miscellaneous nature of the contents of the work under notice must necessarily give the same character to the remarks which we feel called upon to make. Having therefore brought the subjects of scarlet-fever and influenza into juxta-position with general fever to which they are nearly allied, we now retrace our steps and take up the consideration of bronchial inflammation, which in the "system" follows immediately after fever. Dr. Graves is of opinion that catarrhal disease, taking its rise from exposure to cold, may be equally occasioned by direct impression of the inspired air on the bronchial membrane as by the secondary effects of a chill on the surface, and though there are

evident provisions calculated to act as a check to the mischiefs which might result from the respiration of air greatly reduced in temperature, we cannot doubt that bronchitis does occasionally arise in this way. The stethoscopic signs of bronchial inflammation are dismissed in few words, Dr. Graves contenting himself with pointing out the difference between those of the cases in which the larger bronchi alone are concerned, and those where the minute tubes are implicated. In the former the sounds under the stethoscope are few in number, of a deeper tone when dry, or when moist the bubbles large, unequal, and scattered; in the latter many sounds may be perceived together in a limited space, which are small, wheezing, dry and sharp, or very minute and evidently moist. We agree with Dr. Graves and Dr. Latham that the burdening of the memory of the young auscultator with names for so many different varieties of râle tends only to produce confusion in his mind, and it may be added that if every modification of sound appreciable to the practised ear is to receive its appropriate appellation and definition, the terms hitherto recognized are altogether inadequate, and might be multiplied almost indefinitely.

To confine our remarks to bronchial inflammation, the main object is to ascertain its locality and extent, whether the larger or smaller tubes are affected, and whether the secretion is greatly or at all increased. Among the cases which Dr. Graves selects to illustrate by his observations, is an instance of chronic bronchitis with an exacerbation from a fresh attack of cold. This is summed up as follows: "Extensive bronchial inflammation with copious expectoration unaccompained by fever, and occurring in a debilitated constitution." The only stethoscope phenomena were extensive minute and moist bronchial râles. In such a case depletion, either by lancet or leeches, is out of the question, for the temporary relief afforded by its employment is rapidly followed by an increase of the already abundant secretion and consequent severity of the symptoms. Dr. Graves especially recommends a combination of nitre, tartar emetic, and camphorated tincture of opium in some mucilaginous or demulcent fluid, with an active rubefacient to the chest. Where bleeding and leeches are contra-indicated, Dr. Graves gives the preference to the combination of nitrate of potash and tartarized antimony over all other remedial agents. In the subsequent steps of the treatment, though occasion arises for many judicious remarks adapted to a class of intelligent pupils, there is nothing which is not already well known to the profession. After relief has been obtained, pectoral mixtures of more stimulant character are recommended. Alkalies are praised as productive of good effects in cases of pulmonary irritation generally, an observation which we have often had occasion to verify; and when the cough becomes entirely chronic, the *mistura ferri comp.*, in smaller doses however than is usually given, combined with tincture of henbane, is much recommended. Dr. Graves remarks that excessive secretion from the lungs may often be diminished by a powerful hydragogue cathartic, and when the patient is sufficiently strong to bear it, such practice may occasionally be adopted with striking benefit. "This, however," he adds, "will require both judgment and discretion, and it should be borne in mind, that, in the majority of cases, there are many circumstances which contraindicate their employment." We will add the further caution that although a cathartic will often relieve chronic cough attended with copious secre-

tion from the bronchial membranes, considerable discretion should always be used in the employment of purgative medicines after inflammatory affections of the lungs, as we have had occasions to witness more than once a relapse induced, unless we are greatly mistaken, by the operation of the cathartic.

In chronic bronchitis unaccompanied by fever, remarkable dyspnœa, or acceleration of the pulse, and when the secretion is copious, Dr. Graves has often experienced good effects from emetics; in other cases he recommends the use of sulphur, which he was led to employ from observing the benefit derived in cases of chronic cough and congested bronchial membrane from sulphuretted mineral waters. We have witnessed the good effect of sulphur in chronic catarrh in a case in which the remedy was used for a different purpose; and it seems to deserve a more extensive trial of its power than it has yet met with. Dr. Graves combines cream of tartar with the sulphur, partly with the view of tempering its stimulant properties, and partly to assist in determining to the kidneys in accordance with the axiom of Baglivi: "In morbis pectoris ad vias urinæ ducendum est."

Among the effects of bronchial irritation alluded to by Dr. Graves, there is related a case of asthma which gives rise to some excellent remarks on the use of counter-irritant remedies in that disease. These are recommended to be applied not merely over the chest, but to the nape and along the sides of the neck over the epigastrium, and in the course of the cervico-spinal and pneumo-gastric nerves generally. Various stimulating liniments and like applications have been at different times proposed and lauded for their effects in bronchial and other pulmonary affections, such as the celebrated liniment of St. John Long, Dr. Granville's ammoniated liniment, and others. The first of these has been analysed by Dr. Apjohn, and found to consist of acetic acid, spirits of turpentine, and two animal matters, the one containing azote, the other not. Dr. Graves is inclined to think that the spirit of turpentine exercises something more than a mere counter-irritant action, and proposes the following formula as an imitation:

Strong acetic acid, ℨss.
Spirit of turpentine, ℨiij.
Rose water, ℥iss.
Essential oil of lemon a few drops.
Yelk of egg, sufficient to suspend the turpentine.

There are some observations on cough and the various causes giving rise to it which are well worthy of attention, and may find their place here. Pulmonary irritation from diseased states of the lungs is of course the most obvious of these causes, but the specific source of the irritation is often involved in obscurity. Dr. Graves gives a remarkable instance in which the cough depended upon tape-worms, the patient having been cured by the prescription of an old woman (castor oil and turpentine given for an attack of colic,) after he himself and another physician, treating the case upon erroneous views of its nature, had exhausted their ingenuity in vain. Hysteria, a syphilitic taint, gout and rheumatism, scorbutus and scrofula may thus all give rise to irritation of the pulmonary organs and excite cough, which in each instance, will require a plan of treatment varied according to the peculiar source from which it

derives its origin. We remember some years ago to have observed that very many of the cases of rheumatism occurring during a particular season were closely connected with bronchial disease, the usual disposition of the heart to suffer having been apparently transferred to the pulmonary mucous membrane. The ascertaining of these constitutional complications, or the specific character of the local disorder derived from them, is a point of the highest practical importance, since the very same remedial measures may prove curative, inert, or even fatally injurious, according to the source from which the symptoms take their rise. Thus in the case referred to in which the cough was dependent upon a sympathetic pulmonary irritation, excited by the presence of tape-worm, bleeding, leeches, blisters, and tartar emetic were employed without other effect than the injurious one of debilitating the patient. Antispasmodics and narcotics proved, to say the least, inert, while the symptom was at once removed together with its cause by the empirical dose administered for an attack of colic. Again, depletion would prove injurious in pulmonary irritation connected with hysteria, mercury in that arising from scrofula, while where there is a syphilitic taint this latter remedy has been observed to produce the most beneficial effects under circumstances of very threatening character.

Laryngitis. The next lecture is devoted to a case of pleuro-pneumonia proving fatal with an acute laryngeal attack and gangrene of the lungs, and to some remarks on pleuritis and pneumonia, and especially with reference to the occasional absence of expectoration in the latter disease from its commencement to the period of complete resolution. Dr. Graves makes the following observation on laryngitis, which, as assisting in the formation of a correct prognosis, is perhaps worthy of being recorded : "*In the aged it is accompanied by considerable fever, and, what you would suppose likely to give relief, copious expectoration, evidently derived from the larynx itself; and yet I do not recollect that I have seen an attack of this kind that did not terminate fatally.*" (p. 252.) Another form of laryngitis which pretty uniformly proved fatal, that which occurs in scrofulous constitutions, is alluded to in a subsequent lecture. This form, though commonly more or less of a chronic character, when it is termed laryngeal phthisis, is occasionally observed to run its course with great rapidity. Laryngitis, therefore, arising in a scrofulous constitution, or in a constitution debilitated by any cause, whether it assumes the acute or chronic form, should always be most carefully watched. In the chronic form, and especially when there is no evidence of scrofulous deposition in the lungs, Dr. Graves recommends the application of leeches in small numbers every second or third night, if there is much tenderness on pressure or motion. If there is no tenderness of consequence he advises nitrate of silver or sulphate of copper in solution in the proportion of ten grains to the ounce, to be applied by means of a sponge fastened to the end of a quill to the excoriated and inflamed part of the fauces. The object here is to change the action of the mucous membrane of the pharynx, fauces, and entrance of the larynx, and subsequently, by extension to the neighbouring parts, of the whole inner surface of the larynx itself. With these measures, counter-irritation and the other remedial agents usually employed may be had recourse to according to the varying circumstances of the case.

HEMOPTYSIS, PULMONARY APOPLEXY. As there are two sets of vessels from which the blood sent to the lungs is derived, the bronchial and the pulmonary, so also are there two sources from whence hemoptysis may derive its origin. Now, as is observed by Dr. Graves, the bronchial artery destined to supply that blood which is necessary for the nutrition of the pulmonary texture is small, and its blood arterial or red, while the pulmonary artery on the contrary is large and transmits the whole of the dark venous blood of the system from the right cavities of the heart through the lungs. It is sufficiently obvious therefore that the danger to the patient, and consequently the importance of the symptom, will be materially modified by the source from which the blood expectorated or poured out into the pulmonary structure is derived. Dr. Graves, from considerations arising out of the minute anatomy of the lungs, which it would be impossible to enter upon here, endeavours to show, "that when hemoptysis, from the engorgement of the system of the pulmonary artery, takes place, it is in consequence of the direct effusion of blood from the branches of the pulmonary artery, which ramify on the air-cells, and that the blood expectorated on such occasions has nothing to do with the bronchial mucous membrane, or bronchial arteries," (p. 261;) and further that the blood may escape also into the inter-vesicular pulmonary tissue, where, having no exit like the portion which is thrown out into the air-cells, it must remain. The union of these two forms of effusion from the minute branches of the pulmonary artery constitutes the disease termed pulmonary apoplexy; but it is important to be aware that the affection characterized by all its essential symptoms may occur without hemoptysis, an instance of which is quoted by Dr. Graves from the Dublin Medical Transactions. Are there any sufficient characteristics by which it can be ascertained whether the blood in expectoration is derived from this formidable source, or whether it comes from the bronchial artery? Let us see what Dr. Graves himself says upon this point:

"In the first place the blood (from the pulmonary artery) is black, as you can perceive when it is spit up. It is also clear, that if this blood be detained for some time in the air-cells and bronchial tubes, it will become coagulated, and be spit up in clots. Many of the worst cases of spitting of blood are attended with this symptom; and it is not [?] a mistake to suppose, as you see it mentioned in books, that blood expectorated from the lungs should be florid and frothy. You are told, gravely, that you can distinguish blood discharged from the stomach from that which is discharged from the lungs, by the difference of its colour, consistence, and the presence or absence of air-bubbles. No, gentlemen, you cannot. If you see blood spit up which is dark and coagulated, and, from stethoscopic examination, have reason to think that it comes from the lungs, you will be convinced that the effusion is from the pulmonary artery. I do not mean to say, that when blood comes from the pulmonary artery it is always black and clotted; but I assert that it is so in a great majority of cases; and in many cases of pneumonia we find the sputa partake more of the venous than the arterial character, a circumstance which indicates its formidable source. It is obvious that the blood spit up in those cases may also have a florid tinge, where it has been imperfectly aerated by the imperfect action of air bubbling through it before it is expectorated. There are some hemorrhages, also, from the bronchial artery, which are very copious; but, generally speaking, where there is much cough, constriction of the chest and fever, it is the bronchial mucous surface which is affected; and the spitting of blood which, in such cases, comes from the bronchial arteries, is but scanty, and is seldom dangerous. The blood

will be found to be effused from small spots, as in epistaxis, and the quantity is generally small. You will, however, sometimes find an instance of a person spitting up, very copiously, blood of an arterial colour; for it must be borne in mind, that a very small surface of mucous membrane may often bleed most copiously, as is seen in some cases of epistaxis, when the blood issues from an insulated and small spot. Such cases of copious bronchial hemorrhage occur occasionally, are unconnected with bronchitis, and depend on some peculiar hemorrhagic tendency." (p. 263.)

The chief source of the immediate danger in cases of congestion of the pulmonary artery leading to hemoptysis, is not then from the amount of blood lost to the system, but from the plugging up and obliterating of the ultimate divisions of the air-tubes by the blood effused into and around them. This is important to be borne in mind as a guide to practice, since no hesitation need be experienced, at least in the outset of the attack, in attempting the relief of the congested state and the consequent arrest of the further progress of effusion by copious depletion. The effusion from the branches of the bronchial artery, on the other hand, is seldom extensive, and is derived, Dr. Graves thinks, in phthisical cases in which it is of such frequent occurrence, rather from the congested state of the bronchial membrane so commonly connected with that affection, than from ulcerative action implicating the vascular tissue. We regret that we cannot follow the author throughout the many important questions which are here brought before us, but we must rest contented with recommending his views to the close attention of our readers and referring them for comparative illustration to the work of Reissessen, and the paper recently published by Mr. Addison in the Transactions of the Royal Society on the minute structure of the lungs, to the article Hemoptysis in the Cyclop. of Pract. Med. by Dr. Law, and Dr. Townshend's Essay on Pulmonary Apoplexy. We have already stated that in cases of pulmonary apoplexy no hesitation should be experienced in the employment of free depletion; after this has been practised, the remedy in which Dr. Graves reposes most confidence is ipecacuanha given in doses of "two grains every quarter of an hour, until there is some improvement, and then every half hour or hour, until the bleeding stops." We are indebted to Richter for first making known the powers of ipecacuanha in restraining hemorrhage, the effects of the remedy being not confined to hemoptysis. Dr. Graves recommends that its employment be preceded by a purgative enema, and a saline cathartic, and states that it may be given with advantage in hemorrhage from the bowels, and even in hematemesis, he preferring it to the acetate of lead, which he employs only in those cases of passive hemorrhage in which opium is indicated, and then in combination with the last remedy.

Phthisis. In the following passage the author explains his views on the pathology of tubercle:

"I look on tubercular development and consumption as the consequences of that particular state of constitution, which occasions what is falsely termed tubercular inflammation, a state of constitution in which we have three distinct processes, attended by corresponding morbid changes, each different in itself, but depending on one common cause. Every form of consumption, which has hitherto come under our notice, is referrible to one common origin, and this is that debilitated state of constitution which has been termed the scrofulous habit. One of the first tendencies of this habit is to the formation of tissues of an in-

ferior degree of animalization, among which I class tubercles, whether occurring in the lungs, brain, or liver, whether they exist in a minute or granular form, or in large, soft, and yellow masses, or in the state of tubercular infiltration. I look on them as one of the first of these morbid changes dependent on a peculiar constitution of body, and most commonly found to accompany it. The weaker the constitution is, the greater tendency is there to generate tissues of a lower degree of vitality, and, on this principle, I think we can explain the occurrence of entozoa and hydatids. There are some cases in which you will never be able to prevent the generation of intestinal worms, until you direct your attention to the source of the evil, which lies in the weakness of the constitution, for, in such a state of the system all animals are liable to the formation of parasitic productions and tissues imperfectly animalized. I look on tubercles in this light, and not as the consequence of inflammation, nor do I consider that it has been proved that tubercular development is the cause of phthisis." (pp. 279.)

However this may be, and we are not among those who feel disposed to dispute much of what is now advanced, there can be no question that the presence of tubercles in the lungs, forms a most important element in a large majority of the cases of phthisis which we are called upon to treat, and the effect of masses of tubercular infiltration, or of numerous isolated tubercles scattered throughout the pulmonary tissue upon the functions of the lungs, independent of the question of subsequent irritation arising from their presence, cannot be otherwise than of the most injurious character. Dr. Graves has himself seen tubercles to an extraordinary extent make their appearance in the lung in the space of two or three weeks, and has known persons to die of the suffocation caused by this rapid development without the usual symptoms of phthisis; and it must have occurred to many to observe cases in which a similar rapid and fatal change takes place from recent deposition where the symptoms had indicated that a small extent only of the pulmonary tissue had been previously implicated. It may indeed be stated as a general principle that the first deposit of tubercle is rarely to such an extent, as to produce fatal effects, although it can scarcely be questioned that where tubercular deposition has at any time taken place, so as to become appreciable, the predisposition to further deposit is most fearfully increased. The most important, then, and, as Dr. Graves observes, one of the first morbid changes arising from the scrofulous habit, is the formation of tubercular matter. The presence of tubercles in any of its forms, however, is not so much the disease we are called upon to combat as one of its effects.

Dr. Graves contends that in all cases of phthisis "the pectoral symptoms, of whatever nature they may be, are caused by scrofulous inflammation," by which we presume that he means, inflammation as it occurs in individuals of a scrofulous diathesis, and he proceeds to compare the progress of ulcerations of the lungs with that of external scrofulous abscesses. There is, he observes, the same slowness, the same insidious latency, the same gradual solidification and gradual softening; the puriform fluid secreted is similar in characters, while there is the analogous occurrence of burrowing ulcers and fistulous openings with close approximation in the form of thin parietes, and difficulty of healing in each; and at the same time constitutional symptoms identical in nature; hectic flushings and sweats, diarrhœa, emaciation, &c. equally accompany phthisical suppuration of the lungs and scrofulous inflammation of the joints or other external parts. With these views, therefore, we are not

surprised to find Dr. Graves entertaining the opinion that tubercle, though a most frequent accompaniment of phthisis, is neither the essential cause of that disease nor a necessary product. Scrofulous inflammation is with him the *fons et origo*, the real and efficient cause of phthisis, whether tubercle be generated in the course of the diseased action or no, and thus we have scrofulous pneunomia and scrofulous bronchitis equally productive of phthisis without the presence of one single tubercle or spot of deposition of tubercular matter, either in the pulmonary tissue or on the bronchial membrane. In the latter case, scrofulous bronchitis, it is urged by Dr. Graves, that the accompanying fever presents all the material phenomena of phthisis; there is the same emaciation, frequently the same incurability; the same means tend to its aggravation or benefit, and the same scrofulous pus is secreted although not mixed as in cases of true phthisis with broken-down tubercles.

But if we are to admit of this line of argument we might include chronic pleurisy and chronic pneumonia occurring in scrofulous constitutions, with many other forms of pulmonary disease in the same category. The question after all is merely one of name, Dr. Graves understanding the term phthisis in rather a wider and, perhaps it may be said, more indefinite sense, than as it is now generally received. Still, however, if terms are signs by which corresponding ideas are to be conveyed, the more definite their use the more accurate the knowledge derived from them; and though we are by no means disposed to question the fact that tubercle is not an inseparable accident of scrofulous inflammation, the constitutional symptoms of which are the same whatever organ is affected, and the more prominent local symptoms of which, where the pulmonary tissue or bronchial membrane is affected, are the same with those in which there is tubercular deposition,—still we think the term phthisis, as used by modern authorities to indicate tubercular disease of the lungs, has its convenience, and we should be sorry not to retain it in that signification.

That inflammation of the bronchial membrane occurring in persons of marked scrofulous constitution may assume a peculiar character, we see no reason to doubt, and we are quite as willing to admit that there is a form of bronchial disease to which the term scrofulous bronchitis may be applied with as much propriety as the term scrofulous ophthalmia is applied to the inflammations of the eyelids and conjunctiva, of such frequent occurrence in like constitutions. It may also be admitted that the bronchial affections, so commonly met with in tubercular phthisis, are of this character, and that such affections, while, as is well known to most practitioners, they give rise to an increase of cough, dyspnea, and other among the most distressing symptoms of the disease, may also have much to do in producing the chills, flushings, perspirations, and other signs of constitutional disturbance. On the other hand, it by no means follows that tubercle is otherwise connected with them in its origin than as taking its rise from the same constitutional taint or deviation from the healthy standard, predisposing to a certain series of morbid actions in those exposed to causes of disease, rather than to a certain other series which would be developed in constitutions of a different character.

In persons of the scrofulous diathesis every disease assumes more or less of a scrofulous nature, one of the main characteristics of which is a tendency to the deposition of tubercles. But, as it seems to us, these are aberrations of the nutritive and assimilative functions which may or may not occur, giving rise by their presence in whatever organ to serious disease, predisposing to attacks of inflammation, which in such cases assumes the scrofulous character, and sometimes taking place in immediate connexion with but not necessarily dependent on such inflammation. When such a deposition takes place in the lungs, we have the disease to which, by almost universal consent, the term phthisis is now assigned, among the primary symptoms of which are found more or less of cough, dyspnea, quickened circulations and other signs of local and constitutional irritation varying according to the extent of the deposition. To these a disposition to bronchial congestion is added, necessarily connected as it would appear with the diminished capacity of the pulmonary tissue. Frequent attacks or paroxysms of bronchial disease, scrofulous bronchitis if you like, are the consequence, and now we have the more marked characteristics of phthisis in its advanced stages, hectic, chills, evening flushings, and night sweats, purulent expectoration, &c., but in all this we see tubercles as the primary disease, certainly as a prior link in the chain to scrofulous bronchitis, and which therefore we look upon as the true pathological character of phthisis, although perhaps most of the leading symptoms in its later stages may be identical with those observed in other affections, occurring under similar constitutional peculiarities. "But," says Dr. Graves, "if tubercles were capable of producing inflammation, we should discover some traces of it in every lung where they are found to exist, and yet you will meet many cases in which you cannot detect the slightest trace of it down to the very edge of the tubercular mass." (p. 282.) We can by no means agree in this conclusion; various circumstances may occur to restrain or counteract the influence of any active cause of disease, to become a preventive check upon its energy, or to neutralize its effects. That tubercle is per se necessarily productive of inflammation is another affair, but we cannot doubt that it may and does frequently prove the cause of the inflammation and congestion of the bronchial membrane in the lungs of those affected with it. Upon the whole we are disposed to admit the three forms of disease in the lungs recognized by Dr. Graves, as arising from scrofula—scrofulous pneumonia, scrofulous bronchitis, and tubercular development.

"We may, therefore, have tubercles without either the pneumonia or the bronchitis; and we may have scrofulous pneumonia often ending in slow burrowing suppuration, and proving fatal without any tubercles being formed. In like manner, a person may die of scrofulous bronchitis without the occurrence of either tubercles or pneumonia. Of these three effects of scrofula, it may be remarked, that, owing to their cause and origin being the same, they are most frequently found in combination. The same diathesis which produces one may give rise to the others; hence the frequency of their association; hence it is that they generally occur together." (p. 282.)

The last of the three, tubercular development, is distinctly recognizable, in the greater number of instances, from the other two by careful auscultation, and although many of the symptoms, constitutional and local, may be common to all, it is right that it should be as distinctly marked as a separate disease.

The remarks of Dr. Graves on the treatment of phthisis and of persons in whom phthisis threatens are judicious and confirmatory of the views of the best practical authorities upon the subject. Viewing the disease in all its forms, as taking its origin in a scrofulous tendency, his prophylactic measures are directed to the strengthening and support of the general system and the improvement of its tone. Early rising, cold washing, free exposure to the air, exercise, with nutritious not stimulating diet, form the main point of the regimen which he recommends. The treatment of persons in whom tubercle has become developed in the lungs, or who are attacked with scrofulous bronchitis or pneumonia, is conducted upon the same principles as those which are recognized in inflammation of other organs occurring in scrofulous constitutions. It is with regret that we are compelled to add that little if anything is contributed to what is already well known, and no additional prospect given of effecting the cure of consumption.

The proposal made in a subsequent lecture to cure certain forms of phthisis by rapid mercurialization, may be thought by some to prove an exception to this remark, and though our readers are probably already acquainted with the views of Dr. Graves and others upon this point, we cannot, therefore, altogether pass it over without notice. The idea seems to have occurred about the same time to Sir Henry Marsh, Dr. Graves, and Dr. Stokes, and originated, at least as far as Dr. Graves is concerned, in the success attending Dr. O'Beirne's employment of the same means in acute scrofulous inflammation of the joints. The form of phthisis in which this practice is stated to have proved successful in effecting a cure, is where the lungs become affected before any general contamination of the system takes place. "It is in such cases, and such only," says Dr. Graves, "that mercury ought to be tried, and it will avail nothing except when the commencement of the scrofulous inflammation of the lung has arisen suddenly, and in consequence of the operation of some obvious cause, as catching cold or the occurrence of hemoptysis." (p. 602.) Some instances are related of the good effects of this plan of treatment, and the observations of Dr. Munk on the same subject quoted at length from the Medical Gazette. The practice is not altogether new, but has been before recommended and followed by Dr. Rush and some other American physicians. They, however, failed to indicate the class of cases in which it was calculated to prove of service, and the indiscriminate employment of the method, and its consequent want of success in a vast number of instances may probably have been the cause of its having fallen into disuse. In the cases in which the mercurial treatment proves applicable, the tubercular affection, however, would seem to be of only secondary importance, so that this method appears to be chiefly serviceable in scrofulous pulmonary inflammation, rather than in tubercular phthisis, affording an additional reason derived from curative indications for retaining the term phthisis in its more definite sense.

ENLARGEMENT OF THE LIVER. In lecture thirty-six we meet with some interesting observations on the occurrence of hypertrophy of the liver from various causes, and the occasional curability of this affection. In persons below thirty, it is observed, the liver may become very considerably enlarged and yet under proper treatment return again to its natural size. Several cases are referred to in illustration and among

them one remarkable instance of an old gentleman between seventy and eighty years of age who had enormously enlarged liver and ascites, in which the enlargement rapidly decreased with a corresponding improvement of the general health under the use of blue pill in combination with hydriodate of potash.

The enlargement of the liver here spoken of, Dr. Graves traces to a cachectic habit, and regards it as similar to what is frequently observed in scrofulous habits and in persons suffering under the conjoined influence of the poison of syphilis and the injudicious use of mercury. When it was the custom to salivate every venereal patient and to keep him under full mercurial influence for a month or two, it frequently happened, says Dr. Graves, that just as the mercurial course was finished the patient got disease and enlargement of the liver. It is hinted that some connexion might perhaps be traced between the stimulant effects of mercury on the liver and the subsequent hypertrophy. Dr. Graves, however, for the present contents himself with noticing the fact; leaving, as he says, the explanation to his juniors, who always explain matters, according to his observation, much more readily than their seniors. Other diseased states to which hypertrophy of the liver is traced as one of the results of a general affection of the system are scarlet fever, morbid conditions of the heart, and intermittent fevers. It is not however necessary that we should in this place examine the subject at greater length.

Paraplegia. It is well known that much obscurity hangs over the precise seat of many cases of paraplegia, and the cause of the symptoms has been variously located by different observers and in different instances of the disease, according to the general views taken by the observer or the peculiar features of the individual case. The instances of paraplegia to which Dr. Graves directs attention in his thirtieth lecture differ from those commonly described as arising from morbid action in some part of the cerebro-spinal axis in several important particulars. Two forms are especially particularized, and the cases of either form resemble each other in taking their origin from impressions primarily made upon the peripheral extremities of the nerves. In the form first described these impressions would seem to be conveyed by reflex action to the medullary column from various parts of the system, subsequently giving rise to paralysis of the lower extremities. Instances are related in which the mucous membrane of the intestinal canal, the liver, and other abdominal organs were the parts primarily affected; in others, mentioned by Mr. Stanley and Dr. Stokes, disease of the kidneys was the apparent cause of the subsequent paraplegia. In several of these cases the spine was carefully inspected and no morbid change, after the most patient scrutiny, could be detected. Dr. Graves correctly observes that examples are not wanting of paralysis of other parts of the body produced in like manner, and refers also to the effects on the nervous system of another character produced by the reflex action of irritating impressions on the intestinal mucous membrane, as tending to explain these cases. Thus convulsive paroxysms are very commonly caused by the presence of worms in the intestines and by the accumulation of irritating matters of various kinds.

The following example of a partially paralyzed state of the retina,

apparently depending upon an impression made upon the cutaneous branches of the nerves of the face, is interesting:

"A medical student, travelling through Wales on the outside of the mail, was exposed for many hours to a keen north-easterly wind blowing directly in his face. When he arrived at the end of his journey he found that his vision was impaired, and that everything seemed as if he was looking through a gauze veil. There was no headach, no symptom of indigestion, to account for this evidently slight degree of amaurosis, and yet he was recommended to use cupping at the nape of the neck and strong purgatives. When he consulted me, which he did in the course of a few days afterwards, I at once saw that there was something unusual in the case; and, after a careful examination, I at length elicited from him the fact of his having been exposed to the influence of the cold wind. It was now apparent that the retina suffered in consequence of an impression made on the facial branches of the fifth pair. The cure was effected, not by a treatment directed to relieve cerebral congestion, but by stimulation of the skin of the face, forehead, temples, &c." (p. 398.)

In some of the cases referred to, especially those which are connected with and were the sequelæ of fever, the spinal cord may have been itself primarily affected, and the absence of appreciable lesion on inspection forms no conclusive argument to the contrary. Still we are not disposed to question that his views with respect to the occurrence of this and other forms of paralysis as well as other affections of the nervous system from a morbid state of the peripheral extremities of the nerves, are in the main correct. With the remarks which follow we perfectly coincide:

"I could give many instances," says Dr. Graves, "of pains commencing in particular parts of the body and travelling back towards the spine so as to give rise to an affection of that organ, which has been too generally looked upon as the result of idiopathic disease. How often does this happen in hysteria? How often does it occur that the organ primarily engaged in hysterical cases becomes during the attacks acutely painful, and as the disease proceeds the pain travels back towards the spine, until at length the spinal cord itself becomes affected and we find acute pain and tenderness over some portion of its track? I am fully persuaded that many modern authors who have ascribed the phenomena of hysteric and other affections to spinal irritation, have been too hasty and indiscriminate in their explanations. In the majority of cases you will find hysteria patients complain at first, not of pain in any part of the spinal cord, but in the right side in the situation of the liver, in the region of the heart or stomach, or in the head, or the pelvic region. At this period there is seldom any tenderness over the spinal cord; but as the disease goes on the irritation which existed in some of those situations to which I have referred is extended to the spine, and pain and tenderness are now felt over some of the spinous processes of the vertebræ. When this has taken place, then the spinal irritation thus produced becomes itself a new cause of disease, from which, as a centre, the morbid influence is propagated to other organs." (p. 400.)

The other form of paraplegia described by Dr. Graves, and which he is certainly mistaken in believing not to have been mentioned by any other writers, also arises from affection of the extremities of the nerves. In this form, however, there is no reflex action, but the paralysis is apparently the result of a direct impression made upon the nerves of the affected limbs. The following case is an example:

"James Moore, aged 32, was admitted into the Meath Hospital on the 3d of March, 1833, under Dr. Stokes's care, for an attack of paraplegia, which he attributed to cold and wet feet while engaged in a quarry. About a month before admission he perceived a stiffness of the great toe of his right

foot, afterwards numbness and coldness of the sole, and then of the leg as far as the knee, and dragging of the limb in walking. During the progression of the disease up along the right thigh it commenced in the left foot, and after a few days he experienced almost complete paralysis of sensation in the right lower extremity, and a lesser degree in the left, accompained by so much diminution of the power of motion, as to render him unable to walk without support. About three weeks after the appearance of paralysis in the lower extremities, the little finger of the right hand was attacked with numbness, which passed successively to the rest, attended by some loss of the sense of touch and power of grasping objects. He had also retention of urine, and the bowels were obstinately constipated. There was no tenderness over any part of the spine. He had no pain in the head; his pupils were natural; pulse, sleep, appetite also normal." (p. 415.)

Dr. Graves relates several cases of this description; others must have occurred to the notice of such as are engaged in extensive practice. The main feature of this form of paralysis would seem to be loss of power or sensation or both in the extremities of the nerves distributed to the surface, arising from a direct morbid impression made upon them, and sometimes gradually creeping along their course in a retrograde direction, thus implicating other parts in succession, and by extension of the morbid impression, to the spine, gradually becoming propagated to more remote parts of the system. This description of paralysis seems very generally to arise from exposure to cold and wet, two instances of which, when the affection has continued purely local, we have ourselves witnessed. In both of these cases the hands had been kept partially immersed in cold water during many hours, and in both there was loss of power and sensation, which, although it did not spread, resisted every measure which was employed for relief. In one case the paralysed state of the hands still continues, the other we have lost sight of.

Similar to these, though arising from a different cause, would seem to be the dropped hands of the painters. The loss of power in these cases appears as often to arise from the direct impression made by the lead poison on the extremities of the nervous fibriles distributed to the hands, as from the absorption of the poison within the system. The paralysis arising during the course of painters' colic might be attributed to a reflex action on the muscular system, owing to the impression made on the intestinal canal, as in the form of paraplegia first alluded to, but Dr. Graves is disposed to think that it results rather from the direct impression of the poison on the nervous system, and he is probably right.

Before leaving this subject we may allude to another diseased condition in which the spinal cord becomes affected, and paralysis is ultimately generated from impressions originally made as it would seem upon the peripheral extremities of the nerves. This is pointed out by Dr. Graves in a subsequent lecture, where he endeavours to establish the point, "that in certain cases where gout attacks the nerves, giving rise to gouty congestion or inflammation, frequently recurring, and acquiring increased strength, and deeper root as it proceeds, the morbid affection may, after years or even months, run on until it reaches the spinal cord, involving a certain portion or portions of that organ, and producing loss of sensation and motion commensurate to the amount of spinal derangement," (p. 588;) or, as he otherwise lays it down, "*that gouty inflammation of the nerves and their neurilema may, in process of time, extend to the*

spinal marrow and its investments, and give rise to derangements of the latter, terminating in ramollissement and structural derangement, (p. 589.) The principle here attempted to be established is illustrated by some important cases, and gives rise to the practical indication of endeavouring to anticipate and divert the attack elsewhere, when the cerebro-spinal axis is threatened. For this purpose Dr. Graves, having experienced the total inefficacy of colchicum, hydriodate of potash, and all the usual remedies for gout in relieving or removing this form of the disease, recommends the early insertion of issues over the spine with prompt and decided mercurialization.

Inflammation. The thirty-third lecture is devoted to the consideration of inflammation, and the motor power of the circulation. Both of these subjects, at least in the mode in which they are treated, are foreign to the professed objects of the work, while it is sufficiently evident that to take such a view of either of them as would suffice to come to a clear understanding, would far exceed the limits which we could at present spare for the purpose. With respect to the latter subject therefore, "the motor powers which cause and regulate the circulation," we must content ourselves with stating that Dr. Graves is of opinion that the capillary vessels play a most important part, and quotes with approbation the views put forth by Dr. Carpenter in his Physiology.

In inflammation all is attributed to the active force of the capillaries. "*The capillaries have the initiative; with them commences the enlargement which afterwards extends to the smaller arteries, and from these to the larger branches.*" (p. 484.) Dr. Graves is opposed to the idea that in this enlargement there is, as was contended for by Dr. Hastings and Dr. Wilson Philip, any traces of debility. He says distinctly that the swelling, exaltation of nervous sensibility, increase of temperature, and augmented secretion, are all in fact opposed to the theory of debility. "There is no passive dilatation from weakness, the capillaries enlarge from increased and not from diminished action: red blood finds its way into vessels which before received only white, and unusual secretions occur in the affected parts." Now though we have not space to consider and perhaps are not inclined altogether to deny what Dr. Graves advances, as to the property which the capillaries possess of actively dilating and drawing the blood into them being one of the principal causes of the circulation, we can by no means admit that the property in question has been so far shown to appertain to these minute vessels as to entitle it to be received as the basis of pathological deduction. Dr. Hastings observed certain facts in the capillary circulation which, as he conceived, admitted of explanation upon the principle of debility. Whether the stagnation of the blood in the irritated and inflamed capillaries, and the distended state of these vessels observed by him, were caused by debility of their coats or not, there is no question that the distention, or dilatation if you will, does take place, and that, from the crowding of the blood-corpuscles, there is subsequent obstruction. If the active power of the capillaries be the sole efficient and continuing agent of this influx of red globules, there seems to be no reason why these minute vessels should retain the globules imprisoned within their embrace for an indefinite period of time, and at great injury to the vital organization of the part.

Without entering into the controversial discussion which the author takes up with Müller, Dr. Marshall Hall, Dr. C. Williams, and others, and without pledging ourselves to the opinions advanced by these authorities, we cannot but remark that as far as relates to the expression of the fact, Dr. Hall is correct when he states that the first appreciable physical effect of the inflammatory process is the adhesion of the blood-corpuscles upon the internal surface of the capillaries, which is followed by their ultimate stagnation. Upon the statement of this fact, as made by Dr. Hall, we find the following remark:

"Here you perceive that the first step is the adherence of the globules of the blood to the internal surface of the capillaries, the consequence of which is that the caliber of these vessels is considerably diminished so that they become obstructed and cause a stagnation of the blood, which Dr. Hall looks upon as the essential character of inflammation." (p. 466.)

But this is a very unfair inference, and one that is by no means borne out by facts which Dr. Graves may, if he please, observe. It has been shown by Mr. Addison, and we have ourselves verified the observation, that in the passage of the blood through the capillaries the corpuscles pass in single file, some of these occasionally turning aside, as it were, from the current and for a time attaching themselves to the parietes of the vessel, those succeeding passing round or rolling over them as they partially project from the walls. Now in the inflamed or irritated state of the same capillaries, the corpuscles rush in greater numbers through the vessel, the caliber of which is not "considerably diminished," but on the contrary considerably increased, the globules passing in double and triple files, or crowding in still greater numbers with the current of fluid, while the sides of the vessel are occupied by numbers of them in a state of stagnation, some of which from time to time roll off into the general current, others from behind coming up and taking their places, until at length, whether from over distention or debility, scarcely we think from active dilatation, and certainly not from contraction or diminution of caliber, the vessels become obstructed and there is eventually a complete stagnation.

But Dr. Graves and others have reasoned upon the view that the capillaries are simple membranous tubes. We question whether it has yet been proved that such is their nature, or, indeed, whether they are anything more than interstitial passages. We cannot, however, enter into the subject here. Our readers will find many facts bearing upon the investigation in Mr. Addison's paper just published in the eleventh volume of the Provincial Medical and Surgical Transactions.

There are several other subjects we had marked for consideration, and to which we should have much wished to direct attention, but to give even an imperfect abstract of all that is worthy of notice in a work such as that which Dr. Graves has produced is obviously impossible. Among those which we are compelled to pass over are the observations on syphilitic diseases, and many important points connected with them. These occupy lectures twenty-three to twenty-nine inclusive, and would alone require a special review to do justice to them. In lecture thirty-one we find a notice of the successful employment, at the Meath Hospital, of electro-magnetism, in various affections, by Mr. Clarke. The diseases

in which this agent proved serviceable are chronic rheumatism, lumbago, and sciatica, neuralgia, amenorrhea, and irregularities of the catamenia and their sequelæ, amaurosis, deafness, some forms of paralysis, especially those termed partial paralysis, and various nervous, neuralgic, and rheumatic pains. These, it will be observed, are precisely the class of cases which are relieved by electricity employed in other modes, but there would seem to be advantages attending this peculiar method of generating it. In the succeeding lecture on sleeplessness, there are some excellent observations, and it is stated that in the form which frequently occurs in hysterical female hypochondriacs, and persons of nervous and irritable habit, Dr. Graves has found antispasmodics, musk, and assafetida serviceable, when opium in all its forms, and all kinds of narcotics, had previously failed. The cases of jaundice appended are interesting, and show the great importance of immediate attention being paid to nervous symptoms, arising in the course of that disease. Other subjects on which there are valuable remarks scattered through various lectures, are erysipelas, phlebitis, glanders, phlegmasia dolens, cancrum oris, various diseases of the skin, diarrhea, amaurosis, periostitis, &c. Our favorable opinion of the entire work may be gathered from the preceding remarks. We cannot exactly call it, with the author, a System of Clinical Medicine, but no practitioner of medicine should be without it, since there is scarcely a disease to which the human frame is liable which does not receive in it some illustration, direct or incidental; and, as a guide to practice, in very many instances, especially when difficulties arise, it will often be found a most useful work for reference.

Art. XVII.

Medical History of the Expedition to the Niger during the years 1841-2*: comprising an account of the Fever which led to its abrupt termination.* By J. O. M'William, Surgeon of H. M. S. Albert, and Senior Medical Officer of the Expedition. With Plates.—*London,* 1843. 8vo, pp. 296.

There are few of our readers but must remember the melancholy impression made on the public mind by the disastrous result of the expedition to the Niger, when this was made known in England through the newspapers. And none who remember this can forget that pathetic passage in the story, which represented the noble conduct of the surgeon and the geologist of the expedition, when left alone, in the far recesses of the Niger, amid their heroic companions all stricken to death or to death-like helplessness by the fatal fever of the country. In this trying conjuncture, when the salvation of all on board depended on the speedy removal of the ship from her actual position, Dr. M'William took the navigation on himself, steering with his own hand and piloting the vessel through all the intricacies of the river, while his companion worked the engine below. There is something very affecting, we had almost said sublime, in the picture thus presented to the imagination of these two solitary men of science assuming offices so foreign

to their past habits and knowledge, stripped of all exterior cognizance of their class, standing as humble workmen at the helm and furnace, toiling by day, watching by night, while the force of the stream and the paddles was sweeping their ill-fated bark, freighted with their dying or dead companions, through the manifold dangers of their unknown course. The author of the volume before us was the clear-headed and stout-hearted pilot who did this, the undoubted preserver of the ship and her surviving crew; and the slight and simple way in which he speaks of his own exertions strikingly illustrates the old truth, that the brave man is ever modest.

To visit with critical severity a book written under such circumstances would seem harsh in any case; but we can conscientiously say, that the volume before us stands in no need of such protecting influence. It is simply, clearly, and correctly written, and tells exactly what the title-page and preface promise. It is truly a *history*, a narrative of facts, unmixed with speculations; and an interesting history it is, and one which must be referred to by all future explorers of Africa, and all future inquirers into the nature of the fevers of that fatal country. Though the field explored by Dr. M'William was ample, the living subjects of his observation were comparatively few; and on this account he has wisely contented himself with giving the exact results witnessed, without any attempt at generalising them or applying them to other times and places.

The late period at which Dr. M'William's volume has reached us must prevent our giving any other than a brief notice of its contents; but we earnestly recommend its perusal to all our readers. It is a model for the composition of similar works.

The work is divided into three parts:

"The First comprises as much general description as is necessary to put the reader in possession of those circumstances of position and climate which could produce or modify disease.

"The Second contains an account of the fever as it occurred on board the Albert, embracing its main features and treatment.

"In the Third Part will be found a few facts relating to the state of medicine in the Niger, and to vaccination among the Blacks; a brief description of the system of ventilation adopted in the ships, with some remarks on its employment on the coast and on the river; an abstract of the meteorological observations which were made after the plan recommended by the Royal Society; and lastly, a brief account of the geology of the Niger.....

"The Expedition left England on the 12th of May, 1841, and entered the Niger on the 13th of August. Three weeks from this period fever broke out among the crews, and soon produced effects so disastrous that two of the three steam-vessels composing the Expedition were obliged to return to the sea, and the other was compelled to follow a few weeks after." (Preface, iii-iv.)

"The following is a list of officers, seamen, and marines, of men of colour, of various nations, who joined the expedition in England; and of Kroomen and liberated African boys entered on the Coast, who were on board H.M.S. Albert, Wilberforce, Soudan, and Amelia, on their entrance into the Nun branch of the river Niger:

H. M. Ships	Officers, including Civilians and Engineers.	Number of White Seamen.	Number of Marines and Sappers.	Total Number of Whites.	Men of colour entered in England.	Kroomen and liberated Africans entered on the coast.	Blacks for Model Farm.	Total Blacks.	Grand Total.
Albert	18	23	10	51	14	44	...	58	109
Amelia tender	3	2	2	7	...	9	21	30	37
Wilberforce . . .	21	23	13	57	8	39	2	49	106
Soudan	11	15	4	30	3	18	...	21	51
Total of each class . .	53	63	29	145	25	110	23	158	303"

(p. 44.)

And the following Table gives at one view the total mortality from the time the Expedition left England to its completion:

"*Return of the Total Mortality stated under the respective Ships to which the Officers, Seamen, Marines, &c. belonged, from the time the Expedition left England to its completion.*

	ALBERT, including the AMELIA, &c.				WILBERFORCE.				SOUDAN.				
	Officers, including Engineers.	White Seamen, Marines, and Sappers.	Kroomen, liberated Africans, &c.	Total.	Officers, including Engineers.	White Seamen, Marines, and Sappers.	Kroomen, liberated Africans, &c.	Total.	Officers, including Engineers.	White Seamen, Marines, and Sappers.	Kroomen, liberated Africans, and other Blacks.	Total.	Grand Total.
Average complement	21	37	88	146	21	36	49	106	11	19	21	51	303
Deaths from fever contracted on the coast	...	...	...	...	1	...	1	2	...	...	...	...	[illegible]
in the river	6	14	...	20	3	6	...	9	5	8	...	13	42
Accidents . .	1	2	1	4	...	1	...	1	...	...	1	1	6
Other complaints .	...	2	...	2	...	...	1	1	...	...	...	...	3
	7	18	1	26	3	7	2	13	5	8	1	14	53"

(p. 130.)

There appears to have been nothing peculiar in the disease of the Niger: it was evidently the *Bilious remittent fever* of the older authors, as will appear from the following extracts:

"The accession was seldom accompanied by very marked shivering, yet previous to the period of vascular excitement, the patient usually experienced a

sensation of coldness, and for the sake of warmth would fain have exposed himself to the rays of the sun. He would shortly express a wish to lie down, and would complain somewhat suddenly of increase of headach or giddiness, and intense heat of the skin, which had a dry parched feel, restlessness, intolerable nausea, and difficult breathing. The dyspnea in several instances, particularly in my own case, was extremely distressing, and continued from one to four hours, until relieved by spontaneous vomiting, or the occurrence of diaphoresis. Headach was with some the prominent symptom during the hot stage, and the feeling was described as that of a cord being tightly girded round the temples. The thirst was very urgent; the tongue was foul in the centre, moist, clean or reddish, and invariably marked by indentations on the edges. The countenance was more or less flushed, the eye occasionally suffused and always looked wild. Pulse rapid but small, frequently feeble; thirst urgent, bowels constipated, and urine passed often and in small quantity. There was in general tenderness of the epigastrium, sometimes acute, but often not discoverable unless upon pressure.

"In some cases, coldness of the stomach was complained of some days before death. A subsidence of febrile action in general followed in from three to six hours, or at all events, the symptoms if continued beyond the latter period became much mitigated. Diaphoresis came on, the thirst moderated, and the signs of oppression in a great measure disappeared. The principal complaint at this period was from *the disagreeable odour of the perspiration, particularly in those cases that subsequently proved fatal.* I was not sensible of this peculiarity in the smell of the perspiration in my own case, but I perceived it very distinctly in several others. The sweating continued until from eight to twelve hours had been occupied by the whole paroxysm. The patient, although considerably exhausted, expressed himself as free from all trouble, and the countenance also indicated improvement. This seemingly favorable change did not last long, for the accession generally returned in from six to ten or twelve hours. Occasionally the respite extended to twenty-four hours. In a few cases, there was a treacherous interval of forty-eight hours, in the early period of the disease; but these invariably assumed afterwards a low malignant type. The fever in them seemed to have rested only to give strength for a fresh accession.

"The accessions did not seem to observe any law of periodicity. They came on, disappeared, and returned at all hours of the day and night. The evening, however, was a more common time of accession than any other; in which case, after the cold sensation had passed off, the paroxysm generally ran through its stages in the course of the night, and had suffered a considerable remission by the hour of breakfast (eight) the next morning.

"In a few instances the remissions were as complete as in the interval of ague. These were, however, only exceptions to the general rule, for total absence of fever was indeed of rare occurrence during the course of the disease.

"I cannot say that the influence of critical days was at all apparent, further than if no material improvement was evident by the eighth or ninth day, the prognosis was then most gloomy." (pp. 132-4.)

"When the disease was about to take a favorable turn, the remissions became distinctly marked, and the intervals were lengthened. The countenance (the best criterion) assumed a natural expression, a certain look of convalescence, that one can only become acquainted with by experience and contrasting it with that indicative of a fatal termination. The skin became moist, the thirst diminished, the pulse was more voluminous and softer; the tongue gradually lost its tremulousness, and could be more easily thrust out of the mouth; it often continued a long time loaded, but the crust was less brown, and more moist, and seemed to have lost its firm attachment to the organ: at this period diarrhea was by no means uncommon, and also a copious flow of urine, which latter was a very favorable symptom. A strong desire for food was expressed by most of the patients who had advanced thus far, and I had more than once cause to regret having gratified it." (p. 135.)

"*Delirium* was a very bad symptom in the fever of the Niger: of twenty-one cases in which it occurred fourteen died, of whom one was drowned by eluding his nurse, and jumping into the river." (p. 136.)

"*Petechiæ* or *sudamina* were not observed in any case. ....

"*Yellowness of the skin* occurred in nineteen cases, thirteen of which were fatal, and the average day of the appearance of this symptom was the ninth. The yellow colour was first seen in the conjunctiva, and afterwards extended over the face, arms, and the rest of the body. It was in general light, and did not appear after death in any case in which it had not existed during life. The fæces in these cases were generally of a bilious colour." (pp. 137-8.)

Only eight bodies of the dead could be examined by Dr. M'William: the results, given in a concise and satisfactory form, present nothing different from what has been recorded by preceding authorities. No morbid peculiarity that could be fairly attributed to the fever, was found in the head, thorax, or cavity of the abdomen. The mucous coat of the stomach was invariably softened, occasionally marked with gorged vessels and dark patches, and exhibiting in others small points of ulceration. The duodenum was affected in the same way as the stomach but in a less degree. The ileum was softened towards its lower end, and exhibited livid spots and also some ulcerations. Peyer's glands were enlarged in three cases. The liver was large in two cases, anemious and dry in two others. In three cases the gall-bladder was filled with tar-like bile, and in a fourth this was mixed with blood. The spleen was enlarged and softened in one case; in a second, enlarged but firm. In two cases the blood was found fluid several hours after death.

The author has an important section on the *sequences* of the fever, which were chiefly chronic affections of the mucous membrane of the bowels in the form of chronic dysentery, hemorrhage, and colic; also hepatitis. True intermittent fever was still more common.

"On board the Albert none of those who had fever in the Niger, and were not at once sent to England, escaped intermittent. Five who suffered severely were invalided at Ascension, nearly nine months after the vessel left the Niger. In the Wilberforce nine cases of ague, following Niger fever, occurred during the passage to Ascension. The severity of the intermittent did not always bear a relation to the intensity of the primary remittent. Commander Fishbourne was affected by the remittent in a comparatively mild form, but he was many months afterwards as violently visited by intermittent as any one in the expedition. Nearly twelve months after recovery from remittent, and after about eight months of freedom from intermittent, I was three days confined to bed, with the latter complaint, during the passage to England, just as we were getting into cold weather. It appears, therefore, that after remittent a person continues long liable to intermittent, and further that remittent and intermittent are produced by the same cause in different degrees of intensity." (pp. 154-5.)

The section on the *causes* of the fever is valuable, but throws no new light on the subject, except to disprove the theory, promulgated some time before the sailing of the expedition from England, that the existence of free sulphuretted hydrogen gas in the waters of the Niger might account for the fever. Dr. M'William shows in the most satisfactory manner that no such gas is found in the Niger, and that what was detected in the specimens sent to England, originated in the bottles themselves from decomposition of their contents. The temperature was no doubt great, "seldom under 84° on the lower deck," but a similar degree of heat

exists elsewhere without producing fever. It also appears that this high temperature was not combined with great moistness of the air, a combination so generally believed to be instrumental in producing fever.

"A reference to the meteorological tables will show that the atmosphere was in general far from being moist; the dryness of the air indeed increased as we advanced upwards, and it was remarkable at Egga, so that it can hardly be supposed that the body was predisposed to disease by the atmosphere carrying off its electricity by induction, or by impeding exhalation from the skin." (p. 176.)

Failing to trace the source of the disease in these and other chemical or meteorological influences, the author falls back on the old etiology of marsh miasma, or malaria, without attempting to explain what this is. It is evident, however, that the conditions which have been generally recognized as the most fertile sources of this poison, existed, in great force, in the Niger.

"We had traversed (and slowly) a country of a character recognized as eminently fertile in the production of fever. Swamps in a rich alluvial soil abounded. The islands and banks were everywhere more or less inundated. The vegetation was rank and profuse. The ships' companies were necessarily a good deal exposed on deck during the day, and were sometimes harassed by frequent anchoring and weighing in ascending the river. The actual labour cannot be said to have been great, but there was a degree of restless anxiety inseparable from being constantly liable to be called on deck. The upper deck was much lumbered, in a degree preventing the free circulation of the air." (pp. 177-8.)

Dr. M'William shows that *contagion* had no share in producing the disease. He also proves that one attack does not secure against a second.

In the section on *treatment* the author briefly details the result of his own observation and experience. His practice seems to have been judicious, but it presents no feature of novelty. In fact, we are much disposed to believe that, as in the case of Dr. M'William's predecessors, the course of the disease and its fatal issue were, in no way, checked by the curative means employed. For these means we must refer to the work itself. We fully agree with the author in the following observation :

"'Pessimum ægro Cœlum est, quod ægrum facit.' The most important step in the treatment of African fever is comprehended in this maxim of Celsus. By common admission of all who have served on the western coast, the causes of fevers exist in a state of concentration in the rivers; and hence, as a general rule, the greatest amount of mortality will be found on board those ships whose crews are most employed in river service. I have no hesitation in saying that in most instances, a favorable turn in the form of the fever, even in its earlier stages, will attend a speedy removal from the locality where the disease originated ; and that after the fever has run its course, change of climate is indispensable, if we wish to avoid intermittents, visceral complaints, and a host of ailments that rarely fail to follow in its train." (p. 194.)

Our space will not allow us to notice any of the subjects treated of in the last chapter. We cannot dismiss the work, however, without once more recommending it to the notice not only of our own readers,—especially to all engaged in the public service,—but also of the general reader : it is really interesting as a book of travels.

PART SECOND.

Bibliographical Notices.

Art. I.—*The Life of a Travelling Physician, from his first introduction to Practice, including Twenty years' Wanderings through the greater part of Europe.* In Three Volumes.—*London*, 1843. 8vo, pp. 312, 304, 294.

This book is not medical; but we notice it on account of its title and of its author, a distinguished member of the profession. Although there are scattered here and there throughout the volumes, brief remarks respecting medicine and medical men, Sir George Lefevre seems to have studiously avoided everything professional in the composition of his book, no doubt intending to favour his own brethren by and bye with an analogous treat to that which he has here given to the general reader. The book is really a very clever one, full of amusing and interesting matter, and giving unequivocal evidence of the author's talent for observation both of men and manners, as well as of a happy facility of graphically delineating what he saw. Although the perusal of these volumes will not add to the medical knowledge of our readers, we can assure them that it will minister not a little to their amusement. As a slight specimen of the author's lively style and manner, we transcribe a few passages from the beginning of the book. Sir George, just dubbed, and fresh from Edinburgh, was delivering his credentials in London, in all the buoyancy of youthful expectation—

"From Pall Mall East I descended a little in the aristocratic scale, and made my next assault upon a dapper little doctor who lived in Bloomsbury Square, to whom I had very strong recommendatory letters. He was the very antipodes of my Scotch friend; he wore powder and silk stockings, and though not very far advanced in his professional course, was, what is styled in medical parlance, a "rising character." He was under the special protection of an old practitioner, who was putting him by degrees into his shoes, which became daily more easy to their new wearer. He was still upon his legs, and had not even launched his *voiture expectante*, yet the profession looked upon him as a rising man. He was established on the neutral ground, or half way between the city and the west-end; and there is a very sensible medical line of demarcation. All is city from Bedford Row eastward. The neutral ground lies between Bedford Row, which it includes, together with the squares to the right, as far down as Charing Cross. All the rest is west. North and south were not marked in the medical chart at that time. Now the neutral ground is very thick set with doctors. It is aspiring ground, and the public judge much of a man's talents by the way in which he seems to thrive himself. The city is decidedly plebeian, the neutral ground aspiring, the west-end aristocratic.

"When young physicians commence in the city, they take a lodging in Fenchurch street, where they generally reside two years upon their private means, if they have the means of residing there so long. They take no fee during this

period, but talk very much of their practice increasing, as soon as they have taken one, which is about the beginning of the third year. They talk of having doubled their practice the fourth year, which means that they have taken two fees, and they change their lodgings and remove to Bucklersbury. Here they remain stationary for some time, and if they do not succeed, put their diplomas into their pockets and go into the country to practise as apothecaries. If they succeed, however, and get enough to pay their washerwoman, they take part of a house in Broad street, from whence they remove to the neutral ground and become rising men." (pp. 14-16.)

"I received a letter one morning from my West Indian acquaintance, inviting me to join him at the Italian Opera. I did so, for I was glad of the opportunity; I had never been there before. I was most grievously disappointed. It was between the acts of Il flauto Magico that the doctor turned round to me, and asked me if I would undertake his practice for a fortnight. He wished to go to Sevenoaks, where many of his patients resided, and if I would attend his dispensary, and visit the few patients who remained in town, he would gladly leave them under my care.

"In a few days I was installed in the Doctor's chair, and was myself become a doctor *de facto*. It required more tact to manage the dispensary pupils than the dispensary patients. I found some of these said pupils my seniors in more than age, and very inquisitive. A good face upon difficulties, and carry all with a high hand: I was an advocate for decided practice, as it is styled—a decided practitioner; and there is no more certain way of imposing upon people, than by impressing upon them this idea—say that a man is a decided practitioner, it is enough. Nobody will inquire in what sense—*bad* or good—this word 'decided' is to be taken. I bled, purged, and blistered decidedly, and the cases being of an inflammatory character, as upon Gil Blas' debut, it happened to be decidedly *good* practice.

"I never shall forget the joy which I felt when I fingered the first guinea. It was a genuine coin, for it was at this time, and a most memorable period it was, that I took my maiden fee. The old *unreformed guinea*, none of your sovereigns wrapped up with a shilling, as you see them now-a-days. It was pure and without alloy, and often did I finger it over in my pocket, and sighing involuntary, said to myself, How many more shall I receive in the career which is now opening to me? A conscientious hectic flushed across my face; it was the first and last time that I ever felt embarrassed at receiving a fee. I was in a few days afterwards presented with a second one; it came quite as a thing of course. I thought it tardy in its arrival. These are the only two fees which I took at that period in London." (pp. 19-21.)

ART. II.—*Du Bonheur en Chirurgie, Recueil de Faits Cliniques.* Par J. MOULINIÉ, Ex-Chirurgien en chef de l'Hôpital de Bourdeaux, Prof. de Clinique Chirurgicale, &c.—*Paris*, 1842. 8vo, pp. 224.

Success in Surgery; a Collection of Clinical Facts. By J. MOULINIÉ, late Principal Surgeon to the Hospital of Bourdeaux, Professor of Clinical Surgery, &c.—*Paris*, 1842.

THE conceit evinced in the title of this book is enhanced by a presumptuous and vain-glorious preface. "Demand of Fame," says M. Moulinié, "speak to my pupils, question my nurses, converse with men in practice who have attended my clinique, all will declare that my hospital practice has been distinguished by a rare degree of success. If such has not been the case with others, doubtless it is because others committed mistakes,"—which, of course, the all-fortunate M. Moulinié could not do. Still the book itself is not so bad as these self-glorifications would lead us to expect. The writer certainly does not appear

to have duly estimated the serious nature of the subject, which he has chosen. The tone of the book is frivolous and jocose; the quantity of new matter small, and the proofs of the principles desired to be established so deficient, as to be quite insufficient to convince any disbeliever. The author, in fact, has done a great act of injustice to himself; for sufficient hints occur here and there in the book to show that he has removed numerous abuses which existed on his introduction to the hospital, has been most diligent in the discharge of his duties, and in his own words has considered "humanity as the first duty of the surgeon."

To the English reader there is little new or of sufficient interest to attract attention, the best part being that which is devoted to the account of the author's success in operations on varicose veins. The varicose vein to be operated on having been chosen, the following proceeding is recommended from the writer's own experience:

"A fold of skin parallel to the vein, and at some distance above the varicose part, having been selected, an incision is made across this fold, down to the cellular tissue round the vein, disturbing the part by dissection as little as possible. An eye-probe armed with silk or a fine thread is then passed under the vein, by which the vein is tied. An incision is often made across the vein a little above the ligature in order to prevent any inflammation of the vein spreading by continuity along it. The skin is then brought together by strapping, in order to protect the vein from the contact of air, as much as possible." (p. 112.)

This plan appears from the accounts given by the pupils who have observed the practice of M. Moulinié, to have been unattended with bad results, and to have been very successful.

An interesting case of atrophy of one leg, existing from birth, is related (p. 81,) which was amputated at the request of the patient below the knee. The parts were all wasted, fatty and thin. The arteries could not be found, a little blood only flowing from the situation of the posterior tibial artery, when a ligature was placed; but even then it was doubtful whether it included this vessel. No secondary bleeding occurred, and the case did well.

Art. III. — *Beiträge zur Pathologie und Therapie mit besonderer Berucksichtigung der Chirurgie von* Dr. Carl Emmert, Privat docenten in Bern. Erstes Heft. 1842. 8vo, pp. 184.

Contributions to Pathology and Therapeutics, with especial reference to Surgery. By Charles Emmert, m.d. Private Teacher at Bern. Part I. 1842.

This is the first part of a series which it is the intention of the writer to publish annually, consisting of essays on the present condition of medicine and surgery, as well as on some of the principles of pathology, with accounts of and observations on individual cases of disease.

The present number contains a short account of the present condition of the healing art in Germany, with an essay on inflammation and hyperemia. To the former a small space only is devoted, whilst to the latter the author appears to have paid considerable attention. This part contains a summary of the views, especially microscopical, of the various observers of this subject with the addition of new experiments, and a laborious summary by the writer himself. The remaining part of the

work is devoted to the consideration of individual cases, of which the following is a brief summary:

1. The removal of a large bursa from the external surface of the patella with complete success.

2. Division of the ligament on the under surface of the joint between the first and second phalanx of the second toe, as well as the flexor tendons with complete relief to contraction of that joint.

3. Removal of the fleshy growths for the cure of an inverted nail. This paper is also very valuable for the numerous references to the labours of others.

4. Recurrence of a medullary tumour on the arm after its third removal with pulmonary disease.

5. Aneurismal dilatation of the aorta immediately above the valves, with the formation of a second sac between the pericardium and aorta, and a third sac under the skin; the first and second sac communicating by a round opening, the second and third sacs communicating by an opening between the 3d, 4th, and 5th ribs.

6. The most interesting case in the whole work is the description of a case of effusion of blood to the amount of two to three pounds in the thigh after a fall. The sac was laid open, and the wound examined without finding the bleeding vessel, subsequent to which the parts healed and the patient recovered.

ART. IV.—*Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern.* Herausgegeben von Dr. C. CANSTATT. Erstes u. zweites Heft.—*Erlangen*, 1842. 8vo.

Annual Report on the Progress of Medicine in general in all Countries. Edited by Dr. C. CANSTATT. Parts I. II.—*Erlangen*, 1842.

THESE portions of Dr. Canstatt's report are sufficient to show that both in design and in execution it merits the highest place among the numerous systematic *reports*, which it has of late become customary to publish. The whole work is divided into no less than thirty-six departments, each of which is assigned to a different contributor who has made that part of medical science an object of particular study. Among the names of the contributors are several well known and distinguished; for instance, Simon, to whom is assigned the department of medical chemistry and toxicology; Albers, who takes that of pathological anatomy; Remak, who has that of physiology; Lessing, who has the history of medicines; Stilling, who reports the progress of all relating to the nerves, &c. The parts already published are completely and, on the whole, well done; though sometimes in striving not to omit anything, the fault is committed of slurring too quickly over the more important matters. There is also this more serious fault, that before the report arrives in England, the observations upon which it is made are two or more years old. It is therefore of comparatively little value to those who are anxious to be *au courant* with the progress of medical science, without the labour of wading through the multitude of journals that are now published. As an historical record, however, and as a book of reference, the work is excellent.

ART. V.—*Beobachtungen über den Nützen und Gebrauch des Keilschen Magnet-Electrischen Rotations apparats in Krankheiten, &c.* Von J. E. WETZLER.—*Leipzig*, 1842. 8vo, pp. 182.

Observations on the Use of Professor Keil's Magneto-Electric Apparatus, in various diseases, especially in Chronic Neuroses, Rheumatic and Gouty Affections. By J. E. WETZLER.—*Leipzig*, 1842.

THE book before us cannot be considered as a scientific treatise on the subject of which it professes to treat, as it is but a record of cases; of cures performed, partially accomplished, or attempted; with a brief notice at the end, of the apparatus employed. The history of these cases, ninety-five in number, occupies 152 pages of this little volume. They are detailed with a minuteness and fidelity peculiar to the German character, but throughout the whole there runs so strong a predilection for the favorite remedy, that we are naturally rendered cautious in crediting its marvellous powers. The apparatus of Professor Keil is minutely described, and it appears in many points to possess advantages over the first machine invented by Professor Faraday in 1833. The remarkable properties of electro-magnetism have been admirably illustrated by Professor Oersted of Copenhagen, while the weaker agency of the magneto-electric rotation apparatus of Faraday, has been but little attended to by medical men in England. To the speculative Germans, however, the mystic powers of the magnet combined with those of electricity presented too inviting a field for investigation, to be neglected; and we accordingly find that Dr. Wetzler is but one of several physicians who have availed themselves of these properties for the cure of disease. The apparatus of Professor Kiel has been employed in the following disorders:

In affections of the organs of sight. Three cases are given, two of which are far from satisfactory; the third was a case of rheumatic amaurosis of the right eye, in consequence of a sabre cut, which had injured the organs of sight. After three or four applications of the remedy, the sight, which was previously almost entirely lost, improved, and after twelve sittings the patient was enabled to distinguish the window panes at the distance of fifteen feet, but a complete cure was not effected.

Organs of hearing. Of nine cases of deafness, most of which appeared to be connected with abdominal plethora, only two could be reported completely cured, the other patients, though relieved for a time, either lost faith in the prolonged administration of the remedy, or were obliged to leave the baths before a cure could be effected. Such at least appears to be Dr. Wetzler's conviction, but we should not forget that most of his patients whose cases are recorded in this work employed at the same time regularly the mineral waters of the baths, at which he was exercising his magic skill.

Neuralgia. The cases of neuralgia are related at great length, and four or five out of twenty appear really to have been completely cured by the magneto-electric apparatus. But was this a greater number than those who are relieved of severe nervous pains by galvanism, or by other well known remedies?

Dr. Wetzler is free from one great fault of writers of his description; he does not hesitate to record a failure in his practice; and indeed, on analysing carefully the ninety cases he has given to the world, we find

that want of success attended not a few. In muscular *rheumatism* his practice was attended with considerable success, but in such cases we find that he only relates his experience of the *immediate* relief his patients obtained from his apparatus; for the majority after returning to their homes, forgot or neglected to transmit to him any account of their future history.

According to our author there are few of the disorders connected with, or arising from impaired digestive powers, which may not be benefited by magneto-electricity; but, if some improvement be not manifested after three or four trials, he does not recommend the continuance of the remedy.

The description of Professor Keil's apparatus is too long for insertion in this brief notice, and this is the less to be regretted, as the author acknowledges that it is greatly exceeded in efficacy and in cheapness, by the machine lately invented by Professor Neef, of Frankfort on the Maine. But of this latter he gives no detailed account, only mentioning his own astonishment at witnessing the powers of the apparatus, and, moreover, that the cost of it (£4 to £6) will place it within the reach of many to whom the more expensive machine of Professor Keil was unattainable.

To conclude, Dr. Wetzler's book contains some useful information, alloyed with much that could only arise from a great deal of credulity: nor can we wonder that the man who was himself restored to health by following the revelations of a somnambulist, should lend a willing ear and ready hand to the little known and mysterious agency of magnetism and electricity.

Art. VI.—*Clinical Remarks on certain Diseases of the Eye, and on Miscellaneous Subjects, Medical and Surgical, including Gout, Rheumatism, Fistula, Cancer, Hernia, Indigestion, &c. &c.* By John Charles Hall, m.d. of East Retford.—*London*, 1843. 8vo, pp. 228.

Dr. Hall appears to have followed the plan of keeping a common-place-and-case-book, in which he has entered, from time to time, such extracts from the authors he has perused, on a variety of professional subjects, as struck him to be interesting, along with such cases, occurring in his practice, as seemed to be remarkable. Nothing can be more laudable than thus recording the results of his reading and experience, on diseases of the eye, gout, rheumatism, fistulæ, cancer, hernia, indigestion, &c., but the propriety of publishing such a collection of extracts and notes of cases, may fairly be doubted.

As a sample of the work, let us take chapter viii. It consists of two cases of uterine hemorrhage, in which the placenta was situated over the os uteri; the one treated by the author, by introducing the hand, and turning, and the other by some other practitioner not named, managed in a similar way, except that turning was not required as the breech presented; to which two cases are added a third, in which the result was fatal, copied from Dr. Merriman's works, along with scraps from Burns, Lee, and other well-known authors. Now, this is very good as a specimen of Dr. Hall's common-place-book, very useful for him to refer to,

when similar cases occur in his practice, but possessing neither interest nor novelty sufficient to be put into print.

The coming before the public as an author is a very serious step; for if the experiment is premature, unnecessary, or without value, a published work can only be referred to as a proof of ill-judged forwardness on the part of the author. Dr. Hall, we doubt not, is deficient neither in powers of observation nor in those of reflection. Far better then, to wait a few years, exercise those powers in the various interesting professional subjects which might come before him, and then give to the public the well-concocted result, than to offer them such a collection of shreds as the present.

Rather more than the first half of the volume is devoted to diseases of the eye, the treatment recommended being on the whole judicious. Dr. Hall entertains a high opinion of mercury, and especially of the bi-chloride, an alterative, which, he says, " is not, even yet, used so extensively as it ought."

"It can be given in solution, which is a considerable advantage, rendering its action, much more certain, more equal, and by readier absorption, probably more effectual in producing an alterative influence upon the whole system." (p. 14.)

"It is also worthy of note that this medicine may be continued, in uninterrupted use, for a very considerable period, without obvious injury or inconvenience, and in certain cerebral or spinal disorders, a long unbroken course of this preparation is of singular avail." (p. 15.)

The following remarks on opacities of the cornea are judicious:

"It is very important to distinguish, as nearly as possible, those opacities of the cornea which are likely to be removed by the unaided efforts of nature, from those which cannot be dispersed without the assistance of art, and also to ascertain the proper season for commencing the use of remedies, for, if there be any external inflammation, an irritable state of the eye or of the general health, or if the opacity be the result of chronic corneitis, it would be unadvisable to apply local stimulants until these affections are removed." (p. 59.)

We quite agree with Dr. Hall, in the ridicule and contempt he throws on the squint-clippers, who, " without experience, without reflection, without a day's examination of the merits or results of the operation, positively employed their pupils and others to kidnap patients into their operating room." (p. 97.) The ignorance of some of those persons was equalled only by their impudence and brutality. Dr. Hall does not appear to have been very successful in cutting for strabismus. The cause of this appears to be his confining the operation generally to the worst eye only, and leaving the less affected eye untouched.

Dr. Hall is of opinion that the operations for cataract are more hazardous during cold and damp weather. He supports this notion by the authority of Mr. Tyrrell, who, he says, operated for extraction only from October to March. Surely, this is a typographical mistake.

On points of history, Mr. Hall is generally wrong; as, for instance, where he ascribes the notion, that in purulent ophthalmia the cornea dies from the pressure caused by the chemosed conjunctiva, to Mr. Middlemore, (p. 56;) the first demonstration of the advantage of mercury in iritis, to Dr. Farre, (p. 101;) &c. &c.

Dr. Hall tells us, that he has no intention to lay claim to anything that is new in the treatment of fistula. Then, why trouble us with a

school-boy treatise, made up of the opinions of Pott, Samuel Cooper, Brodie, Liston, and Roux?

No sound reason is offered for the following extraordinary dictum, regarding the operation for cancer of the breast. Dr. Hall speaks unfavorably of the success of the operation, being of opinion that in the great majority of cases the disease reappears. "Yet, it is our duty to operate," says he, "even under unfavorable circumstances." (p. 181.)

Art. VII.—*An Essay on the Nature and Treatment of Apoplexy.* By M. Gay.—*Paris*, 1808. Translated by Edward Copeman, Surgeon. *With an Appendix.—London*, 1843. 8vo, pp. 94.

Mr. Copeman, after having been struck with the erroneousness of the too common practice of bleeding in all cases of apoplexy without regard to the concomitant symptoms, and having found the advantage of substituting emetics in cases where the attack had followed a full meal, and purgatives where there was no other indication—met with a treatise on Apoplexy published in Paris in 1808, and has translated it. M. Gay first combats with much warmth and intelligence the views of Portal, the authority of that day, who recommends bleeding in apoplexy; and then proposes to prove that *no* such disease as sanguineous apoplexy exists, that bleeding is injurious in *all* cases of apoplexy, and that emetics are indicated in *every case* because the primary cause is *always* in the primæ viæ. This is proved by reasoning, the force of which we confess our inability to understand; and having seen clots of blood in the brain, having witnessed the occasional good effects of bleeding, and having seen a large clot in the brain following the act of stooping, we should suspect a fallacy, even in an argument which seemed logically to prove that no such things can happen. Emetics have been recommended by the highest authorities, and when the attack follows a full meal, the administration of an emetic is undoubtedly judicious; and there are few cases we suspect where free purging is not indicated. That bleeding is too indiscriminately used cannot be doubted, and it is not improbable that it is often carried so far as to deprive nature of her power of remedying the injury which has already taken place. There are no cases in which bleeding is more frequently resorted to on account of the name of the disease, and not from the actual symptoms of the particular case, always an empirical proceeding, and here much encouraged by the wishes of lookers-on. The difficulty is to lay down rules. Mr. Copeman in an appendix has reprinted two papers on this subject, originally published in the Medical Gazette, which well merit the attention of practitioners. And we cannot but regret that one who could write so judiciously himself should have taken the pains to translate the work of a writer so much his inferior in observation, judgment, and good sense. With such qualities of mind, and with opportunities of seeing disease, we doubt not but that Mr. Copeman may, if he chooses, be of general service by communicating the results of his own observations and reflections on the treatment of many diseases.

Appendix *to the Article on the Medical Jurisprudence of Insanity.*

(Art. V. p. 81.)

We are desirous of communicating to our readers in the present Number the answers returned by the fifteen judges to the questions on the "Plea of Insanity," submitted to them by the House of Lords. It will be obvious that several of these questions arose directly out of the trial of M'Naughten for the murder of Mr. Drummond. The answers were read in the name of all the judges, excepting one, (Mr. Justice Maule,) by Lord Chief Justice Tindal, on the 19th June, 1843.

Question I. What is the law respecting alleged crimes committed by persons afflicted with insane delusion, in respect of one or more particular subjects or persons: as for instance, where at the time of the commission of the alleged crime the accused knew he was acting contrary to law, but did the act complained of with the view, under the influence of some insane delusion, of redressing or avenging some supposed grievance or injury, or of producing some supposed public benefit?

Answer. The opinion of the judges was that notwithstanding the party committed a wrong act, while labouring under the idea that he was redressing a supposed grievance or injury, or under the impression of obtaining some public or private benefit, he was liable to punishment.

Quest. II. What are the proper questions to be submitted to the jury, when a person alleged to be afflicted with insane delusion, respecting one or more particular subjects or persons, is charged with the commission of a crime, murder for example, and insanity is set up as a defence?

Ans. The jury ought in all cases to be told that every man should be considered of sane mind until the contrary were clearly proved in evidence. That before a plea of insanity should be allowed, undoubted evidence ought to be adduced that the accused was of diseased mind, and that at the time he committed the act he was not conscious of right or wrong. This opinion related to every case in which a party was charged with an illegal act, and a plea of insanity was set up. Every person was supposed to know what the law was, and therefore nothing could justify a wrong act except it was clearly proved that the party did not know right from wrong. If that was not satisfactorily proved the accused was liable to punishment; and it was the duty of the judge so to tell the jury when summing up the evidence, accompanied by those remarks and observations which the nature and peculiarities of each case might suggest and require.

Quest. III. In what terms ought the question to be left to the jury as to the prisoner's state of mind at the time when the act was committed?

No answer was returned to this question.

Quest. IV. If a person, under an insane delusion as to existing facts, commits an offence in consequence thereof, is he thereby excused?

Ans. If the delusion were only partial, the party accused was equally liable with a person of sane mind. If the accused killed another in self-defence he would be entitled to an acquittal, but if the crime were committed for any supposed injury he would then be liable to the punishment awarded by the laws to his crime.

Quest. V. Can a medical man, conversant with the disease of insanity, who never saw the prisoner previously to the trial, but who was present during the whole trial and the examination of all the witnesses, be asked his opinion as to the

state of the prisoner's mind at the time of the commission of the alleged crime, or his opinion whether the prisoner was conscious, at the time of doing the act, that he was acting contrary to law? or whether he was labouring under any, and what delusion at the time?

Ans. The question could not be put in the precise form stated above, for by doing so it would be assumed that the facts had been proved. When the facts were proved and admitted, then the question, as one of science, would be generally put to a witness under the circumstances stated in the interrogatory.

Mr. Justice Maule agreed with the judges in respect to the answers returned to all the questions excepting the last; from this he entirely dissented. In his opinion, such questions might be at once put to medical men without reference to the facts proved; and he considered that this had been done, and the legality of the practice thereby confirmed on the trial of M'Naughten.

The present state of the law of England, relative to the plea of insanity in criminal cases may be considered to be embodied in the foregoing replies. To us it appears that the question is left much in the state in which, before these queries were put, it was known to exist. These queries embrace all the difficulties suggested in the article on the Medical Jurisprudence of Insanity; we wish we could say that the replies had removed them. One point has been satisfactorily elicited, namely, that there is great unanimity in the views entertained on this subject by our high law authorities; and in future therefore we anticipate there will be no reason to complain of conflicting decisions in the trial of these cases.

From the reply to the first question we learn that the existence of delusion in the commission of a crime will not excuse it, if the party knew he was acting contrary to law. It is, we think, obvious that this answer admits of a very wide construction; for how is it to be determined, that a man committing an act while labouring under a delusion knew that he was acting contrary to law? Applying the principle here set forth to the case of Martin, the incendiary of York cathedral, it is pretty certain that he was liable to punishment, and yet he was acquitted. Applying it to Mr. M'Naughten's case, we must infer that the accused did not know that in shooting Mr Drummond he was acting contrary to law,—the very point on which so much dissatisfaction with respect to the verdict arose. Hence in all similar cases a like inference may be drawn.

The reply to the second question brings before us the old test of insanity in the perpetration of crime, namely, the consciousness of right or wrong in the mind of the perpetrator at the time of its commission. We have elsewhere remarked upon the insufficiency of this test; and, therefore, it does not require to be further considered in this place. The reply to the fourth question shows that partial delusion does not excuse a person who has committed a crime, except in those cases where a sane man would be equally excused. It would appear further, that if murder were committed for an imaginary injury, the fact of the person labouring under partial delusion as to existing facts would not excuse the crime. To apply this to M'Naughten's case, we find that there was here "a supposed injury," as he considered the deceased to be one of his persecutors, Therefore, in order to reconcile the verdict with this legal doctrine, we must infer that M'Naughten's delusion was not partial but general.

We have elsewhere expressed our opinion as to the form in which questions should be put to medical witnesses; and that view is in the reply to the fifth question, borne out by the opinions of fourteen judges out of fifteen. It is now decided that facts tending to lead to a strong suspicion of insanity must be proved and admitted before the opinions of medical witnesses can be received.

PART THIRD.

Original Reports and Memoirs.

REPORT ON THE NEW TEST FOR ARSENIC,

And its Value compared with the other Methods of detecting that Poison.

By Alfred S. Taylor,
Lecturer on Chemistry and Medical Jurisprudence at Guy's Hospital.

There is no branch of chemical toxicology which has received so much attention as that which relates to the processes for detecting arsenic in simple solutions, and in solids or liquids of an organic nature. The ingenious discovery of Mr. Marsh in 1836 threw open a new field of investigation; and by means of his apparatus, arsenic was detected in substances in which it was not even previously suspected to exist. In adopting processes for fixing the arsenic, Orfila, Danger, and Flandin, succeeded by this apparatus in detecting arsenic in cases of poisoning in the blood, liquids, and solid organs of the body. Experimentalists were, however, induced to rely upon minute stains in deposits, insusceptible of having their nature verified by other chemical processes, and this led to numerous mistakes. Thus arsenic was pronounced to be a natural constituent of the human body; it was said to exist naturally in the bones, and probably in the muscles, and in the soil of cemeteries; but more accurate analyses have since shown that these conclusions were too hastily drawn, and that we do not find arsenic in the body except in those cases where the person has died from the effects of the poison. On the other hand, it was objected to Marsh's test, that in no case could it be relied on as furnishing conclusive evidence of the presence of the poison, because the materials employed generally contained traces of that substance. Indeed it has been asserted by some that no specimen of zinc can be procured entirely free from arsenic; an assertion, however, in the truth of which, in a practical view, it is impossible to agree. Zinc may be procured so pure as to indicate, when used in Marsh's tube, not the slightest trace of the presence of arsenic; and although by using larger quantities of the metal, and employing more delicate methods of testing, some minute portion of arsenic might be found to be present in it, it is obvious that this cannot affect the practical bearing of the question. We have really to consider how far these infinitesimal traces of arsenic, admitting them to be present, may form an objection to Marsh's test, as it is commonly applied? Most toxicologists have satisfied themselves that with proper precautions all such objections may be removed: but it cannot be denied that unless the purity of the materials be carefully looked to, serious fallacies may arise; and that it would not be proper in any medico-legal case to rely simply upon a deposit on glass as evidence of arsenic being present. The real nature of the deposit requires to be confirmed by other experiments; for the physical characters are fallacious; and however the deposits from antimony may be distinguished from those of arsenic, the stains produced by selenium so closely resemble the minute stains from arsenic as to be very easily mistaken for them. It is, therefore, satisfactory that by the late discovery of Hugo Reinsch it is now in the power of the analyst readily to separate arsenic in the metallic state from organic and other liquids, and in the course of a few minutes to transform this into arsenious acid, and render it fitted for the application of sulphuretted hydrogen and other well-known tests for the poison.

Reinsch remarked that a slip of bright metallic copper which he had placed in common muriatic acid became in a short time coated with a metallic film of an

iron-grey colour; and when thus coated the metal resisted for a short time the action of strong nitric acid, although it was slowly dissolved like ordinary copper. On examining the muriatic acid by sulphuretted hydrogen gas, it was found to contain arsenic in the proportion of about 0·16 per cent. of the metal. This led to the idea that copper and muriatic acid might be made a means of separating arsenic in the metallic state from liquids. When metallic copper, previously cleaned by nitric acid and polished by friction, was placed in a close vessel containing arsenical muriatic acid of s. g. 1·172, the deposit took place slowly; and it was only after the lapse of several weeks that the film of arsenic deposited on the copper presented a bright metallic lustre. When the acid was diluted with its weight of water and the mixture exposed to the air the deposit took place in a few hours. On heating the acid liquid, whether concentrated or diluted, the deposit occurred immediately. A grey pellicle was formed; but if the arsenic was in large quantity it became dark coloured at the boiling point, and then separated in black scales. It was found that this precipitation of arsenic on copper took place, although more slowly, when the arsenical compound was in extremely minute proportion; and that it would separate the metal even where a current of sulphuretted hydrogen gas failed to detect its presence.

It was next observed that when copper was boiled in a solution of arsenious acid there was no arsenical deposit formed upon it; but that if one or two drops of muriatic acid were added the copper immediately assumed the aspect of iron. According to Reinsch a quantitative analysis of arsenic might be thus made; for as the metal is separated from the copper by long boiling in the acid liquid, the quantity might be determined by the loss: to this, however, there is the objection that a quantity of chloride of copper is at the same time formed. By the use of copper, the whole of the arsenic, contained in an impure specimen of muriatic acid, may be separated from it. There are various processes by which the arsenic may be removed from the copper, as in dissolving it off by nitric acid,—heating the copper coated with arsenic in a closed tube, whereby the metal is sublimed into crystals of arsenious acid, or heating it in a tube through which a current of hydrogen is passing, in which case the arsenic is sublimed in its metallic state.

There are certain objections to this mode of testing. 1. *Antimony.* This metal affects this test as it does that of Marsh. Antimony is precipitated on copper under the same circumstances as arsenic, and here again no effect takes place until muriatic acid be added. Reinsch, however, states that antimony may be known by the metallic film having a rich violet colour, and the only case in which the arsenical deposit assumes the same tint is where the poison exists in very minute proportion. It will be seen presently that this criterion is not so well marked as the statement of Reinsch would lead us to suppose. *Tin.* Copper underwent no change in contact with a solution of this metal; mixed with a large quantity of muriatic acid until it was warmed, when there were faint traces of a metallic precipitate. Diluted solutions produced no effect whatever. *Lead.* The results obtained with this metal were much the same as those procured with tin. On boiling a salt of lead with muriatic acid there was no metallic deposit on the copper. *Bismuth.* When copper is boiled in a solution of subnitrate of bismuth, mixed with its weight of muriatic acid, a gray metallic deposit is formed on the metal, and on heating the mixture bismuth is deposited on the copper in the form of a crystalline efflorescence, whereby the bismuthic deposit may be known from that produced by arsenic and other metals. *Mercury.* On boiling copper in a weak solution of corrosive sublimate it is speedily covered with a silvery white pellicle, which by the microscope may be easily recognized as consisting of globules of mercury. *Silver.* This metal was found to give a crystalline metallic deposit on copper; although the addition of muriatic acid occasioned the separation of the greater part of the silver as chloride. From the above experiments Reinsch concluded, 1, That metallic copper was the most certain and sensitive test for arsenic, and that the reaction was very evident when the arsenic formed only $\frac{1}{200000}$ part of the solution. 2. That with regard to other metals deposited on copper under similar circumstances, these deposits might be easily distinguished from that of arsenic by their physical or other characters.

In carrying on his experiments he found that the separation of arsenic by means of copper was so complete that Marsh's test failed to show the presence of the smallest trace of arsenic in the residuary liquid. Bismuth and antimony were the only two metals deposited on copper which were likely to be mistaken for arsenic; the bismuth was always (?) deposited in a crystalline form while the antimony had a violet tint in diluted and a white or grey colour in concentrated solutions. M. Reinsch ascertained that copper would detect arsenic in a smaller proportion than he had at first supposed; in short, he found that one millionth part of a grain of arsenious acid contained in a liquid, when boiled with muriatic acid, produced a thin arsenical deposit on copper in from a quarter to half an hour. As a common experiment, a slip of bright copper was similarly treated with muriatic acid only, but it underwent no change. The surface of metallic copper is, however, apt to acquire a tarnish or dark colour, probably from the formation of some chloride of copper when left in the acid for some hours and exposed to the air. This, however, could not be mistaken for an arsenical deposit, since the latter is always completed within half an hour.

In liquids containing organic matter this separation was found to be equally complete and satisfactory. Half a grain of arsenious acid was mixed with potatoes, milk and broth, and the whole was digested with pure muriatic acid diluted with its weight of water. The liquid was boiled and filtered, and slips of copper were introduced into the filtered portion; they were immediately covered with arsenic, the deposit taking place as readily as if the poison had been dissolved in pure water. The arsenic was then converted into arsenious acid by heating the slip of copper in a tube, about thirteen inches long, drawn out to a fine point at one end. This conversion was readily effected by gently breathing into the tube at the time the heat was applied. The arsenious acid thus reproduced was dissolved in water and the usual tests applied.

Among the obvious advantages of Reinsch's test for arsenic are, 1, its extreme simplicity and the consequent facility with which it may be applied; and 2d, the rapidity with which an analysis of the most complex solid or liquid may be performed. Indeed the analysis of the contents of the stomach of a person, who is suspected to have died from arsenic, may now, as it were, form part of the post-mortem examination. Having repeated the whole of Reinsch's experiments, and applied the test in other ways, I shall here give the results.

It is not necessary to dissolve arsenious acid by long boiling in water in order to separate the arsenic in the metallic state by means of copper. If we place half a grain or a grain of the white powder in a small quantity of water, and boil in it a piece of polished copper foil, there is no effect; but if one or two drops of strong and pure muriatic acid be added the arsenic is instantly deposited, and when entirely coated, the copper should be removed, washed in water, and dried. A quantity of metallic arsenic may be thus collected, and this may be converted into distinct but small octohedral crystals of arsenious acid by heating the coated pieces of copper in a tube about four inches long and a quarter of an inch in diameter; the tube should be closed at one end and the heat gradually applied. The arsenious acid thus formed may be dissolved in water and tested with sulphuretted hydrogen gas and ammonio-nitrate of silver. The arsenites, the arseniates, arsenic acid, orpiment, and in short all arsenical compounds, were thus examined with equally satisfactory results; each experiment not occupying more than a few minutes, and the quantities used being extremely small.

Instead of copper foil, copper wire, and in some experiments the finest copper gauze was employed; but in no case were the effects so clear and decisive as with thin copper foil cut into pieces, about an inch long and the eighth of an inch in width. On boiling the copper with dilute nitric or dilute sulphuric acid, and adding a solution of arsenious acid, there was no deposit formed on it; hence the test will serve to distinguish muriatic acid from the nitric and sulphuric, by boiling the suspected acid with metallic copper and arsenious acid. The result is not always satisfactory, if the muriatic acid be in very small proportion. A chloride

may be also thus distinguished from other salts; for as yet it does not appear that there is any acid but the muriatic, which will lead to the deposit of arsenic on copper.

A strong objection to the materials used in Marsh's test is, that they are apt to contain arsenic. The copper test will here assist us in determining their purity. I have tried in this way several specimens of zinc and sulphuric acid: there was no deposit on the copper, but when a minute fractional quantity of arsenious acid was added, the surface became instantly dull, and a film of metallic arsenic appeared. Again, it is possible to determine on the spot whether the matters vomited by a person from the stomach do or do not contain arsenic. A few drops boiled with muriatic acid and copper would enable the practitioner to determine the question, and to decide on the progress of the case. The same experiment might be performed with the matters evacuated. So the efficacy of the so-called iron antidote may be in this way tested. If the arsenic be not found in the supernatant liquid, when the hydrated oxide of iron has been mixed with a clear filtered solution of arsenious acid, it may be discovered by boiling a few grains of the oxide in diluted muriatic acid and adding metallic copper. According to my observation, the hydrated oxide of iron has no effect upon arsenious acid when in a state of coarse or fine powder, the state in which it is most commonly taken as a poison: it precipitates the arsenious acid in an insoluble form only, when added in very large quantity to a clear filtered aqueous solution of that substance. Reinsch's test will enable those who treat cases of poisoning to determine readily whether or not the oxide of iron be readily combined with any portion of arsenic, when expelled from the stomach.

It is right to examine the objections which may be fairly advanced against this mode of testing. In the first place, the only materials used are muriatic acid, and metallic copper; and the only one which may be supposed to present any difficulty in respect to the admixture of arsenic as impurity, is *muriatic acid.* This therefore should be taken absolutely pure, and the proper way of testing it for arsenic would be to dilute it with two parts of water, and boil a portion of copper in it before trying the suspected liquid. *Antimony.* In a diluted solution of an antimonial salt, the deposit of antimony is known from that of arsenic by its violet colour, but the arsenical deposit may have this colour also, where the quantity of poison is extremely small; and I have observed that in boiling copper with the chloride of antimony, the grey deposit was almost identical with that of arsenic. So in experimenting with the acid nitrate of *bismuth*, the deposit on copper was very similar to that of arsenic: it possessed none of the crystalline characters ascribed to it by Reinsch. The protochloride and perchloride of tin gave a dark gray deposit with a purple tint, without rendering it necessary to add muriatic acid. The common sesquichloride of iron gave also a dark gray tarnish, and with nickel there was no effect. It may be proper to state, however, that on immersing a slip of polished copper in a diluted solution of a salt of nickel, it appears to acquire instantly a bright silvery coating. This is owing to an optical effect; since the red colour of the copper neutralizes the green colour of the liquid, and causes the reflection of white light; hence, on removing it the copper is seen to possess its usual red lustre. Cadmium produced no effect; copper was boiled in a solution of the nitrate to which muriatic acid had been added, and also in a solution of chloride made by dissolving the pure oxide in muriatic acid. In neither case, although the metal was left in the liquid for upwards of half an hour, was there any change. Here then we have an important difference between cadmium and arsenic: these metals resembling each other strikingly in the fact, that they yield metallic sublimates when their oxides are heated with carbonaceous matter, and yellow sulphurets, when their compounds are treated with a current of sulphuretted hydrogen gas. In respect to silver and mercury, the metallic deposits are formed without the addition of muriatic acid; although in a very diluted solution of a mercurial salt, the deposit would take place very slowly, unless muriatic acid were added.

The appearance of the deposit in each case, is very different to that formed by arsenic. Lastly, the alkaline sulphurets, or any liquid containing sulphuretted hydrogen or sulphur evolved in decomposition, will give a tarnish to copper, varying in tint, which might be easily mistaken for an arsenical deposit where the quantity of arsenious acid present, was small. Indeed, in several experiments specially made, it was impossible by reference to physical characters only, to distinguish the stain produced by the sulphuret from that formed by arsenic. There is, however, this marked difference that the tarnish from sulphur takes place sooner or later, on mere contact with the liquid, or on warming it without the addition of muriatic acid; while in the case of arsenic, the addition of this acid is absolutely necessary or no deposit will take place. There are besides other and ready means of discovering the presence of the smallest traces of a sulphuret as of sulphuretted hydrogen gas in a liquid.

The only real objections to this test, therefore, refer to the possible deposit on coppers of other metals or substances which might be mistaken for arsenic. The above facts will, it appears to me, justify the conclusion that, from physical characters alone, it would be hazardous to pronounce on the presence of arsenic in a suspected unknown liquid. But here we come to the extreme value of this most useful test; the arsenic may be easily obtained from the copper by heating the metal in a reduction tube. We may procure it under the form of crystalline octohedra or of a white sublimate, easily dissolved by water in sufficient quantity for the application of the silver and sulphuretted hydrogen tests. The obtaining of octohedral crystals is highly characteristic of arsenious acid; since there is no other volatile poison which crystallizes in a similar form; and the octohedra of arsenic, from their great brilliancy, are visible either to the naked eye, or under a lens of moderate power, when existing in imponderable quantities.

The simple answer to all objections founded on the deposit of other metals or substances on copper is, that it is impossible to obtain by heating the metal, octohedral crystals possessing the well-known characters of arsenious acid. The limit to the practical value and application of the test then, is that point where, with a metallic deposit on copper, we are unable to procure by heat a body possessing the properties of arsenious acid. Beyond this, there may be chemical probability of the presence of arsenic from the appearance of the deposit; but it appears to me doubtful whether we should be justified in saying that we had medico-legal certainty. This admission, however, must not lead us to suppose that the test is very limited in its application; on the contrary, it will detect and separate a smaller quantity of arsenic than we shall probably ever have to encounter in a medico-legal analysis; and it is far more easy to obtain arsenious acid from a minute quantity of the metal than to obtain the metal from the reduction of a minute quantity of the sesqui-sulphuret, according to the process commonly adopted. Further, there is as much certainty with this as with Marsh's test, or other modes of testing for arsenic; and where it fails, it is pretty certain that no other test will succeed.

The most valuable application of the copper test will, however, be seen in the detection of the presence of arsenic in the most complex organic mixtures containing that poison. Medical jurists have long abandoned the use of the liquid tests in these cases; and they have relied either on Marsh's apparatus, or on the precipitation of the arsenic in the state of impure sesqui-sulphuret, and the subsequent reduction of that compound to the metallic state by flux. In the use of Marsh's apparatus, much inconvenience has been experienced from the froth; and when the quantity of arsenic was small, its diffusion through a large quantity of liquid rendered its separation troublesome. The processes of carbonization suggested by Orfila and Danger, and Flandin, removed this difficulty to a certain extent; but these processes required time and care to prevent the loss of arsenic. There could be no doubt, however, as it has been elsewhere stated, that Marsh's process was a considerable improvement upon the old plans adopted for the separation of arsenic. With respect to the precipitation of the poison by sulphu-

retted hydrogen, there were many difficulties which the experimentalist had to encounter. The precipitation was slow, the sulphuret was commonly mixed with much organic matter, so that on attempting to obtain from it the metal, the sublimate was often obscured by the foreign substances with which it was mixed. If the proportion of sesqui-sulphuret were small, it could not be wholly separated from the liquid; or if it were thus separated, the quantity was not sufficient to yield a distinct and well-defined sublimate of metallic arsenic. In short, those who have tried both processes must have perceived that there were many inconveniences connected with them. The following experiments will show that the copper test supplies all that is required to render the search after arsenic simple, speedy, and certain.

The $\frac{1}{141}$th part of a grain of arsenious acid was mixed with two fluid drachms of milk. The mixture was boiled with a few drops of muriatic acid, and a slip of copper was introduced. In less than a minute the metal was coated with a grey film of metallic arsenic. Several pieces were thus coated; they were washed in water, dried in the heated current of air over a spirit lamp-flame, and introduced into a small reduction tube. On applying a gentle heat to the copper, octohedral crystals were obtained; visible to the eye in the light of the sun, but plainly distinguishable with a lens of low power. The crystals dissolved in water gave the usual reactions with the ammonio-nitrate of silver and sulphuretted hydrogen gas. In a second experiment, the same quantity of arsenic was mixed with two drachms of porter; in a third, a like quantity with two drachms of gruel, and the copper test was applied with similar satisfactory results. Each experiment was completed in about five minutes. Brandy, containing a poisonous impregnation of arsenic, was then tested; and the arsenic readily separated. A few drops taken from a bottle of port wine, containing arsenic, and which had nearly caused the death of three persons about four years ago, were next submitted to the test, and the metal readily obtained. In organic solids the results were equally striking: a few grains of a cake containing arsenic, which had been used in an attempt to poison, were boiled in a small quantity of distilled water; and muriatic acid and copper added, when metallic arsenic was immediately precipitated. The contents of the stomach of a person who was poisoned by arsenic in February, 1834, which had been loosely exposed, and allowed to become decomposed during a period of upwards of nine years, were next examined. A few drops of the thick turbid liquid were placed in a tube and boiled with dilute muriatic acid and water; the copper introduced was covered with a bright layer of metallic arsenic. The contents of three other stomachs taken from persons who were poisoned by arsenic in 1835, 1838, and 1840, gave precisely similar results. In the last case, the whole of the contents with the food at the time contained in the organ, had been evaporated to a dry solid mass; a few grains of this were sufficient to furnish a clear demonstration of the presence of arsenic by the aid of the test. In the conversion of the metal to arsenious acid, it will at once suggest itself that if octohedral crystals should not be obtained by heating one portion of copper, several slips should be introduced together or successively. In all these cases, arsenic would have been discovered by the application of Marsh's test, or sulphuretted hydrogen gas; but the process would have occupied a much longer time, and with regard to Marsh's test the metallic arsenic could not have been so speedily converted to arsenious acid in a form convenient for the identification of its properties.

There is no doubt that this is an extremely delicate test; but there is a want of precision about the statements of M. Reinsch on this subject. He asserts that he has been able to detect by means of it $\frac{1}{1000000}$th of a grain of arsenious acid. It appears, however, probable that by this we are to understand he separated the arsenic by copper, where it formed only one millionth part of the solution by weight. In order to obtain the millionth of a grain of arsenic, it would be necessary to take *one drop* of a solution formed by dissolving one grain of arsenic in sixteen gallons of water, or their equivalent fractional parts. In these estimates it should always be clearly stated, not only what the absolute weight of the poison

may be, but the actual quantity of water in which it is diffused; for chemical tests cease to act from two causes; first, from the very minute quantity of poison present; and secondly, from the large quantity of menstruum in which it may be diffused. In proof of this it may be stated, that if the same weight of arsenic be dissolved in two different quantities of liquid, a test which will clearly indicate its presence in the one case, will fail to do so in the other. Thus in one experiment the $\frac{1}{400}$th grain of arsenious acid, dissolved in twenty drops, or 8000 times its weight of water, was immediately affected by a current of sulphuretted hydrogen gas; but upon applying the test to $\frac{1}{400}$th grain dissolved in four drachms or 100,000 times its weight of water, the liquid scarcely acquired any perceptible tinge; and had the dilution been carried to a million times, there would have been probably no visible change whatever. The same rule applies to other tests, and when, therefore, we speak of a test detecting or separating the millionth part of a grain of arsenic, the whole of the circumstances should be explicitly stated. The delicacy of Marsh's test has often been estimated by the probable or assumed weight of the sublimate obtained; but there appears to be an error in this mode of estimating the relative efficacy of tests; because it is quite certain that a deposit of arsenic may be procured by Marsh's apparatus, which, if it were transferred to the tube, would give no indication whatever of its presence from its extremely minute quantity. The truth is, in this case we are operating upon the whole quantity of poison, dividing and subdividing the metal into a series of infinitesimal deposits, the weight of some of which might not be equal to the millionth part of the weight of the poison which is furnishing them.

The results which I obtained with respect to the tests for arsenic are that the sulphuretted hydrogen gas begins to produce a reaction indicative of arsenic, when the weight of arsenious acid is not more than $\frac{1}{4000}$th part of a grain in ten drops of water; the arsenic thus forming $\frac{1}{40000}$th part of the liquid. With the $\frac{1}{2000}$th grain, and a smaller quantity of water, the effect was much more decided. With respect to Marsh's test, as the poison was mixed with the whole of the acidulated liquid, the dilution was disproportionately great. No well defined deposits were procured with $\frac{1}{2000}$th of a grain in two ounces of water; but faint brown rings began to appear where, with the same quantity of water, the proportion of arsenious acid reached $\frac{1}{1000}$th of a grain. In the use of Reinsch's test, we are not obliged to dilute the liquid with more water than may be just sufficient to cover the copper; and there is no loss in these minute quantities, a circumstance which necessarily happens with Marsh's test, during the combustion of the arsenuretted hydrogen. The copper test failed to detect $\frac{1}{400}$th grain in thirty drops of water, the dilution being equal to 120,000 times the weight of the arsenic. The deposit commenced with a faint violet coloured film, when the quantity of arsenious acid was equal to $\frac{1}{3000}$th grain in thirty drops of water, or under a dilution of 90,000 times its weight; it was also very decided with the $\frac{1}{2000}$th of a grain in the same quantity of water, but in neither of these cases could octohedral crystals of arsenious acid be obtained. The following experiment will show how much this test is influenced by dilution: the copper was coated in a few minutes when boiled in a solution containing $\frac{1}{4000}$th grain of arsenious acid in ten drops of water, although the test had completely failed to detect the same weight of arsenic, in three times that quantity of water. There is no doubt that by preventing the dilution of the poisonous liquid, Marsh's apparatus would detect a much smaller quantity of arsenic than has here been stated. This has been contrived by Mr. Marsh, in inclosing the liquid for analysis in a small piece of glass-tube, distinct from the general liquid of the apparatus, which acts by confining the gas.

The question really is, however, At what point do the tests fail to detect the poison with certainty? And the reply appears to me to be when the results obtained by these modes of testing cannot be confirmed by other experiments applied to them. To prove that the substance deposited is arsenic, we ought to be able to procure from it arsenious acid recognizable by some at least of its usual properties. This is very easily obtained from small quantities, by the copper test. I have

found that crystals of arsenious acid might be procured from so small a quantity as from the $\frac{1}{100}$th to the $\frac{1}{150}$th of a grain of arsenious acid. Whether a smaller quantity than this will yield sufficient metal for this confirmatory proof must be settled by further experiments; but it appears to me nothing short of this, whether in reference to Marsh's or Reinsch's test, will afford what the medical jurist requires, judical certainty.

Another useful application of the copper test may be made in the following way. If the arsenic have been thrown down from an organic liquid in the form of impure sesquisulphuret, this may be dried and deflagrated with nitre, or decomposed by nitromuriatic acid, whereby arseniate of potash or arsenic acid will result, and the organic matter will be entirely decomposed. In the case of deflagration by nitre, the surplus nitric acid should be expelled by sulphuric acid, and the arseniate dissolved out of the residue; or if nitro-muriatic acid be used, the liquid may be evaporated to dryness. On boiling either of these products with copper and muriatic acid, the metallic arsenic will be readily procured.

This test will no doubt be fully examined by others. Without saying that it will at once supersede all other modes of testing for arsenic, it appears to me to surpass them in certainty and simplicity, and in its ready application to the detection of the poison in all forms of mixture and combination. In its principle it is rather the reverse of the method which has been hitherto pursued; for by it we procure the metal first, and produce the compound to be afterwards tested by liquid reagents. With this discovery we have now analogous processes for the detection of three important metallic poisons and their salts, namely, arsenic, mercury, and copper. The chief matter of surprise is, that a method so simple should not have been earlier discovered in relation to a poison which has received so much attention from toxicologists; more especially, as it has been long known that arsenious acid was soluble in muriatic acid.

PART FOURTH.

Medical Intelligence.

On the Neutral Azotized Materials of Organization. By MM. Dumas and Cahours.

Many of our readers may be aware that the correctness of several of Liebig's views is impugned by the eminent French chemist Dumas; and that, in regard to some others, a claim for priority is set up by the latter. Into these questions we shall not at present enter, but shall confine ourselves to a statement of the most important results which have been obtained by an extensive series of analyses lately concluded by the authors of this paper. These analyses were made upon a larger scale than most of those which have preceded them, in order that the proportion of azote, which is not only the most important item in the whole, but the one most difficult of determination, might be ascertained with the greatest exactness.

1. *Albumen.* These experiments appear to prove satisfactorily, that albumen possesses the same composition in all animals, and *à fortiori* in all the liquids of the same animal. Vegetable albumen does not differ from animal albumen in elementary composition; but it is not united with free soda, as it ordinarily is in the animal body. There is a much closer agreement between the several analyses made by MM. Dumas and Cahours, of albumen obtained from a variety of sources, than there is between the three analyses of M. Scherer (which have been taken as the standard by Liebig and others) of the albumen of the serum of blood only. This, to our minds, is good evidence of the accuracy of the methods employed. The average composition of five different kinds of animal albumen is as follows:

Carbon	53·42
Hydrogen	7·19
Azote	15·74
Oxygen, sulphur, and phosphorus .	23·65 = 100·00

In no instance was a departure from this average indicated, to the amount of more than ·1 or at most ·2 per cent.,—a degree of conformity extremely remarkable.

2. *Casein.* This substance, when drawn from the milk of herbivorous animals, presents the same composition in each case, and properties nearly identical. The milk of the human female, who approaches the carnivorous quadruped in her habits of life, furnishes a casein which, whilst possessing a composition identical with that of the casein of herbivorous quadrupeds, exhibits peculiar properties, which may perhaps become the groundwork of future distinctions. In some abnormal conditions of human blood, there exists a substance which seems to resemble casein, both in composition and properties, but the authors have not succeeded in detecting it in the blood of animals during lactation. The flour of the cerealia includes a substance which seems analogous to casein, presenting the same elementary composition, and its most important properties. All the varieties of *casein* possess the same elementary composition as *albumen*, and these two substances are certainly *isomeric*.

3. *Legumin.* A remarkable and very distinct substance has been found in the kernel of the almond and other similar fruits, as well as in the leguminous seeds, which has been regarded by Proust, Vogel, Liebig, and others, as identical with animal casein. According to our authors, however, this substance bears a nearer resemblance to gelatin in regard to composition, though it completely differs from it in properties. It deserves peculiar attention, on account of its abundance in the alimentary materials just mentioned, and the part which it seems to take in the nourishment of the animal body. For this substance, when dissolved in dilute muriatic acid, communicates to it exactly the same properties with albumen;

whence it may be inferred that, under the influence of the gastric juice, it will furnish the same soluble products as albumen itself. It appears probable that this substance consists of a mixture or combination of albumen or casein with some other product; but as this mixture appears to be made in constant proportions, there is no inconvenience in retaining for the present the term *legumin*, which was proposed by M. Braconnot for the substance extracted from peas and beans. Legumin, then, in a physiological view, consists of a substance analogous either to albumen or to casein, but mixed and combined with another body richer in azote, which modifies its most important properties. Without doubt, the nutritive power of *legumes* is determined in great degree by the proportion of legumin which they contain; but it would be premature to consider this substance as performing a part identical with albumen or casein. A portion of the elements of legumin are combined in a peculiar state, which probably renders them less adapted for aliment than those which are united in the exact proportions which constitute albumen and casein.

4. *Fibrin.* It seems to be a very definite and positive conclusion from analyses of MM. Dumas and Cahours, that fibrin, instead of being identical with albumen, (as recently maintained by Liebig and his school,) in the proportion of the four elements—oxygen, hydrogen, carbon, and azote, differs from it very sensibly. The following are the results of these analyses, which certainly present a very remarkable accordance.

	Blood of							
	Sheep.	Calf.	Ox.	Horse.	Dog.	Dog A.	Dog B.	Man.
Carbon	52·8	52·5	52·7	52·67	52·74	52·77	52·57	52·78
Hydrogen	7·0	7·0	7·0	7·0	6·92	6·95	7·07	6·96
Azote	16·5	16·5	16·6	16·63	16·72	16·51	16·55	16·78
Oxygen, &c.	23·7	24·0	23·7	23·70	23·62	23·77	23·81	23·48

The dog A had been fed during two months and a half upon meat alone, whilst the dog B had been fed during the same time on bread only.

Upon comparing the average of these analyses with that already given for albumen, it is evident that the proportion of carbon is about ·7 per cent. less in fibrin than in albumen, whilst the proportion of azote is from ·8 to ·9 per cent. greater. The difference is so striking and constant, that it cannot be attributed to accidental causes. A tolerably just idea of the elementary composition of fibrin might be formed, by regarding it as a combination of casein or albumen, and ammonia. This idea would seem, at first sight, to derive confirmation from the fact, that when fibrin (previously well washed) is boiled for a long time in water, the liquid which is distilled over contains ammonia, whilst the insoluble residue has the composition of ammonia. But as fibrin may be dissolved in a solution of caustic potass, without losing its excess of azote, it is not probable that this azote exists in it in the state of ammonia. There can be little doubt that the prolonged boiling produces an actual change of composition. Our readers will bear in mind the fact supposed to be ascertained by M. Bouchardat, that after fibrin has been long boiled, a substance which he regards as analogous to gelatin is found in the water. (See Brit. and For. Med. Rev., vol. XV. p. 229.) It might hence be thought that the excess of azote, and the smaller proportion of carbon, in fibrin, is due to an admixture of gelatin with the albuminous matter of which it chiefly consists, since this difference in elementary composition is characteristic of gelatin. But the fibrin, whilst thus setting free the (so-called) gelatinous matter, *also* disengages ammonia; and, according to MM. Dumas and Cahours, this matter is very unlike true gelatin, either in its composition or its properties; for it does not form a jelly on cooling, and it is precipitated by nitric acid; and it contains a smaller proportion both of carbon and azote, and a considerably greater amount of oxygen. Hence they consider it not improbable that, in the prolonged action of boiling water upon fibrin, there is an actual decomposition of the water and of the animal substance; and that the oxygen of the water fixes itself in the latter, whilst the hydrogen sets free some of its azote in the form of ammonia.

From the general properties of fibrin, it seems certain that this substance contains a large proportion of a product identical with albumen or casein, which it yields to dilute muriatic acid; and that, consequently, it undergoes the same action with the gastric fluid with these substances. Hence, as an alimentary material, it may be classed with them; but as a product of animal life it must be regarded in a very different light, since its physiological properties are far more distinct from those of albumen, than are its chemical ones.

The question naturally arises, whether there is a *vegetable fibrin* equally distinct from vegetable albumen. This also appears to have been resolved by MM. Dumas and Cahours, the former of whom indicated the existence of such a substance in the year 1839. It may be obtained from wheat flour, by using cold or tepid water in the analysis; and its composition *then* resembles that of animal fibrin. But if it be treated with boiling water (as was the case in the analyses of Dr. Jones) it is converted into a substance having the composition and properties of albumen.

5. Independently of these four principal products, albumen, casein, legumin, and fibrin, there are two others which approach them in their mode of action with dilute muriatic acid, so as evidently to appertain to the same group, although their properties seem at first sight altogether distinct. These are *glutin* and *vitellin.* The former is one of the substances extracted from gluten, and is distinguished by its solubility in boiling alcohol, from which it can only be separated by evaporation. The ultimate composition of this substance appears to be the same with that of albumen and casein; but it has certain special properties, and appears to be the matter chiefly concerned in the pannary fermentation. Its full history, however, is yet to be developed. The *vitellin*, which constitutes the albuminous matter of the *yelk* of egg, nearly approaches albumen and casein in its properties; but it differs considerably in composition, a much larger proportion of oxygen being present in it.

The authors agree with Liebig in thinking that these *albuminoid* substances are the only materials from which most of the animal tissues are formed; and that neither the non-azotized substances, nor gelatin, can have any share in their production. They proceed to examine the ultimate destiny of these substances, and to calculate the amount of heat which will be produced by their *combustion*,—a combustion of which they estimate the following as the results:

1 Atom of Albumen, consisting of C. 48, H. 37, Az. 12, O. 15, together with 100 additional atoms of Oxygen	is converted into	C. 6,	H. 12, Az. 12, O. 6, or 3 Urea.
		C. 42,	O. 84, or 42 Carb. Acid.
			H. 25, O. 25, or 25 Water.

All that they say on this subject exhibits the tendency to regard the animal body as a sort of machine, that is, (like a steam-engine,) to gain a certain amount of power by the combustion of a given quantity of fuel, on which we have elsewhere remarked, as fundamentally erroneous.

Annales de Chemie, Dec. 1842.

RESEARCHES ON THE QUANTITY OF CARBONIC ACID EXHALED FROM THE LUNGS OF THE HUMAN SPECIES. By MM. ANDRAL and GAVARRET.

THIS paper gives the partial results of a very extensive series of experiments, on which the authors are engaged. If they continue as they have begun, there can be no doubt that their inquiries will tend greatly to the advancement of our knowledge of the respiratory function, in health and disease. Their first object has been to ascertain the modifying influence of *age*, *sex*, and constitution. To determine this, their observations have been made under circumstances as uniform as possible; the subjects of them having been in good health, and in similar condition as regards food, muscular expenditure, moral state, interval subsequently to the last meal, and hour of the day. Each experiment was repeated several times on the same subjects, and the accordance between the results has been as great as

could be desired in physiological researches. The apparatus employed was so devised, as to enable the respirations to be freely performed; no portion of the expired air was again inspired; and the greatest care was taken to analyse the expired air with accuracy. The observations were continued, in each case, from about 8 to 13 minutes, until about 130 *litres* of gas were collected; and from these, the amount of carbonic acid thrown off per hour is calculated in terms of the quantity of solid carbon it contains. This is done, however, for the sake of comparison only, as it is not supposed by the authors that the means are thus afforded of estimating the whole quantity thrown off in 24 hours,—a question of which they reserve the consideration for the present. The following are the general results already obtained by them.

1. The quantity of carbonic acid exhaled by the lungs in a given time, varies according to the age, sex, and constitution of the subjects.

2. In both man and woman, the quantity undergoes modification according to the ages of the subjects experimented on, quite independently of their weights.

3. In all periods of life, comprised between 8 years and the most advanced old age, man and woman are distinguished by the difference in the amount of carbonic acid exhaled from their lungs in a given time. All other things being equal, man exhales a much larger quantity than woman. This difference is particularly well marked between the ages of 16 and 40 years, during which period the quantity of carbonic acid exhaled is nearly twice as much in man as in woman.

4. In man, the quantity of carbonic acid exhaled, continues to increase regularly from 8 to 30 years of age, and a remarkable increase takes place at the period of puberty. After 30 years, the exhalation of carbonic acid begins to decrease; and this decrease continues, becoming more and more marked, as the individual approaches nearer to extreme old age; so that, at this period, it returns to the standard at which it was about the age of 10 years.

5. In woman, the exhalation of carbonic acid augments according to the same laws as in man, up to the period of puberty; but at that epoch, the increase suddenly ceases; and the amount continues at this low standard, with little variation, as long as the catamenia make their regular appearance. But as soon as they cease, the exhalation of carbonic acid by the lungs undergoes a considerable increase; after which it decreases, as in man, in proportion as the age advances.

6. During the period of gestation, the amount of carbonic acid exhaled is temporarily raised to the standard which it attains after the cessation of the catamenia.

7. In both sexes, and at all ages, the quantity of carbonic acid exhaled by the lungs, is greater in proportion to the strength of the constitution, and the development of the muscular system.

The numerical results contained in the memoir itself have been collected by us in the following table, which expresses the quantity of solid carbon calculated to be exhaled in one hour. The gramme is equal to about $15\frac{1}{2}$ grains.

MALE.		FEMALE.		
8 years	5 grammes.	8 years	5 grammes.	The same standard continues in women during the whole of the menstrual period; but if the catamenia be temporarily suppressed, or pregnancy occur, it rises to the standard it attains after their entire cessation, namely 8·4 grammes.
15	8·7	12-38	6·4	
16	10·8	38-50	8·4	
18-20	11·4	50-60	7·3	
20-30	12·2	60-80	6·8	
30-40	12·2	82	6·0	
40-60	10·1			
60-80	9·2			
102	5·9			

These numbers express the *averages;* the maximum amount is often considerably greater. In a young man of athletic system, and sound constitution, the quantity of carbonic acid exhaled in an hour was 14·1 grammes; a man of 60, equally vigorous for his age, threw off 13·6 grammes; and a man of 63 years,

12·4 grammes. An old man of 92 years, who preserved a remarkable degree of energy, and who had possessed an uncommon degree of vigour in his youth, was found to throw off 8·8 grammes per hour; whilst the same amount appeared to be the usual standard in a man of 45 years of age, who, unlike the preceding, had a feeble system, though in equally good health. The question how far these variations are connected with differences in the capacity of the chest, and in the number of respiratory movements, will be discussed in a future memoir.

Annales des Sciences Naturelles. Fevrier, 1843.

Blood-corpuscles and Spermatozoa of the Camelidæ.

After M. Mandl had discovered the oval shape of the blood-discs of the dromedary and paco, Mr. Gulliver ascertained that those of the vicugna and llama have the same form, (see Appendix to Gerber's Anatomy, pp. 10 and 43.) To complete the history of the blood-corpuscles of this family, Mr. Gulliver has lately given an account of those of the camel (*Camelus Bactrianus*), from which it appears that their average long diameter is $\frac{1}{3123}$d, and their short diameter $\frac{1}{5876}$th of an English inch. (See Proceedings of the Zoological Society of London, Part x. p. 191.)

Thus the corpuscles of this animal are elliptical; and their form, as far as we yet know, is confined to the camelidæ among the mammalia, a very curious anomaly, especially as Mr. Gulliver's observations have shown that even the blood-discs of the marsupialia have the usual circular figure. (See Willis's translation of Wagner's Physiology, Part II. p. 234, *note.*)

At the meeting of the Zoological Society, April 11th, 1843, Mr. Gulliver read an account of the spermatozoa of the camel, confirming that which he had previously given of those of the dromedary, (Proc Zool. Society, Part X. 1842, p. 355,) namely, that they are of the same type as the corresponding animalcules of other mammals. This fact is curious, because, from the singular correspondence of form between the blood-corpuscles of the camelidæ and the corpuscles of the oviparous vertebrata, it might have been supposed that a similar affinity would exist between the essential elements of the semen of these animals.

Blood-corpuscles of the Stanley Musk Deer.

"The average diameter of the blood-discs of the Stanley musk-deer, (*Moschus Stanleyanus*, Gray) is $\frac{1}{10825}$th of an English inch. Hence they are nearly as small as those of the Napu musk-deer, (*Moschus Javanicus*, Pallas.)" (Abstract of a paper read by Mr. Gulliver at the Zoological Society of London, May 9th, 1843.)

In the British and Foreign Medical Review, Vol. XIV. p. 570, will be found some measurements of the smallest blood-corpuscles of mammals, from which it appears, according to Mr. Gulliver's observations, that the blood-discs of the Napu musk-deer and of the ibex, (*Capra Caucasica*,) are minuter than any before described. The blood-discs of the Stanley musk-deer are intermediate in size to those of the Napu musk deer and ibex, and therefore much smaller than the corpuscles of the common goat, (*Capra Hircus.*)

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. Remarks on Medical Reform; in a second letter to Sir James Graham. By Sir James Clark, Bart.—London, 1843. 8vo, pp. 40. 1s.

2. Animal Magnetism. By Edwin Lee, Esq. Third Edition.—London, 1843. 8vo, pp. 86. 2s. 6d.

3. The Life of a Travelling Physician, from his first introduction to practice; including twenty years wanderings through the greater part of Europe. In Three Vols. 8vo, pp. 312, 304, 291.—Lond. 1843. 1*l.* 11s. 6d.

4. A Treatise on Mental Derangement. By Francis Willis, M.D. Second Edition, revised.—London, 1843. 8vo, pp. 136.

5. Fallacies of the Faculty, &c. By Samuel Dickson, M.D.—London, 1843. Royal 8vo, pp. 188. 2s. 6d.

6. A Lecture on Quack Medicines, delivered to the Wakefield Mechanic's Institution. By T. G. Wright, M.D.—London, 1843. 8vo. pp. 44.

7. A concise Historical Sketch of the Progress of Pharmacy in Great Britain. By Jacob Bell.–Lon. 1843. 8vo, pp. 108. 1s.6d.

8. Guy's Hospital Reports. Second Series. No. I. April, 1843. 6s.

9. A Register of Experiments performed on Living Animals; disclosing new views of the Circulation, &c. By James Turner, Veterinary Surgeon. Part II.—London, 1843. 8vo, pp. 100.

10. An Essay on the Nature and Treatment of Apoplexy. By J. A. Gay, M.D. —Paris, 1808. Translated by E. Copeman, M.R.C.S., with an Appendix.—Lond. 1843. 8vo, pp. 94. 3s. 6d.

11. A System of Phrenology. By George Combe. Fifth Edition. Two Volumes. —Edinb. 1843. 8vo, pp. 519, 442. 21s.

12. On the Amendment of the Law of Lunacy. A Letter to Lord Brougham. By a Phrenologist.—London, 1843. 8vo, pp. 39.

13. Homœopathy, with Notes, &c. By Edwin Lee, Esq. Third Edition.—Lond. 1843. 8vo, pp. 51. 1s. 6d.

14. Bloodletting as a Remedy for the Diseases incidental to the Horse and other Animals. By Hugh Ferguson.—Dublin, 1843. 8vo, pp. 82.

15. On Spasm, Languor, Palsy, and other Disorders, termed Nervous, of the Muscular System. By J. A. Wilson, M.D. &c.—London, 1843. 8vo, pp. 201. 7s.

16. The Baths of Germany considered with reference to their remedial efficacy in Chronic Diseases. By Edwin Lee, Esq. 2d Ed.—Lond, 1843. 8vo, pp. 268. 7s. 6d.

17. On the Anatomy and Diseases of the Urinary and Sexual Organs. By G. J. Guthrie, F.R.S.—Lond. 1843. 8vo. pp. 155. Third Edition. 5s.

18. Practical Remarks on Gout, Rheumatic Fever, and Chronic Rheumatism of the Joints. By R. B. Todd, M.D., F.R.S. &c.—London, 1843. 8vo, pp. 216. 7s. 6d.

19. The Plea of Humanity and Common Sense against Two Publications—The Stammerer's Handbook and Mr. Yearsley's, &c.—London, 1843. 8vo, pp. 51.

20. General Therapeutics and Materia Medica, adapted for a Medical Text-book. By R. Dunglison, M.D. &c. In Two Vols. —Philadelphia, 1843. 8vo, pp. 515, 489.

21. A Practical Treatise on the Diseases of the Testis and of the Spermatic Cord and Scrotum. With Illustrations. By T. B. Curling, &c.–Lon. 1843. 8vo, pp. 540. 18s.

22. Austria: its Literary, Scientific, and Medical Institutions, &c. By W. R. Wilde, M.R.I.A.—Dub. 1843. 8vo, pp. 325. 9s. 6d.

23. Treatise on the Dental Art, founded on actual experience. By F. Maury. Translated from the French, with Notes and Additions. By J. B. Savier.—Philadelphia, 1843. 8vo, pp. 285.

24. A Practical Treatise on the Diseases of Women. By S. Ashwell, M.D. Part II. Organic Diseases.—Lond. 1843. 8vo. 8s.

25. A Conspectus of the Pharmacopœias, &c. By A. T. Thomson, M.D. The second American Edition. Edited by C. A. Lee, M.D.—New York, 1843. 12mo, 288.

26. The Transactions of the Provincial Medical and Surgical Association. Vol. XI.—London, 1843. 8vo, pp. 567. 21s.

27. Mental Hygiene, or an Examination of the Intellect and Passions, designed to illustrate their influence on health and the duration of life. By W. Sweetser, M.D.—New York, 1843. 8vo, pp. 270.

28. Clinical Remarks on certain diseases of the Eye, &c. By J. C. Hall, M.D.—London, 1843. 8vo, pp. 228. 7s.

29. Manual of British Botany, according to the natural orders. By C. C. Babington, M.A.—London, 1843. 8vo, pp. 400. 9s.

30. Medical History of the Expedition to the Niger during the years 1841-2, comprising an account of the fever which led to its abrupt termination. By J. O. M'William, M.D. With Plates.—London, 1843. 8vo, pp. 296. 10s.

31. Essays on Surgical Pathology and Practice. By A. Watson, M.D., F.R.C.S.E., &c. Parts I & II.—Edinburgh, 1843. 4to, pp. 70.

32. The Art of Living. By Dr. Henry Duhring.—London, 1843. 8vo, pp. 144.

FOREIGN.

1. Traité Pratique sur les Maladies des Organs genito-urinaires. Par le Docteur Civiale. 2d Edition. 1e Partie.—Paris, 1843. 8vo, pp. 588.

2. De Anatomia Pathologica Pedis equini et vari. Auctore J. C. Weis.—Arhusii, 1842. 8vo, pp. 97.

3. In Tuberculorin Pulmonum Dissertatio. Auctore Olavo Glas, M.D.—Holmiæ, 1839. 8vo, pp. 84.

4. Mémoire sur l'Emploi des Carbonate d'Ammoniaque dans la Scarlatine, &c. Par le Docteur Rieken.—Bruxelles, 1843. 8vo, pp. 120.

5. Thèse sur le Delirium Tremens ou Folie des Ivrognes. Par le Docteur Bouyard.—Bruxelles, 1843. 8vo, pp. 163.

6. Mikroscopiske Undersögelser af Nervesystemet. Ved Adolph Hannover, Lic. Med.—Kjöbenhavn, 1842. 4to, pp. 112.

7. Ueber den Generations wechsel, order die Fortpflanzung und Entwickelung, &c. Von J. J. S. Steenstrup, Lector an der Academie in Sorö.—Copenhagen, 1842. 8vo, pp. 140.

8. Den Pathologiske Anatomies Svar paa Spörgsmaalet; Hvad er Cancer? Ved Adolph Hannover, L.M.—Kjöbenhavn, 1843. 8vo, pp. 106.

9. Die Geschichte der Phrenologie. Von Gustav von Strave.—Heidelberg, 1843, 8vo, pp. 60.

THE

BRITISH AND FOREIGN
MEDICAL REVIEW,

FOR OCTOBER, 1843.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *De la Peste Orientale, d'après les matériaux recueillis à Alexandrie, au Caire, à Smyrne, et à Constantinople, pendant les années* 1833-38. Par A. F. Bulard, de Méru; chargé de mission par le gouvernement français pour l'observation de la Peste dans toutes les localités de l'Empire Ottoman; inspecteur du service de la marine Egyptienne; et médecin en chef de l'hôpital militaire du Caire, etc. etc.—*Paris*, 1839. 8vo, pp. 422.

On the Oriental Plague, from materials collected in Alexandria, Cairo, Smyrna, and Constantinople, during the years 1833-38. By A. F. Bulard, of Méru; Commissioner appointed by the French government for the observation of the Plague throughout the Ottoman Empire; Inspector of the Egyptian Navy; and chief Physician of the Military Hospital of Cairo, &c. &c.—*Paris*, 1839.

2. *De la Peste observée en Egypte. Recherches et Considérations sur cette Maladie.* Par Clot-Bey.—*Paris*, 1840. 8vo.

Researches and Considerations on the Plague as observed in Egypt. By Clot-Bey.—*Paris*, 1840.

3. *Notes and Observations on the Ionian Islands and Malta, with some Remarks on Constantinople and Turkey, and on the System of Quarantine as at present conducted.* By John Davy, M.D. F.R.SS.L. & E. Inspector-general of Army Hospitals.—*London*, 1842. 2 vols. 8vo, pp. 436, 478.

4. *Rapport addressé à S. E. le Ministre de l'Agriculture et du Commerce sur des Modifications à apporter aux Réglements sanitaires.* Par M. de Ségur-Dupeyron, Secrétaire du Conseil supérieur de Santé, etc. —*Paris*, 1839. 8vo, pp. 147.

Report addressed to his Excellency the Minister of Agriculture and Commerce, on Modifications of the Sanatory Regulations. By M. de Ségur-Dupeyron, Secretary of the Superior Council of Health, &c.—*Paris*, 1839.

5. *Elements of Medicine.* Vol. II.—*Morbid Poisons.* By Robert Williams, m.d., President of the Royal Medical and Chirurgical Society, and Senior-Physician of St. Thomas's Hospital, &c.—*London*, 1841. 8vo, pp. 686.

6. *Rapporto officiale fatto al sopra-intendente alla Quarantina di alcuni casi di Peste.* Scritto da Luigi Gravagna, m.d., medico principale di Sanita.—*Malta*, 1841. 8vo, p. 12.

Official Report of certain cases of Plague made to the Superintendent of Quarantine. By L. Gravagna, m.d., Principal Physician of the Quarantine Establishment.

7. *Lettre première au sujet des Accidents de Peste survenus tant au Lazaret de Koulély qu'a l'île de Proti, &c.* Par Antoine Pezzoni, m.d., Membre du Conseil supérieur de Santé Ottoman, etc.—*Constantinople*, 1841. 8vo, pp. 15.

First Letter on Cases of Plague which occurred in the Lazaretto of Koulély and the Isle of Proti, &c. By A. Pezzoni, m.d., Member of the Turkish Superior Council of Health, &c.—*Constantinople*, 1841.

8. *The Quarantine Laws, their abuses and inconsistencies; a Letter addressed to Sir John C. Hobhouse, M.P., President of the Board of Control.* By Arthur T. Holroyd, Esq.—*London*, 1839. 8vo, pp. 65.

9. *Report from the Select Committee of the House of Commons on the Contagion of Plague.*—*London*, 1819. Folio, pp. 102.

If any proof were wanting of the unsettled state of opinion among the medical profession on the subject of the contagious nature of plague, it would be fully supplied by a glance at the works we have just enumerated. We have here men of high reputation and evident ability, who have the most ample opportunities for observation and inquiry, arriving at the most opposite conclusions, and entertaining the most contradictory opinions. One asserts positively that the plague is truly a contagious disease, and may be propagated either by direct contact with diseased persons, or indirectly through the medium of various articles which have been in contact with such persons; and that the protection of the public health requires the establishment of quarantine regulations for the purpose of preventing the importation of the disease from diseased into healthy countries, by means of persons or goods. We have another equally confident that the plague is a purely endemic or epidemic disease, having a local origin from malaria or other causes in certain districts, and that it cannot be communicated from a diseased to a healthy person; consequently, that quarantine regulations, without being of the slightest service in preventing its extension, are expensive and vexatious impediments to commerce, causing immense loss of interest and time, injury to merchandise, fluctuation of markets, and great personal inconvenience.

Such a contrariety of opinion upon one of the few questions in which our profession is consulted by the legislative power, is peculiarly unsatisfactory; and as recent parliamentary inquiries would make it appear that our government, the head of the first commercial system in the world, will take that share in its settlement which the magnitude of our interests in the Mediterranean demands, we shall endeavour to lay before

our readers the real state of our knowledge on the subject, drawn from the works before us and personal observation in the Mediterranean. We do not purpose to give a critical analysis of any of these works, as in the present inquiry we have little to do with the detailed account of the history of the disease,—the various speculations as to its remote and predisposing causes,—its coexistence with other diseases,—its pathology, symptoms, diagnosis, or treatment. These may be found in any standard essay on plague, and do not bear so directly on the quarantine question; we therefore confine ourselves to the inquiries: How is the plague propagated? If by contagion, what are the best means of arresting its progress? How far do the present quarantine establishments fulfil these objects; in what respects are their regulations at variance with scientific observation; and how may they be regulated with full regard to the safety of the public, and the smallest restrictions upon commerce?

I. In proceeding with the inquiry, *How is the plague propagated?* let us determine what are the tests by which it may be proved that any given disease may be received by a healthy person in consequence of direct communication with the sick, and then ascertain how far the laws of plague are in accordance with such tests. In the first place, then, it is self-evident that when a disease can be communicated by inoculation, that disease is contagious: again, there is the strongest reason to suppose a disease contagious when it arises in a place previously healthy, immediately after the arrival of a diseased person or persons, or articles of merchandize from infected places, especially when those first attacked are those who have been exposed to direct communication with the diseased persons or infected merchandise: thirdly, a similar argument is well founded when those persons whose duties bring them into more direct communication with the sick, are affected by the disease in a greater proportion than the rest of the population, and, *a fortiori*, when the proportion of the former varies with their more or less continued communication with the sick: lastly, when seclusion and separation from diseased persons, and from all objects which have been brought into contact with such persons, have an evident influence in protecting those secluded from a prevailing disease, other things being equal, the conclusion in favour of contagion is very strongly grounded.

With regard to the effects of inoculation the evidence is contradictory. Clot-Bey inoculated himself with blood and pus from a plague patient without any result, while Dr. White after two unsuccessful attempts, caught the plague after the third, and died in three days. Dr. Valli in 1803, died after a similar experiment. Deggio, a Russian army-surgeon, inoculated himself "at Bucharest in 1773, and took the plague on the fourth day, but survived." (Williams, p. 286.) In 1818-19 Dr. Sola inoculated fourteen condemned Spanish deserters at Tangier, seven of whom are said to have taken the plague. (Clot-Bey, p. 349.) M. Bulard inoculated four persons with blood, pus from the buboes, and serosity from the phlyctena of the carbuncle of plague patients. One only took the disease, and recovered, and he was exposed to the infection of a diseased neighbourhood. (Bulard, 141-4.) Dr. Grassi, Protomedico di Sanità at Alexandria, has recorded the case of a physician who pricked himself on the ring finger of the left hand in opening the dead body of a

plague patient. Four days afterwards he had cold chills, vomiting, headach, and lumbar pains, which continued during the next day, when he noticed a small phlyctena on the finger, and a painful swelling of the corresponding axilla. On the following day the general symptoms increased to delirium, and the finger and axilla had become the seats of plague carbuncle and bubo. He ultimately recovered. (Il Filocamo, 25 Aprile, 1842.) This we believe to be the sum of veritable evidence on the results of inoculation, as the experiments on animals are valueless, until it can be shown that the plague is not a disease peculiar to the human subject. Further inquiry is necessary on this point, because the noncontagionist may urge that the above experiments were made during a prevalence or epidemic of plague, and therefore it is uncertain where the disease arose independently of inoculation. This argument, however, could scarcely be tenable, where, as in Dr. Grassi's case, a carbuncle formed at the seat of puncture.

If we examine the evidence of contagion as derived from the importation of plague into a place previously healthy, by the arrival of persons from infected districts, we shall find an immense number of cases recorded, (a very good summary of which is given in the work of M. Ségur-Dupeyron,) in which plague has followed the arrival of ships having plague on board. We, however, do not insist on these, as the greater part occurred in places subject to occasional visitations of the pestilence, and more especially as we have ample means of following up the inquiry where no such objection can be urged, namely, in the island of Malta, a dry rock of limestone, free from any source of malaria which could be conceived possible to give origin to the disease. In 1813 this island had been free from plague for 130 years, when a ship arrived on the 29th of March from Alexandria, where the plague was raging at the time of her departure, and she had lost two men by plague during the passage. Three days after her arrival in Malta the master also died, the ship lying in the harbour close to the city of Valetta. The inhabitants became very uneasy and some merchants remonstrated against the vessel remaining in the harbour, and Sir A. B. Faulkner, at that time physician to the forces in the island, addressed an official letter to the Commander-in-chief, recommending the immediate removal of the vessel. This was not attended to, and on the 16th of April, *six days after this admonitory letter was written*, the first case of plague occurred in Valetta. (Evidence of Sir A. B. Faulkner, Parliamentary Report, pp. 46.) The quarantine regulations had been very lax; some new linen was found in the house of Borg, where the first case occurred; this linen corresponded with that of a missing bale from the ship, and Borg died crying, "Oh the linen, the linen." Borg and two others of his family were the first victims; one died on the 19th of April, the two others on the 2d of May. The next house attacked was that of a schoolmistress, an intimate friend of Borg, who had attended him during his illness, and the progress of the contagion was also clearly traced through some of her scholars. After this, in the words of Sir A. Faulkner, "The foci of contagion became so rapidly multiplied, that it appeared to me impossible to carry the investigation in a direct line further in that populous city; but I am in possession of documents furnished to me by one of the captains of the Lazaretto himself, a man

of strict integrity, and many years employed in that official situation, showing that the contagion made its way in a direct line from Valetta into most of the infected capals or villages." (Parliamentary Report, p.47.)

Here then we have as striking an example as can possibly be conceived of importation of disease into a healthy community, those being first affected who communicated with the disease, and the course of the contagion being afterwards traceable. From the pamphlets of M. Pezzoni and Dr. Gravagna, we draw other undoubted proofs of importation. The letter of M. Pezzoni was addressed to Dr. Davy, and that gentleman after having written a long chapter, in which he has strongly supported the case of the non-contagionists, allows in a foot-note, (Davy, p. 133,) that it "carried conviction to my mind, previously in doubt on the question." The facts are shortly these: On June 8, 1841, a merchant vessel arrived at Constantinople from Alexandria, with some cases of plague among the passengers and crew. Constantinople and the neighbourhood had been free from plague for three years previously. A lazaretto guardian in perfect health was sent on board and assisted in landing the patients. He was taken ill on the 13th and died on the 15th with bubo and every symptom of plague. A porter also on the 22d was found to have had symptoms of plague for two days, and a very large bubo in the left groin followed. He was conveyed to the pest-house and recovered. Two other employées of the lazaret died on the 15th and 17th of July. Here then, we have a city of 800,000 inhabitants, free from epidemic influence; healthy persons are placed in contact with plague patients, four take the disease and three die. The disease is confined within the walls of the lazaret, no one but those brought into connexion with infected persons suffer; and the city has since remained healthy.

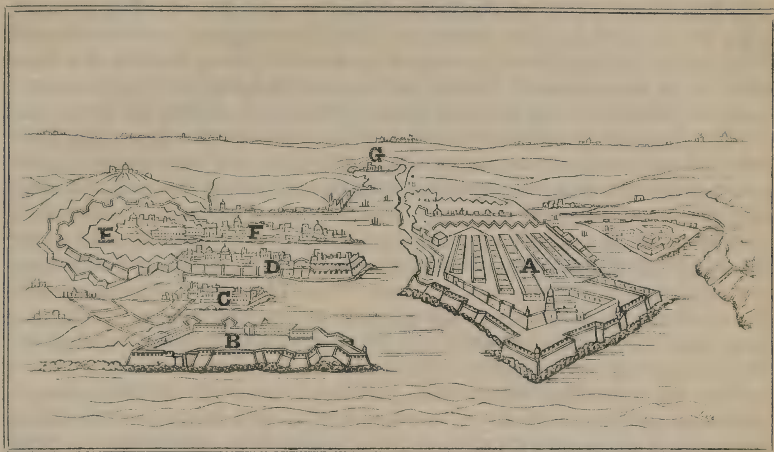
Similar facts are related by Dr. Gravagna. On the 26th of May, 1841, a vessel arrived in Malta from Alexandria, with plague on board among the sailors and pilgrims. The whole of the crew, &c., of the vessel, with guardians and two boatmen who had communicated with the pilgrims were segregated, and every precaution taken to prevent the spread of the disease. Cases appeared among the crew on the 4th and 6th of June, making in all ten cases, one showing itself during the voyage, and nine others in lazaret. One of the Maltese boatmen communicated with the vessel on the 28th of May, and on the 6th of June he was attacked by plague, with carbuncle and bubo, and died on the 10th. The other boatman was not attacked, nor were the guardians. Here, then, we see that the only man of a large population attacked by the disease, was one who was brought into contact with the diseased. He was confined in the lazaret, and the disease did not extend beyond the walls. Dr. Gravagna refers to a similar instance in 1821, when an infected vessel arrived from Alexandria, and thirteen of its crew or passengers died of plague in the lazaretto. A Maltese who had had the disease in 1813, believed himself invulnerable and volunteered to be a nurse, but after eight days he had symptoms of plague with bubo. He recovered, and, as in 1841, the disease was confined to the lazaretto. We could adduce other instances in which plague has been confined within other lazarettos, but consider the above instances amply sufficient to prove the fact of importation. They also bear on our second test for conta-

gion, persons brought into direct contact with the sick, and suffering in a greater proportion than the rest of the population, indeed the only ones who suffered. Let us now see how far this argument can be carried out during an epidemic prevalence of plague, observing how the proportion of the attendants varies with the extent of their communication with the sick.

Now, Clot-Bey himself allows that many medical attendants died in the hospitals of Cairo; "many employées, but especially those who are most in connexion with the patients." (p. 8.) Dr. Williams (p. 284) adduces some strong confirmatory facts. He says, "The French army on first taking possession of Egypt lost no less than eighty medical officers by the plague, an immense proportion compared with that of the army generally......At length the French resorted to the expedient of employing Turkish barbers to dress carbuncles, buboes, and blisters, as well as to bleed the plague patients; and after the adoption of this measure only twelve medical officers died in twice the former space of time...... In the English army of 7883 Europeans and native Sepoys only 165 died of plague, or about 1 in 48, yet of 13 medical officers 7 died of this disease, or more than half." These are striking facts, and we shall notice others when speaking of the results of segregation, as we are about to do in tracing its preservative influence to those who are separated from diseased persons.

On this part of our subject we cannot have a better field of inquiry than Malta, where the origin of the disease was evidently not local, and the inhabitants of different districts differ so little from each other as to the circumstances of their soil, climate, or customs. In this island, during the plague of 1813, when out of a population of about 90,000, 4486 deaths took place between April and November, we find that a whole city, "shut up," enjoyed perfect immunity. The annexed sketch gives a bird's-eye view of Valetta and the smaller cities to the south of the harbour. The distance from the point of Isola to Valetta is only 360 yards. This town differs in no respect from its neighbour, Vittorioso; both are surrounded by the sea on three sides and have strong fortifications on the land side. The plague was raging in Vittorioso; it was raging in Cospiena, immediately beneath the walls fortifying the land side of Isola; but not one single case occurred in this large city (Isola) during the whole period of the epidemic. The place was the residence of many old masters of merchant vessels, who had been engaged in the Levant trade and therefore accustomed to plague precautions; and they with the assistance of their townsmen, without government aid, completely severed their city from the rest of the island, admitting no person or thing without expurgation, enforcing the most rigid quarantine, and carefully guarding the place night and day. Now this town enjoyed a *total* exemption from the plague; a monument to the Virgin commemorative of this fact stands in the central square to this day, and an annual religious ceremony is regularly observed. Here is an instance then of plague kept *out* of a town by means of separation: the same epidemic furnishes us with another instance in which it was kept *within* another by similar means. The village of Curmi, situated at the head of the grand harbour, suffered severely from the plague; and when the disease was on the decline in Valetta, Sir Thomas Maitland had this

VALETTA AND ITS ENVIRONS.



A Valetta.
B Ricasoli.
C Bighi.
D Vittorioso.
E Cospiena.
F Isola.
G Casal Curmi.

village surrounded by a wall and a series of sentinels. This cordon became gradually contracted, leaving part of the village free from restriction; and in this part alone was there plague in Malta when Sir Thomas Maitland admitted the rest of the island to free intercourse. He publicly proclaimed the plague extinct when he had shut it up within these lines, while the disease was raging within a mile of his capital; and no fresh case of plague afterwards took place. Here, then, is plague surrounded in a village of a small island, the rest of the inhabitants of that island following their usual avocations, and not one receiving the disease. Another remarkable case, showing the effects of segregation, occurred in the convent of St. Augustine, which had been in strict quarantine until a servant purchased some old clothes from an infected district, and was taken ill. A monk who volunteered to attend him was placed with him in a separate apartment, and these two died, but no other individual within the walls. At Moscow in 1770-1, the Foundling Hospital "shut up," and the inmates, 1400 in number, were completely secluded. Some workmen who got over "the fences in the night time were the only persons attacked, and these being immediately separated the contagion did not spread, although more than 100,000 persons fell victims to this pestilence in other parts of the city." (Williams, p. 282.) At Marseilles in 1720, the religious houses which "shut up" were exempt; the Bishop certified that "the plague has not penetrated into the religious communities which had not held any communication with persons without." (Williams, p. 282.) The Naval Hospital in Malta was strictly insulated, and the only case that occurred was in the person of the market-man, who had strayed into an infected family. The inmates of the prisons, nunneries, and convents, with the exception of the case just mentioned, had an entire immunity; and the proportion of cases among

the military was much less than among the remainder of the population. The Sicilian regiment, which adopted strict precautions, only lost one man, who had strayed into the town; and the other regiments which adopted similar regulations, though less strictly enforced, only lost 20 of a force of 4000, a proportion 30 times less than that of the general population.

Here then are *facts*, positive facts, established beyond the possibility of question by accurate observers and faithful recorders. We have selected from the works before us, and stated circumstances to which hundreds now living in Malta can testify; and it now becomes necessary to draw the attention of our readers to the case of the non-contagionists, and the work of their grand champion Clot-Bey, which is presented under the most imposing authority. The author had resided seventeen consecutive years in Egypt, and had been placed in the most advantageous circumstances for studying the diseases of the country and obtaining every species of documentary and official information that he wished for. He successively treats of the description of the disease, its incubation, symptomatology, morbid anatomy, and treatment. Then follow disquisitions on its origin, etiology, contagionality, and lastly, on the means of guarding against its attacks, with an inquiry into the sanatory system. The aim of the work is to prove that the plague is endemic throughout the Levant; that it is never propagated by contagion nor by infection; that it is developed solely under the influence of the causes which produced it, and that it disappears with them; that its primary origin is lost in the darkness of past ages; that all therapeutic means are almost useless; that no cause assigned by any previous writer is sufficient to explain the development of the disease, and that the sole valid or necessary causes are meteorological circumstances or atmospheric conditions which he calls *pestilential constitution*, but the nature of which he does not define; that the plague is not contagious, and that therefore all quarantine institutions are useless. In connexion with this conclusion it must not be forgotten that Clot-Bey, as an officer of Mehemet Ali, must have been greatly biassed by his wish to remove the restrictions which all nations have placed upon their intercourse with Egypt; indeed he acknowledges that he commenced his observations with such a *hope*. His arguments, and those advanced by Dr. Davy, Mr. Holroyd, and Dr. M'Lean and Dr. Mitchell, among those gentlemen examined before the parliamentary committee, are simply that the plague is generated in Egypt and some neighbouring parts of the Levant by unknown local causes, being constantly endemic and often epidemic among the inhabitants; that, like other epidemics, it has a period of commencement, of maximum intensity, and of decline; that it has a regular season, never beginning before December or after March and always ceasing in June, the people on St. John's day (24th June) regularly holding a *festa* to celebrate its cessation; that this epidemic season varies in Syria and Constantinople, its annual period being later; that Persia, though yearly surrounded by plague, seldom suffers from it, and that 70 or 80,000 pilgrims depart yearly for Mecca, taking merchandize from infected places, and the disease very seldom spreads; that the clothes of many thousand persons who have died of plague are publicly sold in the market-places after St. John's day, but no accident happens to those who wear them; that the plague has appeared in houses or districts

where the strictest quarantine had been observed; that numbers in free contact with plague-patients are unaffected by the disease, and that inoculation has been practised with impunity.

Let us examine each of these arguments in detail; fully admitting, as we do, the correctness of the facts just stated, but demurring at the conclusions attempted to be drawn from them. These writers believe that if they prove the local origin of plague, if they prove it to be an endemic and epidemic disease, that they establish at once a proof of its non-contagionality. But does either experience or analogy prove that diseases arising in one mode may not be propagated in another? Have we not daily proof that when the human body has received a poison, that poison is reproduced and discharged in the exhalations? The twentieth part of a drop of variolous matter introduced beneath the cuticle is converted by the vital actions into a quantity of similar matter sufficient to cover the whole surface of the body with pustules, and healthy persons exposed to the exhalations of the sufferer are subject to the same successive effects. Is it at all unlikely that in the same way the animal body receiving the plague-poison from some exhalation peculiar to certain countries, not only suffers from the specific effects of this poison, but also, regenerating and exhaling it, becomes an irradiating focus of the disease? The periodical commencement and decline of the epidemic is also used as a strong argument against contagion, for it may be said, if it spread by contagion from May to June, why not from July to August and so on, keeping up a perpetual series of cases? But this may only show that the energy or intensity of the cause varies with the season; that the terrestrial emanations or atmospherical impregnations which produce the plague are only formed at certain seasons. We have never a perpetual series of cases of smallpox in any district, of typhus, scarlatina, nor measles. These diseases arise, arrive at a certain maximum of intensity, and then decline. The epidemic has ceased with the cessation of the atmospheric or terrestrial cause, and then no cases are met with but such as arise from direct contagion, which become more and more rare and the disease is at an end. Some morbid matter is thrown into the atmosphere and the disease commences as an epidemic; it is reproduced in the bodies of the infected and spreads by contagion while the atmospheric cause is still in operation, and this is the period of maximum intensity. Then when the epidemic cause ceases the disease declines, its propagation being effected by contagion alone.

It may be said, and said truly, that sporadic cases of plague prevail throughout the year in Lower Egypt, and if the disease be contagious, why does it not spread? It might as well be asked why does not the physical cause engendered in that district extend its operations? Or how is it that in the same district the number of cases is very small, while all the inhabitants are exposed to its operation? The fact is, that the cause of the periodically increased intensity of plague, like that of the fevers of the West Indies, of the Guinea coast, and of Ceylon; the variola of some parts of Africa; the yaws of Guinea; and the cholera of Hindostan —is perfectly inappreciable by us. The peculiar limitation of these diseases to certain districts shows that in some of the operations of nature, the morbific matter is engendered, and that in certain seasons, or certain conditions of the atmosphere, its intensity or quantity varies, and the

effects, from being merely local, become epidemic. But it does not follow that because this disease is epidemic, therefore it is not contagious, or, in the words of Dr. M'Lean, (Parliamentary Evidence, pp. 96,) "that the laws of epidemic and those of contagious diseases are not only different, but incompatible." We have just shown the fallacy of this argument from analogy with variola; it is shown in the different course of a disease purely epidemic, and that of an epidemic disease which under certain states of the atmosphere extends its ravages by contagion. The different course of the plague of Malta, in 1813, and that of cholera in 1837 was most striking. The cholera originating in the east had advanced steadily in a westward course through the north of Europe, until it reached England, when like the stream of a river diverted from its course, it turned southwards through France and Italy, and then proceeded in a south-westerly direction through Sicily to Malta. The course of the epidemic was obvious; it broke out in Malta within forty-eight hours from its appearance in Palermo; it attacked Gozo at the same time; it attacked the shipping in port; it proceeded in a certain tract, instances having been observed in which one side of a street suffered, while the other was perfectly free; the military and those who observed quarantine were attacked in a proportionate ratio with the rest of the population; and at the time of its cessation the whole population were convinced that it was non-contagious, although at the commencement all the efforts which had been made to convince them of this had been ineffectual. Now, with plague, we have seen the proofs of direct importation; and Sicily which observed strict quarantine escaped, as did Gozo until the next year, when the plague entered in a manner which we shall presently describe; the shipping, which observed quarantine, escaped from the plague; the plague did not advance in a direct line, but from one corner of the city to another, the means of its communication being traceable; we have seen that in plague the military and those who observed quarantine did not suffer in proportion with the rest of the population; and lastly from the experience they had had of plague the whole mass of the population, educated and uneducated, natives and foreigners, medical and non-medical, became universally convinced that it was contagious.

The commencement and cessation of plague at certain seasons is one of the most obscure of the laws of the disease, but proves nothing against its contagionability. This, with the facts that it "has never been known to pass the first cataracts," (Williams, p. 268;) that Persia enjoys an immunity almost perfect; and that the disease so seldom spreads during the pilgrimages to Mecca, only show that certain conditions of the atmosphere are necessary to allow of the extensive operation of the poison. Mr. Green who had resided several years in plague countries gave evidence before the Parliamentary Committee, (Report, p. 35,) that in June, about the time the plague ceases in Alexandria, "the sun has such power, that it occasions strong exhalations; a strong fall of dew, almost like rain." "I had a letter from Mr. Morier, consul-general, dated I think, in February; in which he stated, that the plague that had begun to be very prevalent, had all on a sudden entirely ceased; and that he could not account for it, unless it had been occasioned by the extraordinary continuance of dense heavy fogs." At Smyrna it generally

ceases in the month of July when the falls of dew are very heavy. This law of periodical cessation is only observed in countries in which the plague may be considered as indigenous, its continuance having been very variable in Malta, Marseilles, Moscow, &c. And what does it prove with regard to the question of contagion, but that certain atmospheric conditions are necessary either to predispose persons to the action of the poison, or even to admit of the existence of such a poison? just as certain conditions of air, water, and temperature are necessary to the process of fermentation. These conditions may all be present, but in the absence of the fermenting principle, no fermentation follows, and on the other hand without these conditions, the fermenting principle is inert.

We shall refer to the argument of the immunity of those wearing the clothes of dead plague-patients after St. John's day, when speaking of the question of contagion by *fomites*. As to the fact that plague has appeared in houses or districts where strict quarantine has been observed, it merely proves, what we have freely admitted, that in certain countries the specific causes exists in the atmosphere. All the facts on which this argument rests were observed in plague countries, and it is singular that no such facts have been observed in countries into which the disease has been imported, as Malta or Marseilles, where, as we have seen, the results of segregation were most conclusive. The results of inoculation we have shown to be contradictory, but certainly as much in favour of the contagionists as against them. With regard to the fact that numbers in free contact with infected persons do not take the disease, a little examination is necessary, owing to a remarkable instance which lately occurred on the coast of Syria, and which was made the subject of a communication from Sir W. Burnett to the Lords of the Admiralty. It appears that in December, 1840, the Zebra was driven ashore at Kaiffa, and some part of the crew were employed to repair the fortifications of St. Jean d'Acre, where several cases of plague had appeared among the inhabitants. On the 17th of February, the Castor arrived at Kaiffa, and on the 20th, fourteen men belonging to the Zebra were sent on board the Castor in exchange for a party of artificers, the working party employed at Acre having returned to the Zebra five days previously. On the evening of this day, the 20th, one of this party was taken ill and died on the 22d, of plague, and between this day and the 25th, thirteen more cases were added, who were immediately embarked on board the Castor, which sailed the next day for Malta. The disease proved to be most unequivocally the plague; and the remarkable point of the affair is that the only man belonging to the Castor who was attacked by the disease, was one of those who had been employed on shore, and lived with the Zebra's men at Kaiffa. The strictest precautions were taken to separate the infected persons from the rest of the crew of the Castor, and eleven men were selected solely to attend upon them, but none of these men, neither of the medical officers, indeed none but the men who had been on shore took the disease. It must be noticed, however, that great care was taken by the eleven men and by the medical officers, to effect the freest possible ventilation, and avoid actual contact as much as possible, and washing carefully after dressing the buboes. It is singular also that only one of the party of artificers sent on shore from the Castor, should have taken the disease. Dr. Johnson, in the 70th number of the Medico-chirurgical

Review, p. 599, speaking of these facts, says, "if anything stronger than this document can be necessary to prove the idle fears of contagionists, it must be a sign in the sky, like that which Constantine shaped his course by." But remarkable as it is, it is a mere negative fact, valuable in showing that under certain circumstances, plague may not be necessarily contagious, that predisposing causes may have great influence over its progress, and that free ventilation, especially of sea air, may obviate in a great measure the danger even of direct contact. It proves nothing against positive facts, in which contagion has been traced, any more than the fact of a man having cohabited with impunity with a diseased female, could prove that syphilis was not contagious. Yet examples of this circumstance are frequently met with, it being by no means uncommon for two persons to have connexion with the same female, the one receiving the disease, the other escaping. Persons may attend upon smallpox patients without becoming infected, and no one considers this a proof that the smallpox is not contagious, but that the parties were not predisposed to the disease at the time. Now that in the case of the men of the Zebra, there were predisposing causes for disease is evident, for they had been wrecked, consequently obliged to work hard, and submit to many privations, besides being lodged in a place "which is described as being most filthy with all kinds of abominations." Why only one of those men of the Castor sent on shore should have taken the disease is not the least curious part of the affair. That there is something in sea air which tends to check the progress of plague appears very probable from the evidence of Mr. Green, (Parliamentary Report, p. 37.) He states that he believes that there is no instance on record of any English sailor dying of the plague on board British merchant ships in Turkey, although he has taken pains to investigate this point. He attributes this to their different habits, and also to their "in general sleeping on board their ships, where there is a great difference in the atmosphere, from what it is on shore, perhaps eight or ten degrees." As in other contagious diseases, a sort of concentration of the poison appears to be necessary to ensure the propagation of plague; thus there is but little danger in approaching a patient labouring under any disease in a large open well-ventilated apartment, but crowd several into one room, or leave one in a small confined chamber, so that the pulmonary and cutaneous secretions and exhalations are collected into narrow space, and enter the lungs of the attendants in this concentrated state, in all probability the disease will be excited unless previous attacks or peculiar circumstances have exhausted or destroyed the susceptibility of the party. Dr. Marcet once collected a number of cases of typhus into one of the wards of Guy's Hospital, for the purposes of clinical instruction, when all at once the disease began to spread to other patients and to the nurses, although it had not done so when the cases were distributed throughout the hospital, and it ceased to do so when they were again divided. Whether the free ventilation, then the sea air, the previous healthy condition, or the short exposure to the "abominations" of Kaiffa of the men of the Castor, may account for their escape we know not, but must insist that if well-authenticated proofs of contagion can be adduced, no negative fact can controvert them.

It is with regard to actual contact being necessary for the propagation

of plague, as the majority of contagionists believe and as it was firmly believed by those who witnessed the plague in Malta, that the argument has been carried too far; because the proofs of local origin and extension in certain districts independent of personal contact are indisputable, and all the facts brought forward in support of this view merely prove that the infecting distance is small. It seems highly improbable that any exhalation should be formed upon the skin of a plague-patient so powerful in its effects, that applied to the cuticle of the fingers (not a highly-absorbing surface) it should propagate the disease; while the pulmonary exhalations, the very *excretions of the blood*, applied to the highly-absorbent surfaces of the air-passages, should be innocuous. It must be proved that the morbid poison is not volatile, that it is fixed like that of syphilis and hydrophobia, before such an improbable assumption can be received. The effects of segregation in Malta, in which the distance between the diseased and those separated from them was very small, do not prove the necessity for actual contact, but simply that the exhalations are not infectious at any considerable distance, a certain dilution with atmospheric air appearing to render them innocuous.

We have thus given the sum of the information to be derived from the works before us, and the results of our own inquiries in the Mediterranean, with regard to the means by which plague is propagated. We have now to consider the bearing which the knowledge we possess of the laws of the disease has upon quarantine regulations; how far these regulations are capable of arresting its progress, and in what respects they are defective or injurious to the common good of society.

II. The conclusions on which all quarantine regulations are founded are simply these: first, that the poison of plague and of some other contagious diseases can lie dormant in the body for a certain period, and *only* for a certain period; in other words, that the poison has a definite period of latency; and, secondly, that various articles of merchandise, wearing-apparel, &c., are capable of containing the contagious principle and of conveying it in sufficient intensity to propagate the disease, while some other articles have not these properties. Classes of susceptible and non-susceptible substances are thus formed, and the former are supposed to vary in their degree of susceptibility. The object of the regulations is to prevent communication for a definite period with persons, ships, and goods arriving from certain districts, and to purify all clothes or other articles supposed to be capable of absorbing and retaining the contagious principle. These purposes are effected by means of lazarettos, which are establishments in the vicinity of an anchorage, provided with apartments properly constructed for separating persons who have different periods of quarantine; a pest-house for plague patients, warehouses for the deposit of goods brought from suspected ports, and every convenience for their purification. The duration of the quarantine varies according to the port from which the ship arrived and the nature of her bill of health. This bill of health is a certificate given to the commander of all vessels at the date of their departure from any port supposed to be subject to contagious diseases, or any port in free communication with such suspected places. It is signed by the consuls or other constituted authorities of the port, attesting the state of health of the locality at the

time of the sailing of the ship. These bills are said to be "clean," "suspected," or "foul." The first imports that no infectious disease was known to exist; the second, that there were rumours of such disease, but it was not known for certainty; the third, or the absence of a clean bill, shows that the port was the seat of some epidemic or contagious disease.

Before speaking of the efficiency of these regulations, their inconsistencies or abuses, it is necessary to make some inquiry with regard to the latency of plague, and the fact of its contagion by fomites, as grounds on which the regulations are founded.

1. Now, in making inquiries with regard to the exact period of latency of plague, it is necessary to lay aside all evidence which has been collected in situations exposed to epidemic influence; because, in this case, if the disease show itself within a given period after exposure to contagion it is by no means certain that the exposure and the disease stand in the relation of cause and effect. This objection removes almost all value from the observations of Bulard, Clot-Bey, and others in plague countries. Some of the remarks of M. Ségur-Dupeyron are of considerable value. He says (Rapport, pp. 26-33) he had noticed that the cases of plague which had occurred in the various lazaretti of Europe since 1720 "had all been conveyed and communicated by men; and that, with two or three exceptions, the disease showed itself during the voyage." M. Dupeyron extended his researches to the East, and by the aid of consular authorities furnished a report in which he shows that "in 65 cases of maritime importation which it contains, the disease has broken out on board of 50 vessels during the voyage." Unfortunately he has not given returns of the actual length of the voyages, or cited cases upon which we can rely in determining the exact period of latency; but states as the result of his inquiries that 11 days may be considered as the extreme period. Dr. Aubert, in a memoir read before the Academy of Sciences in Paris, September 1841, states that in the space of 124 years, 64 vessels having plague on board have arrived in the lazaretti of Europe, and that in 26 only has the disease continued after the arrival; that it has never shown itself after the arrival unless it had previously appeared during the passage; and that the period of latency on board has never exceeded eight days from the date of departure. At Malta in 1813 the disease was said to show itself *generally* from three to six days after communication. Dr. Williams quotes from Aubert the fact, that "four criminals condemned to death were inoculated for the plague, and they all laboured under the disease before the fifth day." (Williams, p. 298.) Clot-Bey believes the mean period to be from three to six days, and that the extension to eight days is rare. The whole credible evidence goes to prove that the period is much shorter than is generally supposed; but more accurate information must be obtained before satisfactory conclusions can be arrived at.

2. On the question of contagion "per fomitem," the most contradictory evidence is advanced, and on a belief in such a mode of propagation not only are founded the quarantine regulations for the depurgation of merchandise, but also the duration of the term of quarantine of the passengers and crew of the vessels carrying suspected articles. It is on this belief that the quarantine authorities explain the apparent absurdity of

giving steam and sailing vessels the same period of segregation. If the cargo be infected, and kept in the close hold of a ship, it matters little whether the voyage has been four or five days in a steamer or two or three weeks in a sailing vessel, because the crew must be detained under observation until such a time after the free ventilation of the cargo, as shall convince the guardians of the public health that no one has suffered from the exposure of the cargo and their contact with it. This is perfectly reasonable if mediate contagion be allowed, and it is clear that the period of quarantine should then in every case be regulated by the extreme period of latency of plague after the perfect ventilation or depurgation of the cargo; less than this would be obviously unsafe, more unnecessary. But let us inquire what are the facts on which the believers and sceptics in this mode of propagation rely? Bulard (pp. 49-54) narrates two cases in which condemned criminals were dressed in the shirts and drawers taken from plague-patients and placed in the beds still warm and impregnated with the perspirations of the diseased. Both took the disease and one died. Bulard submitted himself to the same experiment and experienced no ill effect, and states as the result of his experiments and inquiries "that this mode of transmission, in the actual state of science, can neither be sustained nor combated logically." (p. 49.) Sir James M'Grigor, in his evidence before the parliamentary committee, (Report, p. 60,) when asked if the plague "can be produced from goods and things," says, "I can speak with certainty of the clothing of men; and I can also speak with certainty that blankets have conveyed it." His observations were made with the army in Egypt, and the officers of the French army became so convinced that captured articles of dress communicated the disease, that Napoleon ordered all such articles to be burnt. Mr. Green, who resided many years in Constantinople states, "that he does not think clothes or goods can convey the disease, goods certainly not." He is asked (Report, p. 34,) with regard to the habits of the Turks:

"What do they do with the clothes belonging to persons who die of the plague? Sell them; they never destroy them.

"Have you ever known the clothes to be the cause of the plague in other persons? No; I have strong reason to think they are not the cause.

"Why? Because the people who deal in them are not affected.........

"Even the bedding is sold, you say? Even the bedding is sold. There is a custom in Turkey, that if a stranger dies in the plague, the Governor or Vasha takes possession of his property, and the clothes are part of the property; and of course he orders them to be sold for his own benefit, and they dare not destroy them."

M. Dupeyron (Rapport, p. 112,) records a curious fact which bears both upon the duration of the period of latency and the infecting power of merchandise. "A ship commanded by Captain Maranguvich arrived at Venice in 1818. This ship was submitted to thirty days of quarantine. Two days only were required to be admitted to free pratique, when a passenger named Micheli Cetti rummaged his trunk to find his purse, and immediately contracted the disease. It was afterwards discovered, that owing to the carelessness of the guard this trunk had never been opened." This is all M. Dupeyron states of this case; and if the facts be correctly stated, and that the rest of the merchandise had been purified at the time of the ship's arrival, (which is perhaps doubtful,) either

a period of latency of twenty-eight days must be allowed, or the possibility of infection by mediate contact. Yet we find M. Dupeyron (p. 122) allowing "that there are no positive proofs that articles of merchandise have communicated plague in the lazarets;" and it seems to be a generally acknowledged fact that the merchandise of vessels in which plague was not actually on board, has never communicated the disease in any lazaretto. We have just alluded to the custom of selling the clothes and bedding of those dead of plague in Turkey; it would appear that this is done in Egypt and Syria with equal impunity, for Clot-Bey states that the day after St. John's day, such sales take place openly in the market-places, and without ill effect. He says that in 1836 the effects of 50,000 persons dead of plague, were sold in the bazaars without previous disinfection, and in no single instance did they communicate the disease. He adds, that upwards of 3000 patients were treated in the plague-hospital in Cairo, but that at the close of the epidemic, although through carelessness other patients were placed in the same bed, under the same woollen counterpanes, and with no other change than the blankets, yet no individual caught the plague. When we compare these facts with the experiments of M. Bulard, where the disease actually followed in two cases the exposure to fomites, it appears that disinfection of clothes and bedding is very rapid, and that the poison of plague is readily decomposed by free access of air or slight changes of temperature; and this is of great importance in its bearing on quarantine regulations, giving us some assistance in determining the period during which the contagious principle can be preserved in an active state in wearing apparel or merchandize. This period would appear to vary with the circumstances under which the fomites may be placed, as in the best authenticated cases of contagion 'per fomitem,' the suspected substance has been kept away from air and light. One most extraordinary instance occurred in Malta in 1813, which shows, at the least, that a full examination of the whole subject, and of the effects of ventilation as a disinfectant, must be made before the facts cited by Clot-Bey and Mr. Green can be considered to prove that fear of mediate contagion is groundless. We have described the state of Casal Curmi, in which the plague raged after the rest of Malta was free; in this village a man employed as a burier of the dead stole and buried in a box some articles of wearing apparel. Upwards of two months after the plague had ceased in Casal Curmi, this man dug up the box and carried it to the island of Gozo, which island had adopted the strictest quarantine, and had remained to this time perfectly free from plague. A marriage was about to take place in his family, and he opened the box to present a silk covering for the head (a *faldetta*) to the bride. He, the female, and a priest who was present, all became affected with plague from that day and died, and from that family it was clearly traced to others, and finally spread over the whole island. This fact is known to hundreds of persons now living in the islands of Malta and Gozo, and was recorded in the government despatches of Sir Thomas Maitland. It must be recollected that these islands are separated by a channel scarcely a mile in width; that their soil and productions, and the manners of the inhabitants are similar; that during the time the plague raged in Malta, Gozo entirely escaped, having adopted strict quarantine; that a man after the cessation of the disease

in Malta takes wearing apparel of persons dead of plague to a relation's house, opens it, and he himself and those in contact with him die of plague, and then the disease is traced over the island; and lastly, that Malta put Gozo into a strict quarantine of forty days, and plague did not reappear in the former island.

We may conclude, then, that although there would appear to be great danger of contagion from the clothes of plague patients which have not been exposed to air and light; that the danger of contagion, by means of merchandise, or even of clothes and bedding that have been exposed to the air, is by no means established, and we may add, without entering into the subject, that the present distinction between susceptible and nonsusceptible articles, does not appear to have any foundation in scientific principles. We think we have shown that plague is not a simply *contagious* disease, like syphilis, vaccinia, or hydrophobia; that it is not a simply *infectious* disease, like the remittent and intermittent marsh fevers; but that, like variola, it is both contagious and infectious, although the infecting distance is but small. The definite period of latency is as yet undetermined; but the whole credible evidence would prove that it has never exceeded fifteen days, probably has always been within ten. It necessarily follows that the belief on which the whole quarantine of the Mediterranean is carried on, that the disease is simply contagious and admitting everything but actual contact between healthy and suspected individuals, is unfounded, and the system based on it must be unsound in principle and faulty in operation; and the regulations of the different sanatory bodies not having fixed principles to guide them in regulating the duration of quarantine, and therefore being led by individual opinion, must give rise to the most glaring inconsistencies. Thus two travellers leave Constantinople on the same day; the one passes by land through Austria, and arrives in England in free pratique a fortnight before the other, who has come by sea, and who is still subjected to a long period of quarantine. Lastly, it would appear that many of the most injurious commercial restrictions, based as they are upon the fear of contagion by merchandise and upon the capricious distinctions between susceptible and nonsusceptible articles, might be removed with perfect safety to the public health, and with great advantage to society in general.

Admitting, then, that there are many glaring inconsistencies in the present quarantine regulations, and doubtless many abuses in the mode of carrying them into effect, we are still of opinion that the faults of any institution, the object of which is salutary, should lead, not to clamours for its total abolition, but to well-directed efforts towards its reformation. Let us inquire how may this be effected? and at the first glance it will appear that two objects must first be obtained: the one, a more accurate knowledge of the natural history of plague on those particulars with which we have shown our present information to be defective; the other, a proper understanding between the boards of health in the different states of Europe and in the Levant, by which a uniform and consistent period of quarantine and some harmless method of expurgation would be agreed upon. If it be asked how are we to obtain these desirable objects? we reply by a method which has been recommended by every practical man who has written on this subject; the appointment of a

commission of medical men by government to examine into the whole question, and furnish the necessary scientific information on which to base all sanatory regulations.

As we have seen something of the lazarettoes in the Mediterranean, it may not be amiss to give a few hints on the formation of such a commission, and their chief points of inquiry and best mode of obtaining the requisite information. In the first place, then, we should say, let men be selected who have had opportunities of studying the subject, and who have a sufficient knowledge of French and Italian to avoid the misconception of an interpreter; and we may add that Government might find such men among the medical officers of the navy, and thus carry on the inquiry with scarcely any expense. Two or three such individuals having been selected, their first object would be to obtain the full cooperation of the board of health of Marseilles, because this port virtually governs the whole quarantine of the Mediterranean. For instance, if Malta were to admit the Barbary states to free pratique, the Marseilles authorities would at once place all vessels from Malta in fifteen days' quarantine; and the same with other ports. In fact, being the chief port of the Mediterranean commerce, her authorities have merely to say " put a certain place in the quarantine we require, or we shall put quarantine upon your port," to have their wishes complied with. Thus it is evident Marseilles is the point whence to commence operations. Then it will be necessary to make inquiries in the lazarettoes of Genoa, Leghorn, Naples, Malta, Venice, Sicily, &c. with regard to the cases of plague which are recorded to have occurred in these lazarettoes, the places from whence they were imported, the state of health in that place at the time, and the length of the voyage, in order to determine as nearly as possible the period of latency in every case. With these inquiries will of course be combined others to determine if the fear of contagion by merchandise has any foundation in experience; whether any scientific classification can be made of susceptible or nonsusceptible substances; and whether living animals, not being subject to the plague, can convey it. If clothes or merchandise be found capable of conveying the disease, the most simple, speedy, and economical mode of depuration must be determined. There are many other points which will suggest themselves to any one who has read the present article. The commission extending its researches into the Levant, more accurate knowledge might certainly be obtained of some of the laws of the disease, and its relation with other endemic fevers of the country. It appears extremely probable that more accurate information might be gained with regard to the *origin* of the plague, and that though this is confined to Lower Egypt, it may arise from some habits of the inhabitants of the Delta, particularly that of throwing the dead bodies of animals into the Nile which decompose upon its banks in large numbers. Be this as it may, it is now universally observed that whenever there is much sickness among the cattle, a plague season is sure to follow, and perhaps this and other habits of the people may be as much the cause of plague as our old prison discipline was the cause of gaol fever. The plague was formerly prevalent from time to time in Europe; since the establishment of sanatory laws, this has become very rare, and then, as in the case of Malta, it could be traced to their infringement. The uninterrupted existence of the plague in the

Levant for a long series of years led to the belief that it was indigenous in the whole Ottoman empire; but after the occupation of some Turkish provinces by Russia, and the establishment of proper sanatory regulations, these provinces were kept as completely free from plague as the rest of Europe. This belief was again shaken, when, after the introduction of sanatory laws into the new kingdom of Greece, that country was completely emancipated from the scourge. Latterly, the rules of quarantine have been observed in Turkey, in Smyrna, in many parts of Asia Minor, in Syria, and in the Barbary states, and since their establishment plague has not made its appearance, Constantinople having been perfectly free from plague for upwards of five years, although formerly it was so universally present, that the authorities laughed at sanatory rules, and believed that a focus of plague existed in every corner of the city. May we not hope that in Egypt a proper system of drainage, burial of dead animals, and improvement in the dwellings and customs of the people, with proper separation of every case of plague as it arises, may gradually extirpate the disease, as they have done in Turkey? These plans Mehemet Ali has every disposition to follow, and ample means of carrying into effect; and if properly followed up it is not impossible that we might see Egypt as clear of plague as our own gaols are of typhus, or, to say the least, as our own marshy districts, or districts which were marshy, are of ague. At any rate let the inquiry be made, putting aside all theory, and disinterestedly searching for the truth from disinterested persons; and then we shall probably see a modern crusade against an exterminator of the human race which our own ignorance and evil habits have created.

Before concluding we may add that we have made many of the inquiries above recommended, and may therefore speak on something more than supposition as to their probable beneficial result. We believe it will appear that after an experience of 200 years the plague has never been brought into any lazaret except by ships coming from places where it was raging at the time of their departure; and that no well-authenticated case has been observed where the period of latency has exceeded ten days. Why then subject whole countries to quarantine where the plague is never engendered, and which take proper precautions against those places where it does reign? And why continue the system of twenty and thirty days' quarantine? If a *foul* bill of health were required only from places where the plague prevailed; and a *suspected* bill only from ports in free communication with infected districts; while ships from all places which take what may be agreed upon as the necessary precautions, should be received with clean bills, and in free pratique, every security would be given to the public health and immense advantages result to society. The whole of Barbary, Syria, Greece, the Ionian Islands and Candia would be thrown open to the free commerce of the world, and a great advance thereby gained in civilization and the promotion of human happiness. Barbary, for example, by means of Malta, would be brought within a day's journey of the centre of the civilized world, into free communication with a people speaking the same language, enjoying the benefit of liberal institutions, and anxious to establish commercial and friendly relations, which are now rendered almost impossible by the expense and delay of fifteen days' quarantine, which all experience has

shown to be perfectly useless. The Ionian Islands, Greece, and Syria, again, being brought into free communication with each other, mutual good offices are exchanged, the dependence of one state upon another made clear to all, selfish prejudices wiped away, and many obstacles removed from the attainment of the philanthropic desire to reduce man into one great family; a great moral empire in which reason, intelligence, and social improvement take the place of despotism, ignorance, and the sword.

Some may think that by such measures the public health would be endangered; we reply, on the reverse, it would be rendered much more secure, because the quarantine regulations upon vessels really liable to import the disease would be much more strictly and securely carried into effect than is at present possible. Even in Malta, where they have more space than in any other quarantine harbour in the world, it is evident to all that a hundred vessels in different periods of quarantine must be more or less crowded, and although each has a guardian on board, and some classification of the ships is adopted, yet it is impossible to be certain that communication may not at all times take place between them. This must be much more liable to occur in the confined harbours of Marseilles, Genoa, and Leghorn, and the public health is thus far more endangered than it would be by admitting ships from the places we have named to free pratique, and leaving those from districts really dangerous under the full observation and complete segregation which their small number would render easy and certain.

We have taken up this subject at a greater length than we intended: but we are deeply impressed with its importance, as one in which the medical profession must necessarily be the guide of the legislative power; and in which, by the force of reason and intelligence it may wipe away old prejudices, advance the progress of civilization, and thus adding vastly to the general sum of human happiness, shine forth more clearly than ever, a great benefactor of the world.

Art. II.

Guy's Hospital Reports. Edited by G. H. Barlow, m.d. and J. P. Babington, m.a. Vol. VII. (Nos. XIV-XV.) — *London*, 1842-3. 8vo, pp. 496.

In the throng of other candidates clamorous for notice, we have allowed our old friends of Guy's to get into the rear rank; we now proceed to do justice to the Reports in a brief analysis, according to our wont.

1. *Cases illustrative of the diagnosis of disease of the kidney, by* Dr. Barlow. The main objects of this paper are—first, to show the value of sickness, or rather irritability of the stomach, as a distinguishing symptom between diseases of the kidneys, accompanied with irritation of those organs, and affections of other structures in the neighbourhood; and secondly, to add to the stock of information already possessed regarding the peculiar cerebral affections dependent upon the non-depuration of the blood by the kidney. It deserves an attentive perusal, for the cases noticed belong to a set which often perplex the most accomplished physician.

II. This is another admirable communication from the pen of Mr. TAYLOR, entitled *Medico-legal report of a case of infanticide, with additional remarks on the fetal lungs.* Our attention must be confined to the latter part, which forms the continuation of a former paper, already fully analysed in this Journal, (Brit. and For. Med. Rev. vol. VI. p. 62.) In that article some examinations of fetal lungs were reported, and certain inferences deduced from them. The present contains the particulars of fourteen additional cases, some observed by Mr. Taylor himself, and others communicated. The results to which these investigations have led him will be seen by the following abstract:

1. *Of the test of Ploucquet.* Mr. Taylor is now of opinion "that the test is not even capable of furnishing corroborative proof, and that it would be unsafe to rely upon it." It will be evident from the annexed table that there is no sort of constant relation between the weight of the lungs and that of the body, and that when the body is below the average, it by no means necessarily follows that the lungs should be in the same condition:

Before Respiration.

	Weight of the body.	Lungs.	Ratio.
I.	57,000 gr.	694 gr.	1 : 82
II.	62,660 "	683 "	1 : 91
III.	34,540 "	630 "	1 : 54
IV.	47,170 "	703 "	1 : 67
V.	51,890 "	744 "	1 : 70
VI.	29,460 "	520 "	1 : 57
VII.	29,966 "	666 "	1 : 45
VIII.	47,025 "	658 "	1 : 71
IX.	39,370 "	550 "	1 : 71

Compare with this the following table, exhibiting the ratios obtained from lungs in which the process of respiration had been established:

After Respiration.

	Weight of the body.	Lungs.	Ratio.
I.	56,160 gr.	1000 gr.	1 : 56
II.	34,125 "	861 "	1 : 39
III.	41,788 "	920 "	1 : 45

It should be observed that in case II. respiration was imperfect, though the child lived some days.

2. *Absolute weight of the lungs.* In the former paper the mean weight of healthy lungs, from six mature children which had died *without breathing*, was stated to be 569 gr., in the nine cases reported now, the average weight was 649 gr. Mr. Taylor correctly observes, that it is of the utmost importance that the maturity of the child should be fully ascertained in making estimates of this kind, and it would, therefore, be well that the reporters, instead of resting satisfied with the bare assertion that such was the case, should describe the general characters presented, by which means others would be enabled to judge for themselves.

The average weight of the lungs *after respiration*, derived from the three cases in the table, is 927 gr., but the degree to which respiration has been established must materially influence the result. There is also another source of fallacy which, to our minds at least, would render all deductions from experiments of this kind of very small value, viz. the

impossibility of determining the previous weight of the organs, without which it is clear that even an approach to certainty cannot be anticipated; inordinately small lungs, even when fully dilated, may not reach the common average. In respect of immature children, Mr. Taylor remarks that there would be great risk in giving an opinion of respiration having taken place from the state of the lungs. Cases xiii. and xiv. in this paper illustrate the observation. They were both seven-months' children. In case xiii. the lungs weighed 559 gr.; the child lived an hour, respiring, but with difficulty. The organs, even when divided into 48 pieces, sank in water. In case xiv. the lungs weighed 556 gr.; the child lived thirty-six hours, breathing freely, and the lungs floated even when divided into 60 pieces. Here the difference of weight only amounted to three grains, and yet the lungs of one were filled with air, while not a trace of it could be detected in those of the other child. Upon the whole, then, it is evident that the question is one to which a conclusive answer cannot easily be found.

3. *Artificial inflation.* The experiments related in this paper are, for the most part, confirmatory of the opinion formerly expressed, that artificially inflated lungs, as well as those in which natural respiration has been carried on to a limited extent only, may be deprived of the contained air by pressure; but they show also that much greater force is sometimes required than has been generally supposed. Where the lungs are heavy, have great buoyancy, and cannot be made to sink by firm compression, Mr. Taylor thinks we have good corroborative proof of the child having breathed. Considerable weight alone is of little value.

4. *Signs of maturity.* The cases reported in this paper show that in mature children the umbilicus is situated from a quarter to half an inch below the centre of the body, instead of exactly coinciding with that point, as Chaussier has stated.

5. *Signs of live birth.* Mr. Taylor's observations have confirmed the opinion that the circle of redness at the junction of the umbilical cord, with the skin of the abdomen, is no proof of live birth. It was found in five cases of stillborn children, and was absent in others which had survived birth. Dr. Geoghegan observed a slight contraction about the centre of the ductus arteriosus in two children which were born dead. This point requires further investigation.

We need scarcely say that we strongly recommend this paper to the notice of our readers.

iii. *Observations on pelvic tumours obstructing parturition, by* John C. W. Lever, Esq. This is a very lengthened communication, extending to sixty-eight pages. A brief abstract would be of little value, and we must therefore pass it by in silence. To this course we are the less disinclined from the fact that, although containing a good account of the diseases in question, and exceedingly judicious rules for their treatment, the author has not propounded any views beyond those which are generally received and acted upon by the profession.

iv. *On the digestive solution of the œsophagus, and on the distinct properties of the two ends of the stomach, by* Mr. T. Wilkinson King. That the coats of the stomach are not unfrequently dissolved

and perforated, to a greater or less extent, by the action of the gastric fluid after death, is a fact now almost universally recognized, though opinions are still divided as to the necessity of previous disease in the membrane to enable this process to be set up. This point is not specifically noticed by our author, but we should judge that he would answer the question by a negative, for he says that "in the autopsies in London the effects of digestion on the stomach itself are *as often discoverable as not*;" and it is scarcely probable that in half the cases of death there should be affections of that viscus. We apprehend, however, that the experience of other practitioners will not show so great a proportion; we have opened a considerable number of bodies in our time, and been present at the examination of many more, and certainly never met with anything like so great a number. Mr. King observes "it is to be regretted that the inferences afforded by these effects have not had much more influence in limiting and directing the objects of experimentalists on the subject of digestion. When, for instance, the coats of the stomach have been perforated, and the surface of the spleen has become similarly dissolved or the diaphragm perforated, is it not evident that the agencies of the par vagum and of peristaltic motion are altogether inadmissible, as essentials to the solvent action?" This is perfectly true, if the mere *solvent* power of the fluid be regarded, and the same is abundantly proved by the experiments on artificial digestion *out of the body*; but we take it, the question to be determined (and it does not appear to us to have ever yet been satisfactorily cleared up), is not this, but—What influence have the par vagum and other nerves upon the *entire process of digestion?* including under this head the secretion of the gastric juice, as well as its influence upon the alimentary matters contained in the stomach, when once effused. As far as our own observations extend, we feel convinced that these parts have a most important office to perform, in whatever way their action may be explained. But upon this discussion we cannot enter.

Solutions of the œsophagus are also common, but not to such an extent as in the two cases narrated by Mr. King. The first of these is an example of a very extreme kind; the whole circumference of the tube was destroyed in two places, just above the stomach, an intermediate portion about an inch in length being unaffected, probably from the weight of the heart dislodging the fluid. The edges of the divisions were soft and flocculent. The fluid had escaped into the right pleural sac, and the investing membrane of the obtuse posterior edge of the lung was entirely destroyed, over a space of about twelve square inches, the alteration being circumscribed by an abrupt margin of flocculent pleura, beyond which the serous membrane bore a natural appearance. The stomach was entire. Mr. King believes that the portion of the tube immediately above the cardiac sphincter is most commonly affected.

The remainder of the paper is occupied with a series of observations in support of the opinion that the left half of the stomach is alone concerned in the production of the digestive fluid. After noticing the testimony of various authors to this fact, and the frequent existence of a line of demarcation, passing perpendicularly across the lining membrane of the viscus, a little to the right of the œsophageal opening, and on the pyloric side of which he believes solution never takes place, (excepting from ac-

cidental circumstances, such as the casual passage of the proper secretion of the left cul-de-sac, from position or other causes,) the author proceeds to narrate some experiments of a rather novel character. They consisted in the testing of the two ends of the stomach, previously well washed with litmus paper, and the result was almost invariably the same; the fragments of paper laid on the cul-de-sac exhibited the presence of an acid, while those which were in contact with the pyloric half were either altogether unchanged or but feebly reddened.

We cannot regard these experiments as by any means conclusive. They are exposed to many sources of fallacy, more especially from the influence of gravitation, which would necessarily cause any fluid, that might remain in the stomach at the time of death, to collect in the great end of the organ; and although this were removed, and the fluid adhering to the surface entirely washed away, still a sufficient quantity would have permeated the tissues, before examination, to render the results doubtful. Case 8 affords a striking corroboration of this opinion. In this case "the stomach contained but little, and was acid at all parts, but having been well washed under the watercock, and set aside (sprinkled with litmus cuttings), for twenty-four hours, *no part* evinced acidity." The stomach was opened *four* hours after death, which would scarcely allow time for extensive permeation. Still we would not deny that there is some value in these experiments, and we shall quote the one which appears to us least liable to objections:

"Case III, Jan. 13, 1842. Maria H., about thirty-five years of age, fat and intemperate, was found dead. The body was examined about six hours from the probable time of her decease. There was much serous effusion in the head. The liver was tumid and lacerable. The stomach contained only about an ounce of mucus, scented with gin, and not acid. The stomach, being washed under the water-cock, and sprinkled with litmus cuttings, exhibited acid re-action, in the cul-de-sac only, after twenty-four hours." (p. 152.)

Mr. King is opposed to the idea that the mucus performs any part in the solvent actions; he chiefly regards it as the protective means by which the coats of the stomach escape injury, and deprecates the idea of any other agency being required for this purpose.

V. *Two cases of injury of the head, followed by symptoms of compression, produced respectively by extravasation of blood and formation of pus, relieved by operation, by* EDWARD COCK. These are two interesting cases, but we have not space to give more particulars than are contained in the title of the paper.

VI. *Observations on urinary concretions and deposits, by* GOLDING BIRD, M.D. This is a paper of considerable value, and most creditable both to the industry and chemical knowledge of the author. It is chiefly occupied with an account of the calculi in the museum of Guy's Hospital, but is interspersed with remarks derived from other sources of personal experience. The collection now contains 363 specimens, all of which, excepting 21, have been divided so as to exhibit their internal structure. The examination was not limited to the composition of the external crust, but had respect to every layer, and particularly to the chemical characters of the nuclei. Dr. Bird has given a table in which the calculi are ar-

ranged in classes according to the nature of their nuclei. It fully confirms the generally-received opinion of the great preponderance of uric acid and urates in these deposits as will be seen in the annexed abstract.

Abstract View of Nuclei.

Nuclei, consisting of	uric acid or urates	. . .	262
,, ,,	cystic oxide	. . .	11
,, ,,	oxalate of lime	. . .	45
,, ,,	phosphates	. . .	21
			339
	Mixed calculi	. . .	3
			342

We may observe that in a subsequent part of the paper the number of calculi containing uric acid or urates as their nuclei, is stated to be 255 instead of 262. How this discrepancy can be explained we do not know. It is also worthy of remark that although the nucleus most commonly occupies the geometric centre of the calculus, this is not always the case; it is sometimes remarkably eccentric, as in certain uniform concretions; and, in a few, several distinct nuclei, or centres of deposition, are met with. In some rare instances the concretion which forms the nucleus is found loose within the body of the entire calculus; a circumstance in all probability arising from a layer of blood or mucus having concreted around the nucleus, and on which the matter forming the body of the calculus became deposited. In this case, on the whole becoming dry, the mucus or blood would be diminished to a very thin layer, and the nucleus detached from the body of the stone. In a few instances calculi appear to possess no nuclei, the centre being occupied by a cavity full of stalactitic or mammillated projections. Dr. Bird has only observed this in uric-acid calculi.

We shall not occupy any time in describing the shape or other external appearances of the various kinds of calculi, and urinary deposits; they have already been frequently noticed in this Journal, and must be familiar to the majority of our readers. But we would particularly direct their attention to the following condensed account of the *microscopic* characters of the different constituents; and that, not only by reason of the intrinsic interest of these observations, but also, and chiefly, because we desire, as much as possible, to urge upon all future inquirers the essential importance of the microscope in almost all investigations into the structure and diseases of the human frame. For the examination of urine, a low power (a good half-inch achromatic object-glass, with a low eye-piece,) is abundantly sufficient. The deposits, after being washed and allowed to dry on a slip of glass, can be beautifully preserved by means of Canada balsam, and thus rendered permanent.

Uric acid. We shall not here insist upon the characters of urine containing an excess of this constituent, nor attempt to solve the much litigated question of the state in which it exists in the natural secretion, further than to remark that our author inclines to the opinion of Dr. Prout that ammonia is the solvent. The presence of this salt, the urate of ammonia, can be readily demonstrated, either by exposing the urine to a low temperature, or by evaporating it in the vacuum of an air-pump.

Under the microscope the deposit appears amorphous. Dr. Bird has never met with it in the form of needles, as described by M. Vigla.

Urine containing this deposit occasionally presents a very remarkable phenomenon, after being heated to the boiling point, and kept at that temperature for a few minutes. The salt soon disappears, and is replaced as the ebullition continues, by a deposit which resembles albumen in being totally insoluble in nitric acid : it differs from albumen, however, in the fact that it cannot be precipitated from urine by that acid, and that so high a temperature is required for its development. Its nature is not well understood ; Dr. Bird is inclined to refer it to the same category as the incipient albumen of Dr. Prout.

Uric acid is more rarely deposited in a free state ; it may, however, be thrown down by several acids, and the tint of the crystals will vary according to the amount of chemical action exerted on the colouring matter of the urine by the acid employed. The lightest-coloured crystals are obtained by the use of acetic or phosphoric acid ; those produced by the nitric or hydrochloric are generally deeply tinted, and present, under the microscope, a somewhat complex structure. The crystals are sometimes large enough to be visible by the naked eye. M. Vigla affirms that when the grains of uric acid are as large as this, the deposit is always amorphous. The results of observation in this country are totally opposed to such a statement. The following are the varieties noticed by Dr. Bird.*

A. Rhomboids of a tolerably distinct lozenge-shape. This is the natural form. The acute angles are commonly well defined, but the obtuse corners of the lozenge are often rounded off, so that the whole crystal represents a long elliptical figure with sharp extremities. In the centre there is always a curious angular marking resembling a nucleus.

B. The elliptical lozenge-crystal is sometimes singularly modified. A portion being removed near each acute angle, so that the whole resembles a kind of spindle, sometimes approaching a fleur-de-lys figure. These crystals are often of a beautiful amber colour, and are usually met with in very acid urine.

C. Not unfrequently the outlines of the crystals lose their acute angles, and they then form flat rectangular tables, about twice as long as broad, more rarely they resemble cubes, in which case they generally constitute the whole of the deposit. In the cubes there is evidence of an internal structure ; lines arranged in parallelograms, being readily distinguished by the microscope. If these crystals be allowed to dry, and are then examined after immersion in a fluid, the edges become translucent, and they have the appearance of opaque cubes set in a transparent frame.

D. In very acid urine the long tabular crystals are sometimes seen serrated at their extremities, or so deeply shaded as to give the idea of their being edged with a series of dark compact lines. This appearance is never visible at the sides of the crystal. Under the microscope, these crystals are seen to be intersected by two crescents, the convexities of which are turned towards each other, and sometimes touch.

E. Occasionally the tabular, and more rarely the rhomboïdal varieties are found united in the form of a cross, or stars of various degrees of

* Drawings are given of the most interesting of these and other deposits.

regularity. They are frequent in the bright orange-red sand, and are often abundant in the pink deposits of urate of ammonia.

F. Uric-acid crystals, resembling quadrilateral tables, sometimes, with a carefully-adjusted light, are seen to be composed of a series of short cylinders lying on their sides. The discovery of these belongs to M. Vigla. Some other forms we need not mention.

Cystic oxide. The microscope affords the readiest means for the recognition of this rare substance. It is always deposited from the urine in a white crystalline state, and the crystals present themselves under one of the two following forms: 1, Tolerably regular six-sided tables, which are sometimes transparent throughout, but more commonly opaque at the centre; 2, roundish tables, opaque in the centre, and somewhat crenate at the edges. This form has not previously been described by any author. In order to obtain these crystals, the deposit in which they are suspected to exist should be washed with boiling water, to remove urates and any common salt which may be present. The best method to obtain them from a calculus is to digest some of the powder in a warm solution of ammonia, and allow a drop of the clear fluid to evaporate spontaneously on a slip of glass. There is, however, one source of fallacy which must not be overlooked, and this arises from the curious modifications presented by crystals of common salt, when evaporated, either rapidly or slowly from urine; some of these pretty closely resemble the crystals of cystic oxide. They may be detected by the following characters; they are never opaque in the centre as those of cystic oxide generally are; they do not exhibit colours with polarised light, and they are soluble in water.

Uric (xanthic) oxide. The account which Dr. Bird gives of this substance is merely an abstract of the observations of MM. Liebig and Wöhler, which have been already partially noticed in this Journal. (Brit. and For. Med. Rev., vol. IX. pp. 364-5.) We shall, therefore, confine ourselves to the transcription of the following tabular views of its characters as contrasted with those of uric acid, with which substance it is most likely to be confounded, the chemical composition of the two being extremely similar:

Uric Oxide.	*Uric Acid.*
"1. Dissolves slowly, and without any effervescence, in nitric acid.	1. Dissolves readily, with copious effervescence, in nitric acid.
2. The nitric solution leaves, by careful evaporation, a yellow stain.	2. The nitric solution leaves, by evaporation, a pink residue.
3. It dissolves in concentrated sulphuric acid; and the addition of water does not render the solution turbid.	3. It dissolves in concentrated sulphuric acid; and the addition of water produces a copious precipitate of uric acid.
4. Its solution in potass is not precipitated by hydrochlorate of ammonia.	4. Its solution in potass is precipitated by hydrochlorate of ammonia.
5. It is precipitated pure and free from alkaline combination, when a current of carbonic acid is passed through its solution in potass.	5. It is precipitated from its solution in potass, by a current of carbonic acid in the form of urate of potass.
6. When decomposed by heat, it does not yield any trace of urea.	6. Decomposed by heat, it becomes partly converted into urea." (p. 203.)

The pink colouring matter of urine receives the name of *purpurine* from our author; but neither this, nor the *carbonate of lime*, which is occasionally met with in calculi, need detain us.

We pass on to the consideration of *oxalate of lime*, a most important ingredient of urinary concretions. Dr. Bird is not satisfied with the now commonly received opinion that the oxalic acid is a secondary product of the oxidation of saccharine matter occasionally present in the secretions, and is more inclined to account for its existence by the conversion of uric acid into oxalic acid and urea, as shown in the experiments of Liebig and Wöhler.

The oxalate of lime never occurs in an amorphous form ; it is always regularly crystallized. The crystals are white in colour, sharply defined, and always entire: they are perfectly octohedral, but differ materially in the acuteness of the terminal angles. When these are very acute, the crystals are seen lying on their sides, with their angles and edges exceedingly distinct; when the octohedron is more obtuse, the outline of the crystal appears perfectly square, having another square outline in its centre, so arranged that the angles of the inner square are opposite the sides of the outer one. When these crystals are allowed to dry, and then examined either in a drop of water or Canada balsam, the inner square remains transparent, whilst the outer becomes opaque, and often absolutely black. The shape of the crystals can be well seen by adopting a method pointed out by Mr. S. W. Griffith. Add a few drops of alcohol to an oxalate deposit moistened with water; a series of vortiginous currents, perfectly distinguishable by the microscope, become excited by the mixture of the two fluids; in these the crystals roll over each other, and their true figures become beautifully distinct. Dr. Donné is the only writer who has described the crystals of oxalate of lime, as they occur in urine. It should be observed that the artificial crystals which may be produced by adding oxalate of ammonia to urine, differ considerably from these: they resemble globules, transparent in the centre, and having a marked tendency to become arranged in rouleaus, somewhat after the manner of blood-discs. We must refer to the original paper for a description of the plan by which oxalate of lime may be most easily detected in calculi.

Deposits of *phosphate of lime* are sufficiently common, and the same salt very generally exists in calculi, after forming a considerable portion of their mass. It is, however, exceedingly rare to find it constituting the nucleus. It always occurs in an amorphous state. Such is not the case with the *ammoniacal phosphate of magnesia* (triple phosphate), which presents two distinct forms of crystal, according as the proportion of ammonia varies. The first, or neutral triple phosphate, is frequently met with in the well-known iridescent pellicle which collects upon urine containing this salt, and under the microscope is seen to consist of beautifully defined transparent crystals, all of them being either prisms, or some modification of that figure. They are often strongly shaded on one side, and illuminated at the edges and angles, unless they are immersed in water or some other powerfully refracting medium, when they appear perfectly transparent. The second, or bibasic phosphate, always occurs in alkaline urine, and in all probability is a secondary product. Dr. Bird has never seen it constituting the entire mass of a deposit, but it can frequently be detected in mixture with phosphate of lime. Its crystals consist of thin laminæ, resembling foliage, and sometimes traversed by a transparent cross. When formed artificially by the addition of am-

monia to urine, they are always of an elegant stellar shape. When mixed with phosphate of lime they constitute the fusible compound. The nature of any deposit, or fragment of calculus containing phosphates, may therefore be at once determined, by dissolving it in hydrochloric acid, placing a drop under the microscope and adding ammonia; this determines the precipitation, either of an amorphous granular mass, of a series of stellar crystals, or of both together: in the first case, the matter consisted of phosphate of lime; in the second, of the triple phosphate; and in the third, of the fusible compound. A striking example this of the value of such methods of investigation.

We have not room to discuss Dr. Bird's speculations regarding the derivation of these deposits, and as he promises to bring forward the data upon which they are founded at some future period, we abstain altogether from any remarks upon them. The paper, which our readers will see is one of no small value, concludes with a useful table of the *action of re-agents on urine containing deposits*, or as our author somewhat quaintly terms it, at various times in the body of the communication, the *behaviour* of urine under these circumstances.

vii. *On the location of pulmonary phthisis, and its relation to diagnosis, by* H. Marshall Hughes, m.d. This paper comprises the results of numerous investigations into a subject of great importance, and bears throughout the impress of a cautious and discriminating mind. We shall endeavour to present our readers with as complete an abstract as our limits will allow.

Before entering upon the more immediate object of his study, Dr. Hughes makes some remarks upon a form of phthisis, fortunately rare, in which the usual auscultatory signs are absent, the chest being throughout perfectly free from dulness, the voice not abnormally resonant, and nothing being heard by the stethoscope beyond a small mucous or muco-crepitating rattle, universally diffused through every part of the lungs. In such a state of things, it is no marvel that the disease should be mistaken for bronchitis, and the practitioner grievously disappointed at the utter failure of all the ordinary means of treatment. After death, which is seldom long delayed, the mystery is cleared up, and the lungs are found thickly studded with miliary tubercles, or granulations. We have said that a fatal result is, in general, rapidly attained, but such is not always the course of events; the malady may assume a chronic character, and time be given for the softening and suppuration of the tubercles. We extract the following case (abridged,) which not only illustrates this fact, but shows also that the disease may occur in persons who have arrived at or passed the middle period of life. The patient was a male, aged forty-five, and had formerly been intemperate. About a year before he was seen by Dr. Hughes he received a severe blow upon the right side, from the effects of which he believed that he had never recovered. Nine months after this occurrence, he was attacked, by his own report, with inflammation of the lungs, and treated actively. He recovered partially, but the cough did not entirely leave him. When first seen he appeared to be labouring under simple bronchitis and dyspepsia, and was treated accordingly; but after a time " he began to sink rapidly, and though the chest was repeatedly examined, and found uniformly and

universally resonant on percussion, gurgling succeeded to simple mucous rattles; and the characteristic sputa of advanced phthisis, independently of other symptoms, indicated but too clearly the nature of the malady. He died about six weeks after I first saw him, and between four and five months after being first troubled with cough. On inspection of the body, twenty-four hours after death, tubercles were found to be equally and universally distributed throughout both lungs. They were in all stages; some were transparent and others opaque; some had become softened, and others, by the suppuration of surrounding parts, had already been expectorated, and had left small cavities of the size of small nuts. There existed no pneumonic consolidation; and the only exception to the general condition of the lungs I have mentioned was a cavity, rather larger than the others, situated in the very apex of the right lung."

We must now follow our author in the examination of the table he has constructed, and which includes the records of 250 cases, 175 being males and 75 females. This table originated from a desire to ascertain which lung was most frequently affected with tubercles; in which, when both were diseased, they were first deposited, or in which they made the most rapid progress; and, what is of far higher importance, in what proportion tubercles were first deposited in the superior lobes of the lungs. A large majority of the individuals were alive at the period of examination; but as cognizance was only taken of those cases in which the disease was in an advanced stage, and where the symptoms rendered the nature of the malady perfectly unequivocal, even without the aid of auscultation, this consideration detracts but little from the value of the deductions. The only exception we would be inclined to make is one which perhaps deserves the name of hypercritical, viz., that as the disease was approaching a termination before the patients were examined, the seat of the *first* deposition could only be ascertained by determining the part most extensively affected, and must therefore rest upon an assumption, probably enough correct, that excavations will soonest occur in the part that was primarily attacked. An example of the table is given, from which the nature of the whole may be readily appreciated.

Is one lung more obnoxious than the other to the deposition of tubercles? Laennec believed that the right was most generally affected, and Louis affirms that the reverse is the case. The following table exhibits the results of Dr. Hughes's investigations:

		Cases.	per cent.
The left side was chiefly diseased in		116	46
The right ditto ditto		89	36
The more diseased side was doubtful in		45	18
Of the 116 cases on the left side, there were	males	76	43
	females	40	53
Of the 89 cases on the right side, there were	males	66	38
	females	23	30
Of the 45 cases, in which the most diseased side was doubtful, there were	males	33	19
	females	12	16

Of the 48 cases examined after death, of which 11 only were females,

	Males.	Females.
Tubercles were confined to the left lung in	3	1
" " right "	1	0

From the above statements it would appear that the left lung is upon

the whole most susceptible of tubercular disease; but the difference is manifestly so slight as to be utterly useless in relation either to diagnosis or prophylaxis.

In what proportion of cases are tubercles first deposited in the upper part of the lungs? Of the 250 cases recorded by Dr. Hughes, the upper lobe of one or both lungs was solely or principally diseased in 237 or 95 per cent. Of the remaining 13 cases, of which 11 were males and 2 females, there were 9 or $3\frac{3}{5}$ per cent. of the whole number, in which both lungs were universally and uniformly diseased. Of these nine, 8 were males and 1 was a female. Of the remaining 4 cases, the upper lobe in them was at least equally affected with other parts. In only one case out of the whole 250 were tubercles confined to the base, there being none in the upper lobes of the lungs.

It is not our purpose to enter into any discussion of the diagnosis of phthisis, and we shall therefore abstain from noticing the remarks made by our author in reference to the differential signs between this malady and those with which it is most liable to be confounded; but we must beg leave to refer our readers to the observations of M. Grisolle (noticed in our last Volume, p. 385,) as showing that pneumonia more frequently attacks the upper lobe of the lungs in the first instance than is generally believed in this country; and we shall conclude this abstract by inserting the following table, exhibiting *the age at which phthisis is most fatal*, although the numbers are too limited for certain conclusions to be deduced from it.

"Of persons (not children) who have died, or were in a condition which would soon terminate in death, there were—

	Males.	per cent.	Females.	per cent.	Total.	per cent.
Under the age of 20	14	8	5	7	19	8
Aged 20, and under 30	70	41	37	49	107	43
,, 30 ,, 40	45	26	21	27	66	26
,, 40 ,, 50	39	23	9	12	48	19
,, 50, and upwards	7	4	3	4	10	4
Total	175		75		250	

(p. 258.)

VIII. *On the proceeding to be adopted in a case of injured intestine, from a blow upon a hernial sac, by* C. ASTON KEY, Esq. Mr. Key recognizes three states of the bowel which may be produced by an accident of this nature: the coats may become merely inflamed in consequence of the contusion, or the violence may be such as at once to rupture the intestine, or, failing this, the contusion may be so severe as to be followed by sloughing and escape of fæces. The treatment of each of these must necessarily vary. In the first case, the contents of the sac should be returned and the ordinary antiphlogistic regimen adopted, with this exception, that purgatives should be most carefully abstained from. We think Mr. Key is perfectly correct in dwelling with much emphasis on this point. When there is either inflammation or a tendency to that state in any other organ of the body, our attention is at once drawn to the necessity of keeping it at rest for some time; why should we pursue a different course when the intestinal canal is the suffering part? It would be hard

to find any better reason than common routine habit. In the severer forms more active proceedings are required, and the safety of the patient will depend upon a free exit being afforded to the offending matters. The sac, therefore, should be at once laid open (i. e. whenever symptoms of rupture present themselves,) and a temporary artificial anus established. The cases related are interesting and instructive.

IX. Mr. FRANCE has related a case of *irideremia*, or *absence of the iris*, an exceedingly rare malformation, in this instance apparently congenital.

X. Dr. BABINGTON records *a case of enormously distended gall-bladder*. The cyst contained at least three wash-hand basins of fluid. The nature of the case was not suspected during life, and treatment was of course of no avail.

XI. *On pneumonia, by* H. M. HUGHES, M.D. This paper is intended to form a sequel to the one on phthisis noticed above. It is not devoid of interest, but the absence of anything peculiarly novel in the views presented, renders a detailed analysis unnecessary, seeing that the disease in question has formed the subject of such frequent discussion already in the pages of this Journal. The results are drawn from the records of cases observed in Guy's Hospital, and which are arranged in two tables; the one consisting of cases of primary pneumonia; the other of cases in which distinct evidence of the disease was found after death, the patients having been, for the most part, the subjects of other and more chronic affections. Our attention in reference to both must be confined to the subject of "location." In the *first* table we have the account of 101 cases. In 19 of these *both* lungs were diseased; in 29 the *left* alone was affected; while the *right* was involved in 52. The disease was confined to the *base* in 62 cases; it occupied the *whole lung* in 12; it was limited to the *posterior* parts of the organ in 8; to the *apex* in 5; and to the *centre* in 3. It was situated in various parts of one or both lungs in 9; and in 2 cases the locality was not mentioned.

The second table contains the records of 145 cases, in 60 of which *both* lungs were affected; in 43 the disease was limited to the *right*, and in 40 to the *left* lung. The parts principally or solely involved were as follows: the *upper lobe* in 13, the *centre* in 7, the *posterior* in 4, and the *base* in 49. The whole organ was generally diseased in 65.

We should scarcely have noticed these results, but for the sake of comparison with the statistics given in the former paper; our readers will see, at once, that in this point of view they have considerable value, the contrast in the allocation of the two diseases being of so striking a nature.

XII. *Cases of hemorrhage after delivery, by* Dr. C. W. LEVER. The author's object is to call attention to two predisposing causes of hemorrhage, which are not generally recognized by obstetric writers, viz., diseases of the spleen and kidneys. In reference to the *first*, he has arrived at the following conclusions: "1. That in females affected with enlargement or disease of the spleen, the uterus is predisposed to dilate, and therefore admits of the effusion of blood into its cavity. 2. That

the blood so collected coagulates, and excites considerable irritation, as marked by the accession of rigors, fever, &c. 3. That the fever so produced, in course of time, (varying in different cases,) assumes the intermittent type, especially when the patients have previously suffered from ague. And 4. That such intermittent fever is curable by the same remedies as are successful in the treatment of pure and uncomplicated ague." In regard to the *second* cause, he believes: "1. That labour occurring in patients affected with morbus Brightii is generally lingering. 2. That in such patients, although the fœtus and its secundines may be expelled by the natural uterine efforts, and the uterus may for a time appear to contract, yet that it is very liable to become relaxed, and distended with blood 3. That in patients so affected, peritonitis of a more or less acute character is prone to occur." We recommend this paper to the careful notice of our obstetric brethren, whose attention cannot be too forcibly directed to the distressing occurrences of which it treats.

xiii. *On the microscopic globules found in urine, by* Dr. Golding Bird. The author distinguishes four kinds. 1. *The true pus-particle,* roughly granular on its surface and evidently compound in its structure, becoming reduced on the addition of ammonia to a mucous mass, readily miscible with water; in which, however, the particles may be discovered, apparently shrivelled. These particles are partially dissolved by acetic acid, leaving numerous minute transparent circular bodies, which are regarded as their nuclei; they are free from granulations, and about one fourth the size of a blood-disc, or 1-8000th of an inch in diameter. The urine is always albuminous, and becomes opaque on the application of heat.

2. *The true mucous globule,* generally rather smaller than the pus-particle, being about 1-2000th of an inch in diameter, appearing circular, with a smooth well-defined edge, under a low magnifying power, but becoming distinctly granular on the surface with a power of 250 diameters. They can be distinguished from the former only by their being not readily miscible with water, becoming less shrivelled on the addition of ammonia, and having fewer and less distinct granulations. They are to be found in all urine after it has been allowed to stand for some time, and are increased in quantity when there is irritability of any part of the urinary mucous membrane. When this last condition co-exists with diuresis or a frequent desire to pass urine, they become replaced by the third species, or,

3. *The large organic globule,* which varies in size from 1-1000th to 3-1000th of an inch in diameter, and is with great difficulty distinguished from the pus-particle. These globules rarely, if ever, constitute a deposit, but are found free and floating in the urine, generally scattered and often few in number. They are not so evidently compound as the pus-globule, though, when treated with acetic acid, they become broken up, and have similar minute transparent globules. Dr. Bird thinks this variety is identical with that described by Rayer under the name of muco-pus. These globules are abundant in the urine of pregnant women, especially during the latter months, and when there is frequent desire to empty the bladder. Dr. Bird has also found them in every

case of ardor urinæ which he has examined, and also in the urine of patients labouring under Bright's disease. He has detected them also in several cases of malignant disease of the womb.

4. *The small organic globules.* These are minute and very rare. They are apparently perfectly simple in their structure, absolutely spherical, free from granulations, and about 1-3000th of an inch in diameter. They mix with water with the utmost readiness, and undergo no change whatever. They often form a glistening white deposit at the bottom of the capsule in which the urine has been allowed to cool after being gently heated; and in this state closely resemble, to the naked eye, the oxalate of lime deposit. Dr. Bird has only met with this variety in two cases, and in each its appearance was evanescent: both specimens of urine were passed by women during menstruation.

XIV. *Case of poisoning by arsenic, by* Mr. HILTON. The value of this paper consists in the admirable analysis conducted by Mr. A. S. Taylor. It is impossible to draw up anything like a perfect abstract, and we must be contented with noting one or two points of special interest. A man aged thirty five swallowed, by his own confession, upwards of a quarter of an ounce of arsenic, from the effects of which he died in ten hours. The stomach was highly inflamed, especially at the pyloric extremity; it contained about a pint of dark-brown mucous fluid, in which some very small granules of the poison were mixed; and its coats were lined with a large quantity of semi-transparent glairy adhesive mucus, which included numerous portions of arsenic. Though there was enough arsenic in the stomach to saturate the contained liquid fully, yet, when the solid matters were allowed to subside, *not a trace* of the poison could be detected in it. We need scarcely point out the lesson which this fact should teach. Four ounces of the blood were examined, according to the improved method recommended by Orfila, but without success, probably on account of the small quantity employed. Mr. Taylor recommends sulphuric acid as a carbonizing agent, in preference to the deflagrating action of nitre. The spleen, analysed after the manner pointed out by MM. Danger and Flandin, gave equally negative results; but when seven ounces of the liver were treated in the same way, slight traces of the presence of the poison were clearly detected. We strongly recommend a perusal of the original.

XV. *Case of fatal pleuritis, by* Mr. W. G. CARPENTER. This is a very singular case. On examination after death, the right pleural cavity was found to contain a large quantity of sero-purulent fluid, and "*a piece of ivory* worked into four artificial front teeth, covered with a brownish crust, with a pointed piece of silver rivetted into the upper part of the teeth, which had evidently assisted in fixing them to the upper jaw; the base of the silver rivet surrounded with wadding." There was an old fistulous opening on the outer surface of the lung, large enough to admit the tip of the little finger. Upon inquiry, it was discovered that the patient, who had been afflicted with asthmatic bronchitis from childhood, had "swallowed" the teeth, during a fit of coughing, thirteen years before his death.

XVI. *Observations on inflammation of the aqueous membrane of the*

eye, by Mr. S. R. Bedford. *Aquo-capsulitis*, or inflammation of the aqueous membrane of the eye, it is stated, is essentially marked by the appearance of a general or partial opalescence, of various degrees of intensity, and which may have its origin in the corneal, iridal, or capsular portion of the structure. When seated on the corneal part, it may be mistaken for corneitis, or corneal conjunctivitis; but a careful examination of the organ, in different lights, will generally lead to a correct diagnosis. There is greater difficulty in determining the true nature of the case, when the iridal portion is affected; but this is practically of little moment. The opalescence does not appear to depend upon morbid deposition, but is simply the effect of vascular turgescence, occurring in a transparent membrane. The disease may terminate in increased secretion, in fibrinous or puriform effusion, and in ulceration. 1. The *first* result, or, in other words, dropsy of the anterior chamber, is well known as a most obstinate affection. Mr. Bedford thinks that evacuation of the aqueous humour is a measure of very doubtful value, as far as a radical cure is concerned, and should only be adopted when the pain, caused by the distention, is extreme. He narrates some cases which go to prove that it may be relieved, at least, by ordinary means of treatment. 2. *Fibrinous effusion*, in the first stage, is amenable to simple remedies; and, when more advanced, yields to slight mercurial action. The membrane, in this state, has sometimes a peculiar spotted appearance, which Mr. Bedford regards as pathognomonic of the disease. The spots are distinctly visible, very circular, and uniformly diffused. 3. *Puriform effusion* is less common. When it exists to a great extent, evacuation may be resorted to with advantage: but in most cases the matter is absorbed, under the action of the ordinary means of cure. 4. *Ulceration* is the least frequent termination.

We can conscientiously recommend the whole paper to the consideration of all who are interested in the diseases of the eye. It is drawn up with much care, and the cases are narrated with a precision and minuteness that are worthy of all praise: but we were somewhat surprised to find no mention of any local applications, in the cases of ulceration of the cornea which are related. The value of a strong solution of the nitrate of silver in such cases is too well known to require a defence here.

xvii. *Observations on the diseases of the orifice and valves of the aorta, by* Norman Chevers, m.d. This is an exceedingly valuable contribution to pathology, and fully maintains the author's character for elaborate minuteness of research. We shall present our readers with as complete an analysis as our limited space will allow.

Great difference of opinion still exists in reference to the nature, the causes, and the effects of the various organic changes which are so frequently observed at the commencement of the aorta; and much of this appears to arise from the common error of attributing to disease, and of charging with the production of morbid symptoms, many slight deviations, which are either unimportant in themselves, or are the results of means adopted by nature, either to limit the extension or repair the ravages of a destructive process, or to adapt the uninjured portions of a diseased heart to the new circumstances in which they are to act. The

object of the paper before us is to place the question upon a more satisfactory basis, from the examination of a large number of aortæ both in a state of health and disease.

Dilatation of the aortic orifice. In order to arrive at an accurate knowledge of the dimensions of this part, it is necessary to measure the circumference of the vessel both above and below the valves, because, even in the healthy state, there is a considerable difference between the two, and this disproportion is greatly increased and varied in every case of organic disease of the heart and arteries. The measurement of eight healthy aortæ in these situations gave the following results. The mean circumference of the canal immediately below the valves was thirty-six and a half lines ; that precisely above, thirty-four lines. But upon traction being made, (after separating the vessel from the heart,) the latter portion was found to be dilatable to forty-three lines, the former only to thirty-nine and a half; proving, that while the lower part of the ostium is the wider, the upper is by far the most dilatable.

Dilatation of the lower part of the orifice becomes developed to a greater or less extent in nearly all cases of eccentric hypertrophy of the left ventricle; it also frequently, but not always, accompanies simple dilatation of that cavity. The most common immediate cause is, either thickening and contraction of the aorta above or behind the valves, or disease of the sigmoid crescents themselves interfering with the free emptying of the ventricle.

Dilatation of the superior part of the ostium usually results from obstruction existing either in the main trunk, or in some of the terminal branches of the aorta. It is generally found in cases of thoracic and abdominal aneurism; in stricture of the descending arch, where the ductus arteriosus is closed; in association with extensive ossification of the smaller arteries; and in the subjects of chronic renal and hepatic disease. This part yields first, because it is placed between two forces, viz., the active impelling power of the ventricle, and the fixed resistance of the obstacle, and because it is in itself much more dilatable than the lower portion. The ventricle does not begin to suffer undue dilatation until the whole of the vessels between the heart and the impediment have been stretched to the uttermost. Previously, therefore, to this occurrence we have expansion of the upper part of the orifice, with a smaller state of the lower, but without actual contraction of this part; for the ventricle, though thickened in its walls, does not become altered in its cavity for some time.

When this state of parts exists, i.e. when the upper portion of the aortic ostium is more dilated than the lower, the most usual condition of the valves is, either great lengthening of their marginal cords, or depression of their superior points of attachment, with shallowing of the pouches, and rounding or, occasionally, more or less retroversion of their free borders. But, in many cases, Dr. Chevers has observed a singular natural adaptation of the parts, by which the ill effects of regurgitation were obviated : "the upper margins of the sigmoid curtains having been stretched laterally, their crescents, even though rendered rather shallower than natural, will still perform their valvular office ; as, having lost their usual horizontal position, they bag down into the still undilated opening of the lower part of the ostium, and, their edges there coming into oppo-

sition, the vessel is perfectly closed against the reflux of blood into the ventricle." It is probable that the good effects of this arrangement are but of temporary duration, for as the edges of the valves only are in contact, there is a great tendency for the edge of one to be pressed below the others, or retroverted by the force of the descending blood at each ventricular diastole. It is clear, therefore, that the rule laid down by some authorities, that dilatation of the orifice of the aorta must of necessity be attended by inefficiency of the valves admits of some qualification. Even when both parts of the ostium are largely dilated, the valves, if undiseased themselves, may perform their office, for they become widened and deepened in proportion to the expansion of the tube.

When the valves are in the position above noticed, viz., in contact by their edges only, they are also very apt to become sacculated below, and are thus exposed to the danger of sudden rupturing, or of more gradual perforation from the ulceration of atheromatous or other matter deposited in their structures. And this leads us to the next point in our inquiry.

Rupture and ulceration of the sigmoid valves. In sixteen cases out of nineteen of this nature, examined by Dr. Chevers, the lower part of the aortic ostium was morbidly wider than the upper. In eleven of these, the upper was much contracted, while the lower was greatly too wide; and in the other five, the upper was rather widened, but less so in proportion than the lower. In the seventeenth and eighteenth, the superior was dilated, while the lower was rather contracted; and in the last, the whole of the orifice was far too narrow; but in this case, another part of aorta was congenitally malformed. It becomes here an interesting subject of inquiry, whether the lesion of the valves was the cause or the effect of the dilatation of the orifice. Dr. C. inclines to the latter opinion, seeing that in nearly two thirds of the cases, there was quite enough to account for the dilatation, altogether independent of the state of the valves. We think the question is still open to investigation. The edges of these lacerations are often covered with granular masses of coagula, which of course tend to lessen the amount of blood which regurgitates. Our author has observed also, "that where a lacerated curtain has become much loaded with fibrinous or boney concretions, the sinus behind is usually far deeper than the other two; as if this indentation allowed the thickened mass as much room as possible to recede while the blood is passing into the aorta." Such a state of disease may continue for some time without proving fatal; of this fact Dr. Chevers relates a very striking instance. (p. 404.)

Atrophy of the semilunar valves. This state of disease our author believes to be exceedingly rare; at least, unless this term be applied to those cases in which, from morbid depositions between the laminæ, the nourishment of the parts has become defective. The cribriform state of the valves, which is occasionally observed, the edges of each opening being quite smooth and covered by a reflection of the serous membrane, and which has been attributed to the same cause, he conceives to be congenital, depending merely upon an arrest of development during an early period of intra-uterine life; for, among other reasons, he has observed it three times in the aorta of children about four years old, and once in an infant which died at, or very shortly after, birth.

Vegetations. Two kinds of these are observed: granular clots, evidently

mere deposits of fibrin from the blood, and minute semi-transparent warty bodies, which some authors regard as of a similar nature. With respect to these latter, Dr. Chevers has been led to form a different opinion. He regards them as duly organized growths, seeing that their form is unvarying and symmetrical; that they are completely blended in with the serous membrane, and that he has found them assuming a deep yellow hue in jaundice; and he believes that they are intended to *retard* the progress of diseased actions in the parts upon which they appear.

Contraction of the aortic orifice. This may occur in three situations, viz. either below, above, or within the curtains, the first of which is the most rare. This state is generally occasioned by local disease in the tissues; but there are several classes of cases in which, without any evidence of such lesion, the whole of the aortic trunk with its appendages is found unusually small. Upon this point Dr. Chevers has recorded some valuable observations, for which we must be contented with a simple reference to the original; and thus conclude our imperfect sketch of what we cannot but characterize as a most interesting production.

xviii. *Case of contracted aorta, by* W. Muriel, Esq. The constriction, which almost amounted to obliteration, was near the junction of the *ductus arteriosus:* the superior intercostals were enormously enlarged.

xix. *Two cases of disease of the larynx, requiring laryngotomy, by* Mr. Hilton. These cases deserve perusal, but are not susceptible of condensation. The operation, in one case, was performed by means of a curved trocar and canula, which Mr. Hilton thinks preferable to the knife.

xx. *On the operation for cataract, by* Mr. Morgan. The peculiarities of Mr. Morgan's methods of proceeding are as follows: the needle is extremely fine, of the same thickness from the point to the handle; cutting at the point but not at the sides, and hardly longer than the diameter of the globe. It is passed through the sclerotic, at the distance of rather more than a line from the cornea, and just below the transverse diameter; it is then carried, with a slight inclination forwards, directly through the central substance of the cataract, completely transfixing the lens. This requires to be done with much care: the needle should be *drilled* through, by rotating the handle of the instrument between the finger and thumb. The lens is then *pulled* out of its place with great ease and precision; and when it has reached the situation it is intended to occupy, the needle is cautiously withdrawn by a similar rotatory motion.

xxi. *Observations on certain diseases originating in early youth, by* Dr. G. H. Barlow. The publication of this paper enables us to perform a promise made some quarters back to our readers, by reviewing a communication of the same author, which we omitted in our notice of the 13th number of the Reports. The two memoirs, taken in connexion, form an essay of much interest and great value, upon a subject of the highest importance. All the cases narrated have reference to diseases

of the circulating and respiratory organs, and may be resolved into four classes: 1. Those in which obstruction to the circulation in the right side of the heart was produced simply by defective expansion of the lungs and air-passages. 2. Those in which it was the result of pericarditis acting *mediately through the impediment opposed to the respiratory movements.* 3. Those in which it resulted directly from the obstruction to the return of the blood to the left side of the heart, produced by contraction of the left auriculo-ventricular orifice. 4. Those in which the origin of the mischief was of a more complicated nature, depending upon two of these causes conjointly, or upon all combined. Our limited space prevents our entering upon a full consideration of the whole; and we shall therefore, in a great measure, confine our notice to the first class, in reference to which the author's views appear to us to possess the most originality.

Under this head we have the details of two cases. The symptoms in each were nearly similar, viz. dyspnea commencing at an early age, disordered circulation, and dropsy, proving at length fatal. The one, a girl, died at the age of sixteen; the other, a male, at the age of eighteen; and in neither were there any signs of puberty. In the first case, that of the female, the right side of the heart was enormously enlarged, both by hypertrophy and dilatation, and the tricuspid valve inadequate to close the auriculo-ventricular opening: the pulmonary artery was smaller than natural, but healthy. The left side was also enlarged, but not to the same extent; the aortic valves healthy, and also the vessel, which however was too small. The trachea was small; there was an obvious narrowing about an inch and a half above the bifurcation, and both bronchi were compressed and flattened. The chest was narrow and contracted, and the lungs fleshy. The liver was very much enlarged. In the second case, the alterations in the heart were very similar; enlargement of the right side, and contraction of the orifices of the pulmonary artery and aorta, both vessels being smaller than natural; but there was also atheromatous matter under the lining membrane of the pulmonary artery, throughout its ramifications. The left bronchus was much flattened, and the trachea and larynx so small that they might have belonged to a boy of six years old. The liver was large, dark, and hard.

In both these cases Dr. Barlow regards the *narrowing of the trachea* as the true cause to which all the subsequent phenomena are to be attributed, the series of causation being as follows: "About the age of eleven in the first case, and fourteen in the second, growth, and the consequent increase in the quantity of the blood returned directly by the cavæ, called for a corresponding increase in the respiratory function. This, however, could not be effected from the imperfectly expanded state of the lungs, arising most probably, as has been shown, from the narrowness of the trachea. The immediate effects of this were, diminished flow of blood from the right ventricle, and narrowing of the pulmonary arteries. Hence arose the distention and hypertrophy of the right ventricle. The accumulation of blood in this ventricle caused either regurgitation into the right auricle, or opposed such an obstacle to the exit of the blood from thence, that it became, in its turn, greatly distended, and the distention being kept up by the pressure of the blood in the veins, dilatation and hypertrophy of the auricle ensued; the concomitant dilatation of

both ventricle and auricle giving rise to the enlargement of the auriculo-ventricular opening, and inadequacy of the tricuspid valve to close that opening. The immediate effect of this state of things would be the accumulation of blood in these reservoirs, if I may so term them, the liver and the spleen; and the secondary consequence, accumulation in the whole venous system, giving rise to the lividity and anasarca observed in both cases. The same cause must also have impeded the passage of the blood through the venæ cavæ hepaticæ, and consequently through the portal veins, and thereby have given rise to the ascites."

The application of these views, thus lucidly stated, to treatment, both in the way of prevention and cure, is sufficiently obvious, and need not detain us; but we cannot pass them by without expressing our admiration of the philosophic manner in which our author addresses himself to the often difficult task of analysing complicated symptoms, and reducing them to a simple and intelligible form. Would that the same qualities were more common!

The *second* class of cases, viz. those in which the cardiac disease was the result of pericarditis acting mediately through the impediment which it opposed to the respiratory movements, is well illustrated by an interesting case, (No. vi.) to which we must refer our readers. We beg their particular attention to it, because the influence of this disease in preventing the due expansion of the lungs is, we apprehend, not generally recognized, and observers are too apt to ascribe all the changes detected in the heart to it alone, as a *primary* cause. We would remark, also, that pericarditis occurring in a subject whose growth is completed, does not, when the valves are unaffected, lead at once to hypertrophy, as is commonly supposed, but tends rather, in the first place, to produce diminution in the volume of the heart. This point is noticed and well illustrated by Dr. Chevers in the paper examined above, (vide p. 421, et seq.) The third and fourth classes do not require any observations on our part. We earnestly recommend a careful perusal of the two memoirs.

ART. III.

Semeiotique des Urines, ou Traité des Altérations de l'Urine dans les Maladies; suivi d'un Traité de la Maladie de Bright aux divers ages de la vie. Par ALFRED BECQUEREL, M.D. &c.—*Paris*, 1841. 8vo, pp. 576.

Urinary Semeiology, or Treatise on the Changes of the Urine in Disease, &c. By ALFRED BECQUEREL, M.D.—*Paris*, 1841.

THE volume of which we have just transcribed the title does not now make its first appearance in this Journal. We some while since, in an earlier number, (26th of April, 1842,) introduced the second division of the work to the notice of our readers,—that devoted to the consideration of Bright's disease. Circumstances before alluded to prevented us at that time from analysing the first division; these circumstances no longer interfere with the discharge of an agreeable duty, that of making English observers of urinary disease acquainted with some of the most elaborate, and in a particular direction the most excellent researches, yet under-

taken on the chemical constitution and pathological relations of the urine.

M. Becquerel's inquiries and results are collected under three heads. To the first of these is referred the study of the chemical and physical properties of the urine; here are examined the elements constituting it, and especially the variations in respect of quantity which it undergoes in disease. Next the physical properties of the fluid are reviewed, and the degree to which the chemical constitution can be inferred from the physical aspect carefully considered. In the second of these parts the scattered facts of the first are brought together, as elements for the establishment of the general propositions relative to the pathology of the urine. In the third part the author endeavours to apply the general principles thus ascertained to special pathology, and follows the modifications of the urine in different diseases in particular. Of each of these parts we shall give as full an account as the nature and importance of the subject justifies.

Chemical and physical properties of the urine. The old division of urines voided at different periods of the day and under different circumstances of fasting, repletion with food, &c., which was established by the early observers and retained by their followers, is rightly regarded as practically just by M. Becquerel. The *urina potûs*, the urine of digestion, and the urina sanguinis, are fluids which, though essentially the same, yet vary in a sufficient number of particulars to make it necessary we should in every case be aware to which of those kinds any given specimen belongs. To chemists having neglected to acquaint their readers of this circumstance, some of the questionable and many of the contradictory statements afloat on the chemistry of the urine may be attributed. An instance of this is furnished, according to M. Becquerel, by the quantitative analysis of Berzelius, which has so often been quoted in all tongues and lands, as the dictum of the great authority; here the quantity of urea and uric acid is certainly greater than it would have been, had the eminent analyst taken the mean of the amounts furnished by the three kinds of fluid. But this state of things prevails only in health. In various forms of disease which are capable of effecting change in the composition of the urine, it may be affirmed generally that the distinction in question ceases to exist; the only very notable exception occurs when a very large quantity of fluid has been gorged during the twenty-four hours. But although the urine is in these maladies, except in really exceptional instances, similar in properties at whatever time it be voided, the author generally selected the morning urine (urina sanguinis) for his experiments, because at that period its composition bears a closer relation than at any other to the constitution of the blood, and because then, if collected under the same circumstances, it is always identical. Such are the views which have led all physicians to choose the morning urine for the subject of experiment; but as M. Becquerel very fully shows, and as we are ourselves quite aware, this mode of proceeding is incapable of furnishing answers to all the questions that the practical physician may put himself. For example, the urine of a subject labouring under intense febrile reaction is denser, more acid, of deeper colour than that of a healthy person, and often presents a uric acid sediment com-

bined with colouring matter; such urine appears to contain more uric acid therefore and animal matter than the healthy secretion: but this augmentation may be apparent and not real. Who can affirm that the case is not one of simple diminution of the proportion of water, and consequent concentration of the solid ingredients? M. Becquerel well argues that the affirmation cannot be made from a consideration of the morning urine only.

Another method of avoiding the errors which are liable to arise from examining the urine voided at a particular period only of the day was pointed out by Chossat, and employed by Lecanu in his very excellent experiments. We mean that of collecting all the urine passed in twenty-four hours, mixing it together and analysing the whole; but even this plan, adopted by the author in many instances is, he points out, liable to entail some kinds of error, and attended with much difficulty. He in the first place observes that it cannot be considered in accordance with sound principles of physiology to divide time into periods of twenty-four hours in reference to secreted products, that a subject may pass more urine one day, less the following, &c., and thus a state of compensation be effected. Lecanu, cognizant of this, examined the urine of six, eight, ten, and some twelve days, and then took the mean of the total results: but this mode of proceeding, though excessively laborious, is at least possible in the examination of the excretion of healthy persons, but it is impossible and open to important fallacy in the state of disease, as the urine alters in character from day to day. A difficulty encountered on the part of patients is, that it is very difficult to persuade them to keep all their urine, more especially to make sure of their always passing the contents of the bladder before those of the rectum, when about to have a stool. A further difficulty arises from the absolute impossibility of collecting the urine of subjects passing it involuntarily, and of others placed in various circumstances, which will readily suggest themselves.

M. Becquerel commences his chapter on the composition of the urine considered generally, by making the following statement: "The urine voided in the course of twenty-four hours, or that passed in the morning, by the same individual, is never of the same composition identically on any two consecutive days or occasions; the mean quantity of each chemical element is never precisely the same." Generally speaking, the variations in the quantity of each element are trifling in amount, but they may become seriously large. For example, M. Lecanu found that the quantity of urea passed by the male in twenty-four hours might vary between 22·446 grains and 9·119 grains, and by the female between 17·983 and 9·881 grains; in the analytical table which we shall presently transcribe, the author gives the mean of the oscillations which he, as Lecanu, observed in the proportions of the different ingredients. And there is an important inference from this, when the quantity of any principle is found in specimens of urine voided by persons out of health, to be but slightly above or below the mean standard, namely that the deviation discovered must be considered one compatible with a healthy constitution of the fluid.

TABLE,

Exhibiting the Mean Composition of the Urine of the Male and Female in the normal state, and the Mean deduced from the addition of both. The Table is constructed from the results of the Analyses of 8 healthy specimens of Urine: 4 of Males and 4 of Females.

Chemical Elements.	Males.		Females.		General Mean.	
	Urine of 24 hours.	Composition in 1000 parts	Urine of 24 hours.	Composition in 1000 parts	Urine of 24 hours.	Composition in 1000 parts
Quantity of urine	1267·3		1371·7		1319·8	
Density	1018·900		1015·120		1017·010	
Water	1227·779	968·815	1337·489	975·052	1282·634	971·935
Other matters beside water furnished by direct evaporation	39·521	31·185	34·211	24·948	36·866	28·066
Urea	17·537	13·838	15·582	10·366	16·555	12·102
Uric acid	0·495	0·391	0·557	0·406	0·526	0·398
Fixed salts not decomposable at a red heat. { Chlorides, Phosphates, Sulphates, or } { Lime, Soda, Potass, Magnesia, }	9·751	7·695	8·426	6·143	*9·089	*6·919
Organic matters, which cannot be isolated and calculated separately, { Lactic acid. Lactate of Ammonia, Colouring matters, Extractive matters, Hydrochlorate of Ammonia. }	11·738	9·261	9·655	8·033	10·696	8·647

* *Composition of the fixed salts in the Urine of* 24 *hours and in* 1000 *parts.*

	Urine of 24 hours.	In 1000 parts.
Chlorine	0·659	0·502
Sulphuric acid	1·123	0·855
Phosphoric acid	0·417	0·317
Potassa	1·708	1·300
Alkaline and Earthy bases { Soda, Lime, Magnesia }	5·181	3·944
	9·089	6·919

The mean quantity of water voided in the urine of twenty-four hours being 1282·634 grains, M. Becquerel has found that it may vary between 800 and 1500 grains without exceeding or falling below "physiological limits." The inference from this is obvious and important: as the quantity of solid matter held in solution is the same whether the water be to the amount of 1200 or only 800 grains, the appearance of the urine must be very different in the two cases. And it might further be inferred from this, as experience proves is the fact, that in various species of disease the quantity of water does not swerve from the wide standard compatible with a healthy state of the fluid.

The quantity of water voided with the urine increases beyond the natural amount when the quantity of liquids drunk is considerable; thus two litres (about two quarts and half a pint) swallowed by a person habitually passing 1000 grains increased this average quantity to 2712·924 grains. In polydipsia (diabetes insipidus), in diabetes, and in hysteria and other nervous conditions, the quantity of water passed is greatly increased, as is well known.

Diminution of the quantity of water is of more common occurrence, and is referrible, according to the author, to the following causes. 1. The existence of fever, and consequently of all circumstances capable of giving rise to febrile reaction, such as acute and chronic inflammations. 2. Diseases of the liver and heart, especially if these are of a kind to produce general disturbance of the system. 3. Diseases, of whatever kind they may be, attended with such general disturbance. 4. The existence of abundant sweats. 5. The quantity diminishes immediately before death; complete suppression even may ensue at this period.

The proportional quantity of water in 1000 parts of urine oscillates slightly above and below 971·934 grains; it increases in chlorosis and anemic affections generally: in the former malady it may reach 990·298 grains. On the other hand, the author has sometimes found the proportional amount fall, in the febrile state, or during the paroxysms of pulmonary emphysema or cardiac disease, to 955 grains; the lowest amount met with, was 948· grains, observed in a strong woman labouring under milk-fever.

The sum of solid principles held in solution in the state of health in the urine is as follows:

	Water.	Solid principles secreted in 24 hours.
Males ...	... 1227·779	... 39·521
Females ...	... 1337·489	... 34·211
General mean	... 1282·634	... 36·866

The quantity may vary in the male between 36 and 41 grains, without ceasing to be compatible with health; in the female between 32 and 36. The conditions determining an unnatural increase are: 1. A rich, abundant, and strongly nitrogenized diet. 2. The consumption of an excessive quantity of water;—to this very important point we shall by and by return. 3. The existence of the disease polydipsia, almost as a consequence of the last. 4. Occasionally the urine voided under the influence of nervous affections, hysteria, &c., are characterized by containing sufficient solid ingredients to cause the total amount of these in the twenty-four hours to be unnaturally great. 5. Diabetes.

On the other hand, the circumstances productive of disease are: 1. The existence of febrile action, of acute inflammation, of functional disturbances, if to a marked amount, of attacks of disease of the heart, of the lungs and liver, &c. 2. If in addition to these states there be some cause of debility, of exhaustion, or of anemia present, the degree of diminution is much greater. 3. In chlorosis, anemia, and various states of debility, the same effect in the solid materials is sometimes observed; the contrary is the case in other instances, as we have just seen. The total amount of solids held in solution sometimes remains natural in disease.

In the natural state the quantity of urea varies as follows, according to the density of the urine and the sex of the subject:

	Density.	Quantity of Urea in 1000 parts.
In Males ...	... 1018·900	... 13·838
In Females ...	... 1015·120	... 10·366
General mean	... 1017·010	... 12·102

The quantity of urea voided in twenty-four hours may vary between 15

in females and 18 in males; it may very rarely exceed this amount in the state of disease; M. Becquerel himself has never observed such augmentation. Diminution of its sum is the general law in the morbid state, and in some cases may be so great as to lower the quantity voided in twenty-four hours to less than a third of the lowest amount compatible with health. A very valuable table is here given, displaying the condition of the urine in respect of density, and its amount of urea in 37 cases of disease arranged in five different series. The first series (we can only give the general results,) composed of cases of which the urine was in general dense, high-coloured, very acid, and moderate in quantity, (febrile cases, &c.); the mean density was 1021·914; the mean quantity of water 616·495 grains, of urea 9 grains. The second series comprises urines generally of lightish colour, of low density, and moderate abundance, furnished by young chlorotic subjects, by anemic patients, &c.; the mean density being here 1011·837; and the quantity of water 1161·390, the urea amounted to 7 grains only. The third series, composed of patients worn out by disease, by discharges of some kind, or by treatment (in one case of chronic pleurisy 200 leeches had been applied within a short time), and also labouring under febrile action, gives a mean density of 8014·880, as the mean quantity of water 594·051, and of urea 4·918 grains. In the fourth series are placed the cases of subjects resembling those of the first in every respect, except that the quantity of water voided by them was not only not diminished, but sometimes increased. The mean density in this series was 1010·500; the mean quantity of water 1285·230 grains, and of urea 8·919. In the fifth series the author has grouped cases of disease in which there was neither fever nor very marked functional disturbance; they give a mean density of 1017·747; as the mean quantity of water 949·316 grains, of urea 12·351.

Among the observations made upon changes in the *quality* of the urea, we observe the following assertion, which is at very distinct variance with ideas generally received. If urine taken indiscriminately and heated to ebullition, become turbid, opaque, and give origin to a deposit of some abundance, this is solely composed of sub-carbonate of lime, and sometimes of magnesia, from which the elevated temperature has removed the excess of carbonic acid required to hold them in solution. The ordinary belief, taught too by all recent writers, is, that the deposit consists in these cases of phosphates. The proofs of his view given by the author are, that a drop of acid instantly dissolves the precipitate,—this does not prove, however, that the salts are not phosphates; secondly, that this solution is accompanied with disengagement of bubbles of carbonic acid.

In the chapter on uric acid, and the alkaline and earthy urates, the everlasting question of the composition of the amorphous sediments of acid urine is once more discussed. The author agrees with the notion promulgated by M. Quevenne, namely, that the sediments in question consist of uric acid rendered amorphous in consequence of its combination or mixture with animal matters—these matters he believes to be the colouring and extractive substances. "Collect these sediments," observes M. Becquerel, "wash them successively with cold distilled water and cold alcohol, and in this way you will remove the greater part of the

associated foreign matters; examine the sediment under the microscope, and an amorphous powder alone presents itself; treat this matter with a small quantity of nitric acid, or, better, of hydrochloric acid, which does not dissolve uric acid, nor the urates at the ordinary temperature; in the greater number of cases there will be no change, the matter will remain amorphous as before: now, if it were composed of acid, alkaline, or earthy urates, no doubt the nitric or hydrochloric acid would have separated the base, and allowed the uric acid to crystallize." This would be a very convincing argument if a correct statement, but we find the author admits that sometimes the crystallization does take place, and we believe that we have frequently detected it ourselves. This, however, only proves that in such cases lithates are present; and the question is, in reality, one of frequency: or it may be put in this way, if in a single instance an amorphous sediment were shown to be non-convertible into uric-acid lozenges by hydrochloric acid, this would prove at all events that its component materials were not ordinary lithates.

The analysis we have now given of some portion—an extremely small one, however—of the author's observations on the chemical and physical properties of the urine, will suffice to show our readers the manner in which those observations were made, and in which their results are recorded. We consider it advisable to leave this division of the subject for the present, and devote such space as we have at our command to the more strictly clinical part of the work having reference to the various descriptions of change occurring in the urine in the state of disease.

The different kinds of urine met with in disease may be divided according to the writer into four grand classes, each of them presenting several varieties: these classes are the febrile, the anemic, the alkaline, the nearly natural.

(1). *Febrile urine.* The author attaches, he says, little importance to the *term* febrile, inasmuch as the condition of urine to which it applies is observed in diseases of the liver, of the heart, and under other circumstances quite unconnected with the presence of fever. The urine in question, nevertheless, most commonly, it would appear, coexists with a febrile condition. M. Becquerel divides febrile urines into three sub-classes: (*a*), Febrile urine properly so called, presenting itself in persons labouring under febrile action or intense functional disturbance, no matter what be the malady causing either: (*b*), Febrile urine occurring under conditions of debility; or in the same circumstances as the last variety, some cause of weakness, debility or exhaustion being superadded: (*c*), Febrile urine, in which the quantity of water in twenty-four hours has not been sensibly influenced, the individuals affected being in the same conditions as those of the first and second varieties.

(*a*). The mean of eleven analyses of febrile urine properly so called was as follows:

Quantity of urine	685·050
Water	660·364
Matters in solution	24·686
Density	1021·840
Urea	8·996
Uric acid	999
Inorganic salts	4·849
Organic matters	9·824

The concentration of the fluid signified by this general result is attended with increased intensity of colour and augmented consistence. Mucus and lithic acid impair its transparence; it sometimes contains a small quantity of albumen, but if so, temporarily. Excess of lithic acid does not always lead to precipitation spontaneously; cold or nitric acid are required to produce this effect. During the continuation of the same febrile action, and without any relation to the period of the affection, the urine may be one day transparent, the next turbid; the varying quantity of water accounts for these changes.

The essential condition for the production of this species of urine is febrile action, no matter what be the cause of this. Its intensity and duration united regulate the amount of the abnormal state of the fluid. And duration appears to be more powerful in its influence than intensity,—at least M. Becquerel states that this is the reason that it is not by any means rare (although the contrary may have been asserted) to find the urine natural during the paroxysm of intermittent fever, whatever be the stage during which it is voided. The same is true of continued fever, if it be of short duration and slight in amount.

The manner in which febrile action induces the diminution of water, diminution of solids, and increases uric acid characteristic of that state, is next investigated by the author. He hesitates not to ascribe the second-named change to the abstinence of fever-patients, which bears principally on the urea and inorganic salts non-decomposable by heat. The two other modifications appear to him due to congestion of the kidneys, a state of those organs of which he has repeatedly ascertained the presence in cases of various acute maladies, or chronic diseases attended with fever. Admitting this, the intimate nature of the influence exercised by congestion of the kidney in effecting the modifications referred to, is still a point to be determined.

(2). *Anemic urine.* We pass over the other varieties of the febrile class, and reach the chapter on the anemic species, of which the author admits two varieties—the anemic properly so called, and the concentrated anemic.

In anemic urine properly so called, the water is in about the natural proportion, or slightly diminished, the sum of matters in solution reduced considerably below the healthy standard; the urea, uric acid, and inorganic salts, likewise diminished, and the organic matter, properly so called, lowered in quantity also, but to a less extent. In point of quantity anemic urine is usually about natural; its density is lower than in the healthy fluid; its colour light, with a greenish hue; it never contains uric acid as a spontaneous sediment, or as one determinable by cold or nitric acid.

Among the numerous circumstances in which anemic urine is observed is the convalescence of acute diseases, especially where the patients have undergone considerable impoverishment of blood during the previous malady. Paleness, low density, and imperfect acidity, constitute natural conditions during convalescence, and such state is of better augury than if they are high coloured, dense, turbid, or strongly acid. These latter qualities denote the occurrence of fever in the evening or night, or the existence of some persistent lesion in the solids. In some cases, in the midst of a state of convalescence with its natural

anemic urine, the sudden appearance of colouring matter with turbidity may be the first circumstance announcing a coming relapse, or the supervention of a near disease. A very interesting example of this kind is related, where variola supervened in an anemic young woman recovering from articular rheumatism.

(3). *Alkaline urine.* Alkaline reaction of the urine always results from decomposition of the urea. This decomposition may occur at the moment of secretion; as we have had occasion to mention in former Volumes, M. Rayer considers this to be the case in chronic nephritis, an opinion which the author regards as difficult of demonstration: certain it is that the urea does not usually undergo change until it has reached the bladder, where the presence of pus facilitates in some cases the chemical action necessary. The causes capable of determining alkalescence of the urine are: acute and chronic nephritis; Bright's disease in some cases; prolonged stagnation of the urine in the bladder; diseases of the bladder accompanied with secretion of pus; certain diseases of the brain and medulla spinalis. In a certain number of cases the cause of alkalescence cannot be traced.

The next chapter introduces us to the modifications induced in the urine by the existence of various general morbid states of the system. Great losses of fluid are among the most important of these states,—take the example of dropsies. It has generally been said that the urine in dropsy contains proportionably to its water (which is diminished) a greater amount than natural of solids. This notion requires qualification however; all dropsies are not identical in their effects on the urine. First, when dropsy (anasarca) occurs suddenly in individuals who have not undergone previous deterioration of strength, the urine does assume the above characters, those of febrile urine,—its colour becomes deep, it is turbid, sedimentary, and becomes loaded with lithic acid. Secondly, if the dropsy have been slowly established, and connected with any of the various states which determine impoverishment of the blood, the urine will possess the qualities of the anemic class. Thirdly, in dropsy connected with disease of the liver, the urine is febrile to a very high degree. Fourthly, in cardiac dropsy the urine varies in character extremely,—it may be pale or high coloured, transparent or turbid, sedimentary or without deposit,—albuminous or not. Lastly, in the dropsy attending Bright's disease, the general tendency of the urine is to assume the anemic type.

The points we have already explained lead to the inference that various conditions and circumstances must, during the course of each particular disease, tend to modify the qualities of the urine: scarcely exists there an affection which will not at different periods of its course, and under varying conditions of the patient's general state, be attended with a urinary discharge of different types. Hence the importance, nay, necessity, of examining the excretion at various periods of maladies.

M. Becquerel has not observed "that the urine remains clear and transparent in cases of disease terminating fatally, or that it becomes turbid and sedimentary in the event of a fortunate termination." He disagrees altogether with M. Martin Solon in his announcements respecting the "critical" character of albuminous and lithic acid sedimentary urine in acute maladies. M. Martin Solon in truth only observed, as

the author, that certain sedimentary conditions of the urine occur during the course of acute diseases; and that he should have met such conditions more frequently in instances of recovery than of death, is a simple consequence of the fact that under ordinary circumstances acute diseases end more frequently by recovery than death. These are precisely the views which we four years ago (Brit. and For. Med. Rev., vol. VIII., p. 143, July, 1839,) took of the bearing of the statements made by M. Solon; and we cannot forbear from noticing how completely the experience of M. Becquerel, as referred to in several places of the present article, supports the ideas frequently advocated in this Journal respecting the utter groundlessness of the fashionable creed of "critical" discharges. The more matured our experience grows, the more firmly persuaded do we become that a greater delusion than this doctrine of "critical discharges" never sprang from *à priori* reasoning, careless observation, and love of the marvellous.

It is to be remembered too, that in estimating the qualities of the urine in certain diseases and in regarding them as dependent upon these, a broad source of error lies in the non-appreciation of the changes effected by the nature of the food, drink, and medicines ingested during sickness. It is only observers who, like him whose work we notice, bear in mind all such modifying circumstances, and who labour upon so vast a number of cases, that slight errors correct themselves in the general result,—it is only men of this stamp that deserve a patient hearing when they announce the issue of their toils. And when we see young gentlemen or old ones boiling a few drops of urine, or mayhap dropping nitric acid into the same a dozen or so times, and on the strength of experience in this wise besieging the editors of Journals with their "Chemico-practical" papers, or even manufacturing volumes thereupon, we cannot avoid a pitying smile at the vanity of these petty dabblers, nor at the same time help lamenting a state of literature which fosters the production of such outrageous abortions.

We have reached the Third Part of M. Becquerel's volume. Here are considered the modifications arising in the urine in the course of particular diseases. We shall select two of these for the subject of some remarks founded on the details given by M. Becquerel. These details are so close, so minute, and so full, that in themselves they will not bear condensation without risk being incurred of misinterpreting their laborious author's meaning; and our space is so limited that complete extraction of the history of any one disease is beyond our means.

Scarlatina. In connexion with the subject of scarlatina the true nature of the anasarca occasionally attending the convalescence or advanced periods of the disease is most luminously discussed. From the writer's researches, conducted at the Children's Hospital in Paris, he finds himself justified in inferring that a certain number of these dropsical affections (the proportion will be presently given,) are the consequence of Bright's disease, and that others cannot be legitimately referred to that cause. Among these dropsical affections some originate and advance after the manner of acute diseases; others follow a chronic course. The latter generally occur in enfeebled and anemic subjects; they may terminate by recovery of health, but in a considerable number of cases the patients succumb under the influence of the dropsy and of increasing

debility, the fatal issue being in some instances hastened by an intercurrent malady. In all these cases the urine presented the following characters: colour of slight depth, sometimes clear, greenish, or at others a little turbid; slight acidity; density below the natural standard; diminution of the solid constituents (anemic urine.) And in one case when the dropsy was unconnected with Bright's disease, the author found a notable quantity of blood in the urine. Albumen was far from being constantly discoverable, and when it did exist it was only in small proportion and at an advanced period of the disease. Consequently it appears that albuminuria in these dropsical subjects instead of preceding the dropsy, as in Bright's disease, occurred only as a consecutive and secondary phenomenon in the limited number of cases in which it existed at all. Nevertheless there is a great and close degree of similarity between the two kinds of urine, that of the dropsy we are now speaking of and of Bright's disease; both are anemic, but the constancy and large amount of the albumen contained in the urine distinguish the latter. The diagnosis must be excessively difficult in some cases; impossible even, where the examination of the urine has not been undertaken until a certain time after the supervention of the dropsy, the small quantity and capriciousness of appearance of the albumen in the non-renal anasarca seems then the only circumstance capable of aiding us in the distinction. Yet it is most important to be able to distinguish the two causes, for the prognosis is materially different in them.

Children dying with dropsy after scarlatina, and without Bright's disease, present the following conditions on post-mortem examination: The subcutaneous and sometimes the deep intermuscular cellular tissue is infiltrated with serosity. The peritoneum, and in some subjects the pleuræ and pericardium, contains serosity, generally straw-coloured and transparent. All the organs are remarkable for their pallor and anemia; some of them infiltrated with serosity,—for example, in many cases the submucous cellular tissue of the coats of the intestine. The lung is almost always distinctly and considerably œdematous; the liver soft, flaccid, pale, and on incision giving issue to serosity. The kidneys present peculiar appearances, which are referrible to œdema of their substance, occurring in two degrees. In the first degree the kidneys appear harder and more swollen than natural; the capsule may be completely separated from them; under this the tissue appears of a dull white colour, (without any trace of yellow hue,) interrupted here and there with vascular streaks. The entire of the cortical substance, even that prolonged between the tubuli, appears of the same colour, and presents neither granular texture nor vascularity. The tubular substance presents its natural form, and by its reddish hue stands out in strong relief from the surrounding pallid ground. Pressure forces out a certain quantity of serosity from the altered tissue; and when this is removed, the kidney loses its swollen and turgid aspect, and becomes soft and flaccid. This change may affect the entire or a part only of the cortical substance; in the latter case, it would appear, especially the neighbourhood of the hilus.

In the second and more advanced stage, the kidneys are decidedly larger and harder than natural; the capsule continues easily separable; the cortical substance is of a slightly yellowish white colour, or perfectly white and shining. No vascular appearance is visible; serosity in greater quantity is expressible as before; the tubuli look paler, but still are ob-

viously distinguished by colour from the cortical encompassing them. The affection is always spread over the entire of the two kidneys; and it can only be regarded as a very advanced evidence of the general tendency to dropsy, like the infiltration of the liver, &c. The albumen in the urine in such cases appears to be furnished by the serosity pervading the renal tissue, which makes its way by a sort of infiltration or transudation into the excretory passages; be this as it will, the author's cases justify him in the inference that in the present kind of dropsy, the urine only contains albumen when the kidneys participate in the general serous infiltration of the body. In some cases of dropsy after scarlatina, where the urine contained no albumen, the author found the kidneys slightly paler than natural but otherwise unaltered.

From all that has been said it follows that during the period of desquamation of scarlatina, either of these kinds of dropsy may develop itself. 1. Acute dropsy with fever, without albumen in the urine. This is manifestly the rarest form; the author has not himself had occasion to observe it. 2. Dropsy produced by Bright's disease. 3. Dropsy such as we have just with some minuteness supplied the author's remarks upon. This species he conceives to be indubitably the most frequent.

We shall close the subject of scarlatina with a tabular statement of the history of 31 cases of the disease observed by the author at the Children's Hospital or La Charité.

Of the 31 patients 16 died, 15 recovered; an amount of mortality which the author endeavours to account for by the unusual severity of the disease in the entire number of cases. Of the 15 cases of recovery, 3 only suffered from anasarca: in one of these cases there was no trace of albuminuria; in the other two the urine was strongly albuminous and the symptoms of Bright's disease present—in these recovery was much slower than in the other case.

In the 16 fatal cases:

6 were complicated with dropsy; as follows:

6	1 with no disease of the kidneys.
	3 with œdema of the kidneys.
	2 with Bright's disease to a marked amount.

10 died without dropsy; as follows:

10	2 Bright's disease, acute, with albuminuria.
	1 Suppuration of tonsils aud pneumonia.
	1 Gangrene of tonsils.
	1 Pseudo-membranous inflammation of the mouth.
	1 Capillary bronchitis.
	1 Bronchitis and inflammation of intestines.
	1 Pneumonia.
	1 Gastro-enteritis.
	1 Cerebral disease.

All these facts sufficiently prove the important point that congestion of the kidneys is far from being a constant condition in scarlatina. They likewise show that Bright's disease is a rare complication of the cutaneous affection (4 times in 31 cases.)

Diseases of the spinal marrow. We shall next condense the author's observations upon the condition of the urine in diseases of the spinal marrow. These diseases may, in respect of their relation to the urine, be divided into two classes: in the first of these there is no disorder of function either in the kidneys or bladder; in the second the urinary functions are more or less disturbed. 1. In the first class of cases the urine

presents no deviations from the natural condition; though there is always a tendency, from the persistence of the spinal disease, to the passage of the case into the second class. This is a very important preliminary point to determine; it shows that when modifications of the urine do attend this class of diseases, they are not the *direct* produce of the medullary affection, but immediately caused by some intervening condition.

2. In the second class of cases there is either (*a*) retention of urine, which is a rare phenomenon, and often accidental, or (*b*) involuntary discharge of the urine, which is on the contrary very frequent. Under such circumstances the urine is always altered in character; it becomes alkaline, of a dirty yellow colour, pale, and of about normal density. In other cases it becomes clearer and less dense than in health. Such urine almost always containsin suspension or solution all or some of the following principles: abundant opaque mucus, muco-pus, or pus; a little albumen, especially in the latter cases; triple phosphate of ammonia and magnesia; pulverulent phosphate and carbonate of lime. Now is such urine a product of vitiated secretion of the kidneys, or is it due to the chronic inflammation of the bladder, which so often accompanies advanced disease of the spinal marrow? The author considers the question one not easy of solution; for when we consider that the paralysed condition of the bladder renders the escape of the urine from that viscus immediate, we should be tempted, à priori, to regard the urine as a direct product of renal secretion. Without doubt such is, too, the fact in some cases; and the kidneys have frequently been found diseased, to confirm the supposition. But the author is of opinion that more commonly the following are the real phenomena of the case. In the first place the evacuation of the contents of the bladder does not occur immediately upon these contents making their way into it; the bladder must first be filled and distended with fluid, and then any excess escapes. The urine stagnates in the blood consequently, and from this stagnation arises a spontaneous alteration of the fluid. Some authors deny the reality of such *spontaneous* change; M. Becquerel, admitting their correctness for a moment, at least contends that the repeated and continued stagnation of the urine must induce inflammation of the mucous membrane of the bladder. Once this cystitis is developed, the muco-pus and pus secreted, alter the urine and give it new properties. These notions are not new, as the reader will observe; but we are glad to find them adopted by so practical a writer as M. Becquerel: for we confess that to have the mere chemist talking of the purulent secretion acting as a ferment on the urine gives us but little satisfaction and confidence in the correctness of their doctrines. If the kidneys themselves become inflamed in consequence of extension of the disease from the bladder, they may, M. Becquerel admits, *secrete* alkaline urine.

We must now for the present (and for the second time) take leave of M. Becquerel's labours. It is but a small part of the encomiums to which these labours have justly entitled him to say, that his book is the best existing guide to the pathologist desirous of making himself familiar with the changes of the important fluid of which it treats, and anxious to learn the inferences in respect of diagnosis and prognosis derivable from a profound consideration of those changes. The work is conceived in a spirit of philosophy which redounds as highly to the honour of the writer's intellect, as the extraordinary labour, with which his researches have been conducted, clearly evince his deep love of truth and powers of industry.

ART. IV.

Deformities of the Spine and Chest, successfully treated by Exercise alone, and without Extension, Pressure, or Division of Muscles. By CHAS. H. ROGERS-HARRISSON, M.R.C.S. &c. &c.—*London,* 1842. 8vo, pp. 164.

THE title of Mr. Harrisson's work would seem to imply that it was merely intended to explain and recommend a particular plan of treatment in deformities of the spine and chest; but we learn from the "advertisement" that it is offered to the profession, and, by the way, to the non-medical world too, as a complete treatise on these affections. The following extract conveys, in the author's words, his own estimate of the nature of the book.

"My object has been to make this an easily intelligible, and, if possible, a useful book, both to the young practitioner and to the *enlightened general reader.* I have therefore generally *supposed very little to be previously understood, have not withheld even elementary details,* and have illustrated by simple lithographic outlines whatever might have been less intelligible without them. I have also drawn from all sources, English, French, and German, everything that I thought would render *the work tolerably complete.*"

The italics in the above quotation are our own. We have introduced them to designate those passages which show that the work purports to be a complete treatise on deformities which constitute its subject; and that it is consequently a legitimate object of inquiry to examine how far this promise has been fulfilled.

Mr. Harrisson, in conformity with the plan above announced, commences with a description of the "normal structure of the parts liable to deformity;" and devotes forty pages, with eleven lithograph illustrations, to an account of the bones, ligaments, and muscles of the spine and thorax. In a work such as the present, we would be far indeed from desiring any laboured display of minute anatomical knowledge; but as the author has made his election to enter into anatomical details, we have a right to require that what he does say should be precise and accurate, and that, at the least, every point having a direct practical application should be clearly stated and its importance distinctly indicated; more particularly when the writer has "supposed very little to be previously understood," and premises that he has "not withheld even elementary details." But although a quarter of the entire book is occupied with anatomy, what is said on the subject is imperfect, inaccurate, and practically useless. We have neither sufficient time nor space at our command to point out the defects which we have noticed in this respect; but our readers may rely on finding our judgment confirmed by a perusal of Mr. Harrisson's volume. We must say a few words on the parts of the work having a more direct bearing on pathology and practice; and in doing so it will be our unpleasant duty to expose a remarkable instance of unacknowledged appropriation, on the part of the author, of another writer's labours.

The account of the slight lateral convexity to the right that naturally exists at the upper part of the dorsal portion of the spine, is singularly inaccurate and confused. At page 8 it is said to exist "sometimes,"

while at page 85 we are told it exists "early in *all* persons." As to its cause, Mr. Harrisson unhesitatingly adopts Bichat's theory, that it is due to the predominant action of the right arm. But he quotes Bichat (p. 80) as if he attributed the morbid and not the natural lateral curvature to this cause. At page 8 we are told that Cheselden erroneously believed this natural deviation to be designed "to make more room for the heart." We do not mean to advocate Cheselden's opinion, but as Mr. Harrisson noticed it, he should have adverted to the intimate relation, pointed out by Serres, that exists between the position of the heart at the left side of the spine and the usual direction of the dorsal curvature; at all events it is a curious coincidence that in preternatural lateral deviations of the spine, the heart and the liver, in the vast majority of cases, correspond to the concavity of the primary and secondary curvatures, which must to a certain extent tend to prevent their functions being disturbed. At page 53, Mr. Harrisson seems to misapprehend one of the theories that have been advanced on this subject: the author is there considering the causes of *morbid* lateral curvature, and says, "the extreme frequency of this curvature has even made many anatomists believe that it is the effect of some natural disposition, and they have attributed it to the presence of the arch of the aorta which beats against the left side of the anterior part of the bodies of most of these vertebræ, which by deviating from their natural direction commonly constitute it." This is certainly as strange a misconception as we recollect to have met with, unless indeed the inaccuracy is to be set down to mere slovenliness of expression. No anatomist ever attributed *morbid* lateral curvature of the spine (of which Mr. Harrisson is most distinctly speaking in the passage quoted,) to the pulsation of the arch of the aorta; many have, indeed, accounted for the great frequency of the *disease* by the existence of its *rudiment* as a *natural* disposition; and the existence of this rudiment some have referred to the presence of the arch of the aorta. Mr. Harrisson seems to misunderstand, and certainly ridicules, the theory of these anatomists. If their theory was what he represents it to be, it would certainly be absurd enough; but the influence of the presence of the arch of the aorta in producing the *natural* curvature so often mentioned, though perhaps much exaggerated, should scarcely perhaps be altogether rejected, when we know that in some instances of transposition of the viscera this curvature has been to the left, as was observed by M. Grisolles, for example, in two cases; and when we further recollect, as Cruveilhier has expressly remarked, that this is possibly but a particular instance of the general law, that bones present a depression whenever a large artery is in their immediate vicinity. The reader may perhaps suppose that we have dwelt on this point too long, but it is an average example of the confused and disconnected way in which Mr. Harrisson treats every subject he touches; and besides this was a matter which he should have discussed in the fullest and clearest manner, as he adopts Bichat's view respecting the generation of this natural deviation of the spinal column, and extending the same principle to morbid deviations, makes it one of the main foundations of his theory of the etiology and treatment of the affection.

It is very difficult to discover what Mr. Harrisson's opinion respecting the etiology of lateral curvature of the spine really is (p. 43.) He at-

tributes the affection to a derangement of the equilibrium of muscular power, saying, "In fact we attribute these deformities to a want of equality of power which ought to have existed between the muscles of the back having attachment to various points of the spinal column, and exerting an action on the body." But at pages 50-1, the muscles which thus cause the deformity are said to be only altered in consequence of that deformity, from "the effects of such a change in the structure of the bony framework to which they are attached. Those whose insertion is *at a distance* must naturally be distended and elongated," while "the muscles whose attachments *are near* are, on the contrary, reduced to a state of *relaxation* which tends to contract and shorten them." Strongly confused as is this paragraph, we may yet guess at what the author intends to convey. The *distance* or *proximity* of the attachments of the muscles, of course, has nothing to say to the matter; but we suppose Mr. Harrisson's meaning to be, that the muscles which correspond to the convexity of the curvature are distended, while those on the side of the concavity are *relaxed*, and if they be relaxed of course they could have no influence in producing the deformity. At page 74, however, we are distinctly told that "the inequality of the muscular power of the spine is *rarely primitive*, but appears to be *almost always* owing to a continued position of that part in the same vicious direction." The illustration of this proposition is amusing. "A child whose *spine is deformed*, in standing upright for some time, generally does not keep its feet in the same line, but one is placed before the other. Curvature, however temporary, is the result." (p. 75.) That is to say, a certain deformity produces a certain position, and this position produces the deformity of which it is the result, in other words, a cause is produced by its effect. The author afterwards reverts to this original position, and again maintains that inequality of muscular action is the predominant and most frequent cause of lateral curvature of the spine.

"It must then be evident to all who do not decide upon the cause of any abnormal state without taking into account the physiological phenomena which attend its development, that this curvature is, *in almost all cases*, the result of an increase in the vital energy, and, consequently, in the physical development, which the muscles of the left spinal hollow and of the right shoulder acquire, from the habit early taught to children, of making use much more frequently of the right than of the left hand." (pp. 87-8.)

But though the preponderant use of the right arm is thus stated to be the cause of the deformity in "almost all cases," great importance is attached to the malign influence of "that circular instrument of constriction applied to the chest, and known under the name of stays," (p. 56,) and to a bad carriage or faulty attitude unduly persevered in, (p. 72.) Mr. Harrisson employs twelve pages in expounding the ills that flow from the use of tight stays, and strongly objects to them not only as a physician but as a man of taste.

We are far indeed from undervaluing the injurious effects of tight-lacing, or of a faulty attitude, and still further, of course, from blaming Mr. Harrisson for having noticed and dwelt on those points, which have an intimate and important connexion with his subject; but we do blame the loose, vague, inconsistent and contradictory statements which he makes respecting the causes of spinal deformity.

Mr. Harrisson, we have seen, refers almost all cases of lateral curvature of the spine to the custom of using the right hand more frequently than the left. In explaining how this habit produces the effect in question, he adopts Pelletan's theory without the slightest acknowledgment, and, not content with this, borrows the very language in which it is conveyed.

Before we proceed to establish this charge of plagiarism we must premise that Pelletan's views are published in his *Réflexions sur les Causes qui determinent la Courbure de la Colonne vertébrale*, (Journal de Maisonnale, t. ii, p. 372,) which we have not an opportunity of consulting, but an abstract of his theory is given by M. Malgaigne, (Anat. Chirurg, t. ii, pp. 18-19). Whether Mr. Harrisson has copied directly from Pelletan's memoir we do not know, but certain it is that the entire passage which we shall immediately quote from M. Malgaigne appears in Mr. Harrisson's book translated almost literally, though not inserted exactly in its original shape, as its continuity is broken up by the intercalation of numerous sentences and several entire paragraphs. We do not know whether the superadded matter exists in Pelletan's original memoir or whether it has been inserted by Mr. Harrisson himself. The only difference, however, that this makes is, that in the former case Mr Harrisson has borrowed more than we have been able to detect, in the latter he has taken no small pains to conceal his obligation to M. Malgaigne, and we should rather hope that the former of these two alternatives may prove the correct one. The following are the passages from M. Malgaigne, and their unacknowledged translation by Mr. Harrisson; the preceding observations sufficiently explain the broken shape in which the latter extract appears.

"Toutes les fois qu'une partie du corps doit exécuter des mouvemens sur son ensemble, la première condition est la fixité des points auxquels s'insèrent les muscles qui vont la mouvoir. Le bras ne saurait agir par ceux de ses muscles qui s'attachent à l'omoplate, que l'omoplate ne soit solidement fixée, et elle ne peut l'être qu'autant que ses muscles propres, le trapèze, le rhomboïde, l'angulaire, le grand dentelé, trouvent eux-même un point fixe ailleurs; ce point fixe pour les trois premiers est la colonne vertébrale.

"Chez un sujet à muscles forts, à ligamens serrés, les muscles propres à la colonne suffisent pour la maintenir droite et stable; un sujet faible n'y arrive pas toujours, mais il obtient cette stabilité d'une autre manière. Ainsi le rachis dans la rectitude est un corps élastique et flexible que le moindre effort peut entrainer dans une direction quelconque. Mais si cette tige vient à être courbée, elle offre tout-à-coup une rigidité proportionnelle à l'éntendue de sa courbure et qui résulte d'un antagonisme entre sa force élastique qui tend à la redresser et la puissance qui la courbe. Voyez le tireur en garde dans l'escrime, il a le rachis fortement courbé à guache; l'enfant qui s'essaie à ouvrier de la main droite une porte qui résiste est contourné en arc presque demi-circulaire, etc. Ils courbent la colonne pour lui donner plus de fixité.

"Or cette fixité nécessaire aux mouvemens énergiques ne l'est pas moins à des mouvemens légers, mais qui demandent beaucoup de précision. La jeune personne qui brode au plumetis, qui dessine ou qui écrit, doit se premunir contre la vaccillation du rachis qui entraînerait celle de l'épaule et de la main; elle le tient donc légèrement courbé dans sa partie supérieure; cette flexuosité, qu'on regarde généralement comme une habitude vicieuse, n'est autre chose que la condition indispensable pour un sujet faible de l'exécution de certains actes du bras droit. Aussi est-il très souvent impossible, malgré les remonstrances les plus fréquentes, d'obtenir de la jeune fille la plus docile cette rectitude tant désirée des mères de famille." (Malgaigne, Traité d'Anat. Chirurg., etc., t. ii, pp. 18-9.)

"Every time any part of the body has to execute a motion, the first condition of the motion is to fix the points to which the muscles which have to move it are attached . . . . . In the motions of the thoracic extremity the shoulder must be fixed . . . . . The shoulder-blade, which is to present to the motions of the arm the fixed points which are requisite for them. . can be rendered immoveable only by the simultaneous contraction of the trapezoid, rhomboid, etc. . . but it becomes evident that the shoulder-blade itself cannot be fixed in an immoveable manner, unless the superior part of the vertebral column, and the head present in their turn a fixed point for the trapezoid,* *angular*, rhomboid, etc.

"In an individual well made and in the enjoyment of all her muscular vigor the habitual efforts of the superior extremities may be carried on in the manner just spoken of; but in one whose muscular powers are extremely reduced, the muscles of the neck........may not be sufficient to give a proper stability to the upper part of the dorsal region, and nature........gives to this delicate being means of another kind........The spinal column, when upright, is an elastic and flexible body, which the least effort may put into any direction whatever...... .But if a straight elastic pillar can be made to bend by the slightest effort, this same elastic pillar........offers at once a rigidity which is proportioned to the extent of the curvature, and which is the result of an opposition between the elastic power which tends to straighten it and the power which curves it........The fencing-master, on guard, has his spinal column forcibly bent to the left side; the child who attempts to open a door with the right hand is bent to almost a semi-circle.

"We must further remark, that the precision and exactness of certain delicate movements require as much the upper portion of the spinal column to be fixed, as certain abrupt and powerful motions. A young person embroidering at a frame, drawing, or writing, is obliged to guard against the vacillation of the spinal column, which would produce that of the arm and hand; and consequently preserves it curved in its upper part, however slightly........We now see that the bending of the spinal column in the superior part of the dorsal region........ instead of being the effect of a vicious habit, is no more than an indispensable condition for the execution of certain motions of the right arm, so that........it is often quite impossible, in spite of the most frequently repeated care and remonstrances, to obtain from the most docile and submissive young person that erectness so desired by mothers." (Harrisson on Deformities of the Spine, &c., pp. 80-4.)

We thus find that *fourteen* disconnected fragments, scattered through *five* pages, constitute, when the intervening passages are omitted, a continuous, close, and, for the most part, literal translation of the passages quoted from M. Malgaigne's work! We leave it for Mr. Harrisson to explain and account for this strange circumstance: we have felt it our duty to expose the fact thus openly. We took care to quote at the commencement of this article Mr. Harrisson's general acknowledgment of having drawn freely from all sources whatever he thought would render his book tolerably complete; but we cannot regard this as in the least justifying the appropriation of the precise ideas and language of other authors, whose names are never mentioned: we cannot even regard the general confession as an extenuation of the particular offence; indeed, it might easily be made to appear an aggravation of it. Mr. Harrisson himself evidently cannot regard the general acknowledgment as sufficient, for he in several instances mentions by name authors whom he quotes.

Our remaining observations on the present work must be brief. The anatomical characters of the parts engaged in lateral curvature of the spine are scarcely noticed. We are indeed told, at page 42, that

* The French name retained in the translation.

the intervertebral fibro-cartilages and the bodies of the vertebræ are diminished in thickness on one side; but it is erroneously added, "on the contrary, the other side increases in thickness." It is not stated that the cartilages are absorbed to a much greater extent than the bones; nor is there any allusion to the change that is effected in the symmetry of *every part* of the two sides of the affected vertebræ. The curious and most important fact, that the vertebræ, when lateral curvature has existed for any length of time, undergo a kind of rotation or torsion, is indeed mentioned under the head of "accessory deviations" (p. 92); but the nature of the alteration is most imperfectly given. M. Pelletan's explanation of the mode in which this torsion occurs is here again quietly adopted without his name being mentioned, and the French text is literally translated without interpolation or alteration! The reader, however, is not informed of the objections that have been made to this explanation; that several others have been proposed; and that the question has a direct and important bearing on the treatment of lateral curvatures of the spine,—M. J. Guerin's method of *sigmoid extension*, for example, being based, in part at least, on this view (as we think erroneous) of the mechanism by which this torsion is effected.

We shall not enter into any lengthened consideration of the mode of treatment recommended by Mr. Harrisson. He is vehement, even to vituperation, against the employment of all mechanical means; maintains that those who adopt them are "guided rather by the love of gain, than the interest of science and the love of public good," (p. 109;) and that "they have not furnished one single well-authenticated instance of cure." (p. 110.) But he admits that they have sufficient intellect to have "been capable of observing that the best means of refreshment, after long fatigue, is repose." (p. 109.) Now, we freely admit that the employment of mechanism in the treatment of distortions of the spine has been monstrously abused; that it has been and is a fruitful source of charlatanism and the vilest quackery; and is daily applied in numerous cases without any the slightest necessity. We are further well aware that the application of mechanical means has often done more harm than good; is not unfrequently inefficient; and when it does good is too often tediously slow in effects. All this we admit, insomuch so that we are firmly persuaded the best mode of treating considerable lateral curvatures of the spine is a problem yet to be solved; but it is something more than absurd to maintain that mechanical means have never effected "one single well-authenticated instance of cure;" and it is, at the least, indecent to brand, "as being solely guided by the love of gain," the many conscientious practitioners who have employed them.

Mr. Harrisson recommends what he terms the "rational treatment," which means attention to position and certain gymnastic exercises. Mr. Harrisson even claims some originality on this score; he admits that

"The medical men most skilled in this department have, indeed, in their writings and in their practice inculcated the necessity of gymnastics in the general treatment of some of these diseases........but they only provided for a secondary requisite; let us provide for the primary one; they have only opposed the progress of the disease; let us endeavour to point out the true means of curing it in a very great majority of cases." (p. 127.)

And again,

"I hope by assigning to the different muscles of which the vertebral column supports the action, the share which each may have in the production of curvature, and by reducing all to *geometrical data* to be able to prove that if gymnastic exercises, which, in the greater number of cases, ought to form the basis of the treatment of this deformity, especially at the commencement, have not always been productive of good effect, it is because they have not been prescribed in a distinct manner; and few have pointed out the particular kind of exercise required by each species of deviation." (pp. 128-9.)

We have in vain looked for any reduction of the matter to *geometrical data*, or for one tittle that is new in the directions for the application of gymnastic exercises. With respect to lateral curvature, the whole of the *geometrical data* and of the promised precision consist in sixteen lithograph sketches (with explanatory letter-press) of young ladies performing "the military extension motions," "the British military club exercise," and the "Indian club exercise." There is no notice of the equilibrium exercises recommended by Delpech, and which are perhaps the only ones admissible when the curvature is advanced; and the inefficacy and occasional prejudicial consequences of the exercises that are recommended, when the curvature is considerable, are not adverted to.

Art. V.

Dr. Tavernier's Treatise on the Treatment of Deformities of the Spine by the "Lever-belt with Inclination Busk," without Extension Beds or Crutches; containing a Relation of new results obtained, and the Class of Cases in which the Belt may be safely applied. Translated into English, with a Critical Analysis of this Division of Orthopædia, by W. Brewer, m.d., London.—*London*, 1842. 8vo, pp. 43.

It was our original intention to place the present notice with the preceding, under one head, as having both reference to the same general subject. Further consideration, however, points out the propriety of keeping them distinct yet not far apart.

Dr. Tavernier's pamphlet is an inflated eulogium of the "Lever-belt with inclination busk." This instrument was invented by one Hossard, a French quack, whose name, by the way, is but once and incidentally mentioned throughout the entire treatise. Dr. Brewer, the translator, seems, however, to imagine that Dr. Tavernier is the inventor of this machine, for he prefixes to his translation a French dedication to that gentleman, in which he asks, "À qui pourrais-je dédier mon travail mieux qu'à vous-même? *à vous* dont *la belle Découverte* a bientôt frappé mon attention;" but if Dr. Brewer conceives that the homage of a dedication should be paid to the author of the "belle découverte," it is to Hossard the quack that he should have rendered it. This mistake on the part of the translator is the stronger, as Dr. Tavernier, though chary of mentioning Hossard by name, by no means claims the invention for himself; on the contrary, he expressly says that "a stranger to the medical body happened to light upon it by one of those sudden inspirations," &c. (p. 7;) and goes on to tell how the inspired stranger "boldly knocked at the door of the learned societies, holding his orthopedic bandage in his hand." (pp. 7-8.) Dr. Tavernier is only the apostle of the discovery, and Dr. Brewer is the missionary for the English district.

No one could ever suspect from the present pamphlet what the true history of the lever-belt really is; and as the facts of the case are not devoid of interest, we shall state them succinctly, especially as so doing will also enable us to appreciate Dr. Tavernier's treatise more connectedly and concisely than would otherwise be practicable.

Hossard, we believe in 1833, presented his lever-belt to the French Academy as a most efficient means of remedying spinal deformities, and a commission was appointed by that learned body to inquire into and report on its efficacy. Hossard presented several patients affected with lateral curvature of the spine to the commissioners, who were to inspect them again when the treatment was completed, and then draw their conclusion as to its results. Upwards of a year, however, elapsed without anything being heard of these patients; but during the period that the matter was thus sub judice, Hossard diligently advertised his "belt" as having been solemnly "approved of" by the Academy of Medicine, (Gazette Méd. de Paris, 1835, p. 123,) and caused handbills to that effect to be distributed on the Pont Neuf, and in the streets of Paris after the most Goss and Lamert fashion. Some members of the Academy having complained of this scandalous proceeding, Hossard, in 1835, again placed himself in communication with the commissioners, and the result was that M. Bricheteau at length, on the 8th Sept. 1835, (Gaz. Méd. 1835, p. 605,) reported to the Academy that Hossard had submitted to the examination of the commissioners three patients, two of whom laboured under the ordinary form of lateral curvature of the spine, while the third, named Janny Guery, was affected with a *single* and very considerable dorsal curvature; and that after a treatment of four and a half months, the last-mentioned patient and one of the former were perfectly straight, while the other was considerably improved. The commission consequently concluded—we shall here borrow Dr. Brewer's translation—"That the lever-belt, presented by M. Hossard, combats with much energy, and redresses as quickly the lateral curvatures of the vertebral column; and as to the application of the means, they do not appear to the members of the commission to subject the patient to any inconvenience during the treatment which they have observed." (p. 9.) *But that time and new experiments are still requisite to allow of all the advantages of the method being definitively appreciated.*" MM. Villermé, Moreau, Villeneuve, Roux, Double, himself one of the commissioners, and others, during the discussion on the Report, expressed their apprehensions that so much of those conclusions as were decidedly favorable would be turned to trade account by Hossard, but that the qualifying clause which we have underlined would be suppressed. The foresight of these gentlemen has been justified, for this clause is omitted by Dr. Tavernier, who, we should state, is, or at least was, medical consultant to Hossard's orthopedic establishment. This report we have said was read on the 8th Sept., and on the 15th of the same month, M. J. Guerin addressed a letter to the Academy, in which he stated, 1st, That the curvature was simulated in the case of Janny Guery, who was waiting-maid to Me. Hossard; and 2d, That the other two patients had been treated by Hossard for many months before they were presented to the commissioners, who were consequently deceived as to the time really occupied in the treatment, which, in point of fact, had been sixteen or seven-

teen months, instead of four months and thirteen days, and that in these cases, too, the amount of deformity that really existed had been collusively exaggerated. (Gaz. Méd. 1835, p. 606.) On the receipt of these allegations the Academy enlarged the original commission, and directed the matter to be investigated. Accordingly on the 12th Jan. (Gaz. Méd. 1836, p. 42,) and 2d Feb. 1836, (Gaz. Méd. p. 92,) M. Paul Dubois brought up two Reports in succession, both of which agreed in stating:

"1st. It is certain that as regards the first two patients, M. Hossard *knowingly deceived* the commissioners of the Academy, by representing them as not having been yet submitted to any treatment, whereas he had treated them during many months at Angers. 2d. As to the third patient, waiting-maid to Me. Hossard, the documents brought forward by M. Guerin to prove that she was not deformed before her journey to Paris, are contradicted by certificates produced by M. Hossard, which go to establish that Janny's deformity might have been overlooked at Angers, as she concealed it by the arrangement of her dress, etc. Consequently, the commission, while severely blaming M. Hossard for fraud respecting the two former patients, thinks the accusation is not so well proved (n'est pas aussi bien prouvée) as respects the third, and concludes that a copy of M. Bricheteau's first report may be transmitted to M. Hossard, but only on condition that there is annexed thereto a copy of this second report, containing the disapprobation of the Academy."

We do not enter into the subsequent proceedings, in the course of which a commission, consisting of MM. Breschet, Amussat, Orfila, and Cruveilhier expressed their conviction that the deformity of Janny Guery was simulated, because we conceive their conviction was founded on inadequate proof; and indeed it is tolerably clear that these learned academicians were unwittingly influenced by a desire to throw the shield of their protection over M. Guerin, against whom Hossard had instituted an action of libel for some strictures which exceeded the legitimate bounds of scientific discussion.

All these details are carefully avoided in Dr. Tavernier's treatise. He appeals to the approbation of the academy as conclusive proof of the value of the "lever-belt," and represents that approbation as having been unqualified. Thus he says: "When a scientific medical discovery hath won the public, and authentic approbation of a learned body, like that of the Académie Royale de Médecine......it can, resting on public notoriety, meet calmly the disdain; see even the reproach of some critiques, stirred up by any interest but that of science; and, in the assurance of success, pass judgment on those who are unable, or have not the knowledge to appreciate it," (p. 8;) and again, in speaking of the three patients reported on by the first commission, he says: "It is clear these experiments had every possible authenticity, and their importance was enhanced by the effect of *new examinations*, to which they were subjected, in *consequence of many long and warm academical discussions.*" (pp. 8-9.)

The brief but faithful abstract which we have given of these "long and warm academical discussions," will enable our readers to judge of Dr. Tavernier's candour in representing them as having enhanced the value of the evidence in favour of the lever-belt.

It is right to mention that a commission was also appointed by the Academy of Sciences to report on the efficacy of the "lever-belt," but this commission, we believe, never made any report on the subject; in this we may possibly be mistaken, but we may, at all events, assume that the report, if made, was not very favorable, as Dr. Tavernier, though

he repeatedly alludes to the experiments performed before the commissioners of the Institute, makes no mention of their report. Dr. Tavernier assumes that these experiments are so much proof in favour of the lever-belt, because they were made in presence of those commissioners, but of course, in the absence of any report on them, they must be taken as Dr. Tavernier's assertions and nothing more.

We should not have devoted so much space to this pamphlet did we not entertain a strong conviction that the "lever-belt" is really a very valuable instrument in the treatment of lateral curvature of the spine; and it is precisely because we entertain this conviction that we thought it right to reduce Dr. Tavernier's testimony in its favour to its proper value, and to show, as we think we have done, that his evidence must be taken with very considerable limitation. Our experience of it, we must at once acknowledge, has been very limited; we do not happen to be professed orthopedists, and have applied it in but two cases, but in these its effects were most satisfactory. Having then this impression in favour of the machine, but being well aware how unwarrantable it is to put forward a positive conclusion respecting the value of *any* method of treatment, until it has gone through the ordeal of numerous trials in the hands of different practitioners, and believing that nothing can injure any invention more than trying to bolster it up by false facts, we have thought it our duty to examine those chiefly relied on by Dr. Tavernier, and trust we have placed our readers in a predicament to enable them to draw their own conclusion respecting them. We shall now endeavour to explain what the lever-belt is, and how it is to be applied, though we fear our description will not be perfectly intelligible without the assistance of a figure.

The apparatus consists of a broad leather belt, which fits round the pelvis, and has a kind of semicircular steel rack attached to it at a point corresponding to the sacrum. A steel bar or busk, which fits vertically into this rack, can be placed and firmly fixed in any requisite degree of lateral inclination. A broad leather strap is fastened to the belt in front, and is destined, after being brought obliquely round the body, to be fastened by two or more tongues to studs towards the upper part of the busk. The belt round the pelvis is steadied by a strap passed underneath the thigh, in order to render it a sufficiently fixed point of resistance.

To apply this instrument, suppose in a case of right lateral curvature, the belt is first fastened rather loosely round the pelvis, and the busk is inclined laterally towards the left shoulder, the degree of its inclination depending on the amount of the deformity. The strap, which is fastened to the belt in front, is now passed obliquely in front of the thorax and under the right arm, so as to pass over the ribs, which project in consequence of the spinal deformity. The length of this strap is so adjusted that it cannot reach the busk while the body is erect, and the trunk must therefore be bent to the left side, and while it is in that position the strap is fastened to the busk. The patient is now obliged to endeavour to throw the body to the right side, in order to restore the equilibrium; and, to use Dr. Tavernier's words, "by this movement, which that part of the spine situated above the strap can alone perform, the upper half of the arch, which the curved spine presents, finds itself of necessity brought back, in conformity to the axis of the body." (p. 19.)

We may quote the following explanation of the mode of its action:

"It is then, as we may see, by the combination of two actions, one mechanical, and produced by the apparatus, the other physiological, resulting from the play of the muscles, that the method of the lever with inclination acts on the bent spine. It is, in other words, working to obtain an opposite effect to that which we desire to destroy, and by the intervention of the same forces, the same immediate causes, but acting in a contrary direction, and with the view of effecting a cure, that this apparatus acts beneficially on the vertebral column." (p. 20.)

Dr. Tavernier, we are inclined to suspect, rather overrates the general applicability and efficiency of this instrument. He has a chapter on "the cases which contra-indicate the use of the belt, and those which require it." (pp. 10-11.) He seems to limit its inefficiency to cases in which the bones have become preternaturally hardened, or consolidated by bony anchylosis; in other cases, he says, "where applied to timely, art will triumph, and seem sometimes even to surpass the limits of the 'possible.'" (p. 11.) We should mention that he cites a considerable number of cases of cure, in addition to the three already so fully discussed: but we are sorry he puts those three on an equal footing of authenticity with the additional ones; he thereby throws some doubt over the whole series. This we regret, as it is calculated to excite a prejudice against an instrument which we think well worthy of being fully tried in those cases hitherto so very unmanageable in a lamentably large proportion. Some of the cases, too, are in themselves likely to excite a little incredulity. Thus we have the figure of the back of a young lady, aged fifteen, who suffered from a very considerable curvature for three or four years, the cure is represented to have been effected in *six weeks.* In most of the cases, however, from five to seven months were occupied in the treatment. In order to render the cure permanent, Dr. Tavernier recommends the use of "a simple ordinary corset, to which are differently adapted, according to the case, large ribands, which have for their object to bend the spine in the direction of the busk employed for the treatment."

From what we have said it appears that our judgment, quantum valeat, is decidedly favorable as to the value of the "lever-belt;" if we have spoken of its inventor as a quack, it is because he resorted to the practices of a quack; and if we have extended some share of blame to Dr. Tavernier, it is because we think he has exhibited some want of candour in particulars already pointed out and to which we do not wish to return. But this does not prevent us from candidly stating our favorable opinion respecting the apparatus, which we may just state, without any wish of thereby derogating from the merit of its inventor, bears a considerable resemblance to one already proposed by Delpech.

A few words respecting Dr. Brewer's part in the present publication. We rather suspect that he is not aware of the facts we have laid before our readers. He commiserates Dr. Tavernier as a most injured man, and tells him that "There is a thing, Harry, which thou hast often heard of, and it is known to many in our land by the name of pitch; this pitch, as ancient writers do report, doth defile, so our good brother had better let it alone." Now M. J. Guerin is, we suppose, Dr. Brewer's pitch, and so far as we know, it is Hossard and not Dr. Tavernier who hath been defiled by this pitch.

Although we cannot say much in favour of Dr. Brewer's "Analysis of Orthopædia," (which just comprises some four pages in very large type,) we are obliged to him for aiding in making known the "lever-belt" in this country, and shall be happy to meet him again, in print, if the subject-matter of his lucubrations be equally interesting; but we must be pardoned for suggesting to Dr. Brewer a more idiomatic study of his vernacular English, if he proposes to persevere in the functions of a translator. We suspect that our neighbours across the channel will deem Dr. Brewer's "Dédicace" not very classical French; and his own countrymen will not fail to accuse him of a tendency to Gallicise his mother-tongue. *Redress* is not the English for *redresser; grievous* is bad Saxon for *grave;* and "physiological travail" (p. 10) smacks of what Dr. Brewer perhaps would call *outre-manche* English. In the following extracts from Dr. Brewer's translation, the words are no doubt English, but the construction is we know not whence, and the meaning is we know not what; we give the punctuation exactly as we find it:

"Another set, demanding more, with less art, outdare, with more or less hardihood, the thousand ills of beds of extension, so dangerous when the object is really to extend the spine, so useless and unworthy the name, when, after the example of the orthopedists of our day, they are resorted to periodically and for mere popular consideration, their utility is bounded by the object of simply keeping the individual reclining on her back. (p. 7.) .... In this case, reputed incurable, the changes very remarkable to be observed here, were great enough for the traces of the deformity, before so apparent and shocking, to be easily concealed." (p. 32.) .... We call to witness this, not only the authority of experience, but even the luminaries of the medical science, who would boldly tell us their objections; as also the good sense of parents, to whose sight and handling we would not fear to present this apparatus, the simplicity of which admits the most inexperienced to render an account of its action. (p. 33.) .... It is easy to see that in these cases the supposed pressure exerted on the chest and the abdomen produced no great ravages, and that it would be difficult indeed to ally the smallest doubt of injury to the principal functions with such results,—with positive proof of the most perfect nutrition," &c. (p. 35.)

Art. VI.

1. *The Bengal Dispensatory and Companion to the Pharmacopœia, chiefly compiled from the works of Roxburgh, Wallich, Ainslie, Wight and Arnot, Royle, Pereira, Lindley, Richard, Feé, and including the results of numerous special experiments.* By W. B. O'Shaughnessy, m.d., Assistant Surgeon to the Bengal Army, &c. &c., Professor of Chemistry and Materia Medica in the Medical College of Calcutta. *Published by order of the Bengal Government.—Calcutta,* 1842. 8vo, pp. 794.
2. *Elements of Materia Medica and Therapeutics, including the recent discoveries and analysis of Medicines.* By Anthony Todd Thomson, m.d. f.l.s. &c. Third Edition, enlarged and improved.—*London,* 1843. 8vo, pp. 1232.
3. *The Elements of Materia Medica and Therapeutics.* By Jonathan Pereira, m.d. f.r.s. & l.s. &c. Second Edition.—*London,* 1842. Two Vols. 8vo, pp. 1926.

We have comprised these three works under one head, because they all relate to one general subject. We shall, however, review each one

separately, and content ourselves with a very brief notice of the last two on our list, as they are only new editions of works already noticed by us.

I. DR. O'SHAUGHNESSY'S BENGAL DISPENSATORY.

Several years ago the Governor-general of India (Lord Auckland), in council, appointed a Committee to inquire into various subjects connected with the state of the materia medica in British India, and on the expediency of compiling a Pharmacopœia for Bengal and Upper India. The committee consisted of six persons, viz. Drs. W. Jackson, J. Ranken, Pearson, and W. B. O'Shaughnessy, and Messrs. Bramley and James Prinsep. But long before these gentlemen could accomplish the important labours they each most zealously undertook, they were scattered by death, ill-health, and the casualties of service; and to Dr. O'Shaughnessy was intrusted the task of reporting on the subject of the proposed pharmacopœia. This gentleman at that time held the situation of Professor of Chemistry and Materia Medica in the Medical College at Calcutta, and was in every respect ably qualified to fulfil the task imposed on him.

In due time the report made its appearance. It consisted of some observations on the nature and uses of the pharmacopœias in general; a review of the leading arguments for the preparation of a pharmacopœia for Bengal; and notice of some of the circumstances requisite to render it of the widest and readiest utility; and lastly, a sketch of the character and extent of investigation which must indispensably precede the compilation.

In alluding to the causes which have led to the compilation of so many pharmacopœias, the report glances at the remarkable varieties of raw material, from which in different countries one and the same medicinal agent has to be prepared.

"Take potash and its compounds for example—obtained in Canada by the burning of the forest timber; in Ireland from the fern; in France, Italy, and along the Rhenish wine districts, from cream of Tartar; in India, with most economy, from nitre; in each case a totally different process is required for its extraction, purification, and adaptation to medicinal use. Soda gives us another and a striking instance.—The Spaniard obtains it most easily from barilla by mere washing; the Scot from kelp by a much more laborious and unproductive process; in France, and latterly in England, it is prepared from common salt (muriate of soda), which is first decomposed by sulphuric acid, and then the resulting sulphate of soda subjected to another process, which changes it into carbonate. In Bengal and Mysore, again, the soil, in many places, is so impregnated with the alkali, that incineration and washing extract from 35 to 50 per cent. of fine soda." (Introduction, p. vii.)

In considering the necessity and probable degree of utility of compiling a Pharmacopœia for Bengal, the report alludes to the high prices of all medicinal preparations sold by the private European establishments in Calcutta, Cawnpore, and Meerut, the only localities in Bengal or Upper India where European apothecaries have settled.

"Little competition existing, the prices of the most indispensable articles of medicine are fixed at such a rate that rich natives will not, the humbler cannot, avail themselves of the remedies which medical science has pointed out. . . . . We are prepared to show, that in the houses of well, nay highly, educated natives, medicines of every kind may be retailed in Calcutta, and throughout India, at

lower prices than even those of the London general practitioners, who charge at least 200 per cent. less than their Calcutta brethren, and give the patient the benefit of their attendance besides." (Ib. p. ix.)

These extracts show the highly interesting nature of the report, the suggestions of which were ordered by the India government to be carried into effect; and Dr. O'Shaughnessy was directed to devote himself to the task of compiling not only a pharmacopœia but a dispensatory of general materia medica. It now becomes our duty to examine the latter work, the title of which stands at the head of this article.

The work in question treats of a great variety of subjects, too many in fact to be properly investigated in a book restricted to one volume of 800 not very closely-printed pages. It embraces instructions on pharmaceutical manipulations; on the mode of taking specific gravities; of making ordinary meteorological observations, (explanatory details being given under each head;) an outline of chemistry for the guidance of the teachers of native apothecaries; a grammar of botany, constituting a systematised glossary of the terms used in botanical descriptions; a brief account of the mode of action of the several therapeutical classes of remedial agents; the vegetable materia medica arranged in the natural system; the animal materia medica; miscellaneous inorganic substances; and, lastly, an appendix on the manufactory of an improved pottery in Bengal; on the investigation and treatment of poisoning; with meteorological tables and notices of miscellaneous subjects.

We greatly regret that Dr. O'Shaughnessy did not think it expedient to devote the whole of his restricted volume to the proper and legitimate objects of a dispensatory; instead of introducing into it so great a variety of topics, many of which are quite foreign to a work on pharmacology. What connexion, for example, has the manufacture of an improved pottery with a dispensatory? Moreover the attempt to give epitomes of meteorology, of pharmacy, of chemistry, and of botany in a work of this kind, must, as far as practical utility is concerned, prove a failure; the details on each subject being too meager and imperfect to be useful; while their introduction is positively objectionable on the ground that they occupy that space which ought to be devoted to other more appropriate objects of inquiry. Accordingly, Drs. Wood and Bache in the United States Dispensatory, and Dr. Christison in the Edinburgh Dispensatory, have thought it best not to attempt any epitome of pharmacy which had been given by the compilers of previous dispensatories; and we wish that Dr. O'Shaughnessy had adopted the same plan.

We proceed now to a more pleasing duty, that of laying before our readers some of the valuable information, relating to the Indian materia medica, which is contained in Dr. O'Shaughnessy's work; and in doing this we shall, for the most part, select those substances which from being used in Europe, are most likely to interest our readers.

Opium. Dr. O'Shaughnessy gives the following information respecting the mode of preparing Indian opium.

"Towards the end of March when the plant is nearly ripe the bleeding of the capsules takes place. This operation is performed in the evening, and consists of tracing four parallel lines with a sharp knife or shell, penetrating the epicarp and sarcocarp of the capsule; a milky juice immediately exudes, and its flow is promoted by the deposition of dew during the night; each set of incisions yields on

an average one grain of opium. This is removed the ensuing morning, and the incisions repeated as long as any milk flows. The whole of the opium collected daily, and which is necessarily mingled with variable proportions of dew, is mixed together in earthern or wooden vessels. Should the quantity of dew have been excessive, a partial solution of the opium occurs, and an exudation of drops of a black, shining liquid (Pasewa) occurs on the surface of the opium. This pasewa contains many of the active principles of the drug, and should be mixed with it again to secure the uniform power of the mass. The proportion of pasewa is often much increased by the fraudulent admixture of water by the growers, in hope of having their opium purchased by the gross weight.

"When brought to the factories the opium is usually of the consistence of 64 to 68, that is, containing that proportion of solid matter per 100. It is then placed in large tanks of brickwork, or in wooden vats, with the upper surface exposed to the air. From these reservoirs portions are daily removed and exposed on wooden trays three inches deep by four feet long and two broad; evaporation takes place under such circumstances with tolerable rapidity, and the opium eventually reaches the consistence of 70—the standard of the factory for the Chinese investment.

"The degree of inspissation is estimated on the purchase of the drug, as well as during its subsequent evaporation, by drying average portions of 100 grains each, on a metallic table heated to 208° by steam.

"A sufficient quantity of opium, of average quality, is permitted to attain the consistence of 80°, and in that state is issued under the name of "Abkaree" to the licensed dealers of the bazars. Lastly, the finest opium which can be procured is set apart for medical purposes, and allowed to lose by evaporation nearly the entire of its water.

"The *Chinese investment opium* is of a rich chesnut colour, translucent at the edges of thin masses, tears into portions with ragged margins; its stucture is granular, its taste hot and bitter, its smell rich, peculiar, and agreeable; if a small portion be rubbed between the finger and thumb for a few seconds it draws out into long colourless threads, and from the number, fineness, and tenacity of these threads the Chinese form their first estimate of the value of the drug.

The "*abkaree*," or Bengal bazar opium is more solid than the Chinese investment drug, of a darker colour, and often of a heavy ascescent smell.

For the Chinese investment the opium is made into cakes, each containing four pounds, and enveloped in a case made of the petals of the poppy, and agglutinated with a paste made of opium and water, (Lewa.) The abakaree opium is also vended in balls weighing two pounds, covered by strips of a coarse kind of silk cemented by a mucilage of gum. The medicinal opium is solid, brittle in the cold season, of a brown colour, and fine smell. The medicinal drug is packed with great care in four pound and two pound squares, covered with layers of talc, and further defended by a case of brown wax, half an inch in thickness." (pp. 172-4.)

The quantity of pure morphia present in opium varies according to the kind:

"Bengal investment opium contains	2½ per 100.*
Medicinal opium	4
Malwah ditto	6
Garden opium, Patna	10½
Turkey opium	9
Smyrna opium	10½" (p. 176.)

Opium eating. It is a common but an erroneous notion entertained in this country, that the habitual use of opium, whether by chewing or

* "The official returns by the opium examiner of Calcutta for the last four years show the following per centage of *morphia* in a 'tolerably pure' state:

3·7, 4·1, 3·6	1836.	3·8, 4·1, 4·1	1837.	4·0, 3·9, 4·0	1838.	3·9, 4·0, 3·7	1839."

smoking, has a direct tendency to shorten life. That its excessive employment is attended with this effect must be admitted; but that its moderate use has any such tendency we deny. Several years ago Dr. Christison gave abstracts of eleven cases of opium-eating from which it appeared that the popular notion of the practice being injurious to health and shortening life was erroneous; and Dr. O'Shaughnessy in speaking of opium-eating, observes that it has not been "ascertained that this habit has a direct tendency to shorten life; on the contrary, the longevity of opium-eaters is in many parts of the East of proverbial notoriety."

Opium-smoking. On this subject Dr. O'Shaughnessy observes:

"The vice of opium smoking is now so prevalent in Calcutta, not only among the Chinese, but the degraded and depraved of every sect and nation, that we have had many opportunities of witnessing the results to which it leads.

"Stupor, reverie, and voluptuous listlessness are the immediate effects produced. In this state the individual can be at once and easily aroused to exertion or business. No sickness, constipation, or any other functional disturbance supervenes on each indulgence; gradually, however, the appetite diminishes, the bowels become irregular, emaciation takes place, sexual tendencies are destroyed, and premature old age speedily comes on. This we admit is an extreme case; when the habit is but moderately followed it appears to occasion no greater evil than the proportionate indulgence in wine or other spirituous liquors." (pp. 180-1.)

Anarcotina, (*Narcotina.*) Narcotina, which Dr. O'Shaughnessy calls anarcotina, is devoid of all narcotic properties and is capable of arresting the paroxysms of intermittent and remittent fevers.*

Gurjun balsam. As gurjun balsam may be employed as a cheap substitute for balsam of copaiba, and as a very large quantity of it was, a few years back, brought to this country and is we believe yet unsold, it may not be uninteresting to extract a part of Dr. O'Shaughnessy's notice of it.

It is the produce of the *dipterocarpus lævis*, a large tree growing in Chittagong, Pegu, Assam, Tippera, &c.

"To procure the balsam, Dr. Roxburgh informs us a large notch is cut in the trunk of the tree near the earth, where a fire is kept up till the wound is charred, soon after which the liquid begins to ooze out; a small gutter is cut in the wood to conduct the liquid to a receiver. The average produce is said to be 40 gallons in each season, during which it is necessary to cut of the charred surface from time to time, and burn it afresh. The process is performed during the cold season.

"The gurjun balsam varies in consistence from that of thick honey to a light oily liquid. The colour of a fine specimen of thick gurjun obtained from Captain Jenkins of Assam, was a pale grey; specimens sent from Rangoon by Mr. Spier were a light brown. As found in the bazar this substance generally occurs as a brown, oily looking, semitransparent liquid, in odour strongly resembling a mixture of balsam of copaiba with a small portion of naphtha.....

"The close resemblance in the physical and chemical properties of this gurjun and copaiba balsam led to the institution of an extensive set of experiments by the Editor on the medicinal effects of the former in the treatment of gonorrhœa. The results, which have been laid before the profession, and which have been confirmed by trials made by other practitioners, seem perfectly conclusive that in the treatment of gonorrhœa, gleet, and similar affections of the urinary organs the essential oil of gurjun is nearly equal in efficacy to the South American drug.

"The essential oil may be given in ten to thirty drop doses in mucilage, milk, rice-water, or thin gruel, and repeated thrice, or still more frequently daily. It

* See an Analysis of Dr. O'Shaughnessy's paper on Narcotine as a substitute for Quinine in Intermittent Fever in the Brit. and For. Med. Rev., vol. VIII. p. 263.

generally causes a sensation of warmth at the epigastrium, eructations, and sometimes slight purging. It communicates a strong smell of turpentine to the urine, which it increases remarkably in quantity. Some obstinate cases of chronic gonorrhœa and gleet, which had long resisted copaiba and cubebs, have been cured by this remedy in the course of the experiments alluded to." (pp. 223-4.)

Indian hemp. The plant which yields this is the *Cannabis Indica* of botanists, and is probably a variety only of the common hemp, or *Cannabis sativa.* A very full account of the effects and uses of this plant is contained in the Dispensatory; but as we have already given (see Brit. and For. Rev., vol. X. p. 225) a very full analysis of Dr. O'Shaughnessy's pamphlet on this subject, it will be unnecessary to enter further into it, except to observe that subsequent trials with this plant in this country have confirmed neither its asserted activity nor its therapeutical efficacy. Whether this be owing to the preparations having undergone some deterioration in their passage, or to the comparative phlegmatic temperament of the English, we know not. We understand that in a recent case of tetanus in the London Hospital, the extract prepared by Mr. Squire, of Oxford Street, from plants brought from India by Dr. O'Shaughnessy, was given in very large doses but without any apparent alleviation of the symptoms; and in a few days the man died. The case, which was originally under Mr. Luke's care, was transferred by him to Dr. Pereira; and the exhibition and effects of the medicine were watched by Dr. O'Shaughnessy. In this case the hemp acted as a soporific, and produced a slight tendency to inebriation. There was also an approach to the cataleptic condition, for when the arm was very gently and slowly raised it sometimes remained, when left to itself, for a few minutes in the same posture. During its use the pupils were contracted; but in most cases dilatation follows the use of hemp. The medicine had also a complete trial, in a case of idiopathic tetanus, at Guy's Hospital, by Dr. Babington. It was used both externally and internally, but without any manifest effect on the system or the disease.

Grass oil of Nemaur (Roosa-ke-tel, *Hind.*) As this oil is met with in the London shops under the name of *Oil of spikenard*, or *Oil of Nard*, while its origin is but little known, we subjoin Dr. O'Shaughnessy's account of it; premising that the grass which yields it has been denominated by Professor Royle the *Andropogon calamus aromaticus*, and is probably identical with the "*Sweet calamus*," and the "*Sweet cane from a far country*" of Scripture,—the *κάλαμος ἀρωματικὸς* of the Greeks.

"This valuable oil was first brought to the notice of the profession by Dr. Maxwell in 1824, and was further described by Dr. Forsyth in 1826.

"The oil is obtained from the grass by distillation; 250 to 300 small bundles of the grass are placed in a boiler, covered with water, and distilled. About a seer of oil is obtained in the receiver. Dr. Forsyth describes it as volatile, extremely pungent, of light straw colour, very transparent, with a peculiar rich and agreeable odour.

"Dr. Forsyth adds, that it is very highly esteemed by the wealthy natives for the cure of rheumatism, especially that of the chronic kind; two drachms of the diluted oil are rubbed over the pained part in the heat of the sun or before a fire twice daily. It causes a strong sensation of heat or pricking, lasting for two hours or longer. The natives also regard it as an efficacious remedy in slight colds. They anoint the soles of the feet with the oil, and it is stated that slight diaphoresis is thus produced.—See Trans. Med. and Phys. Soc. iii. p. 216." (pp. 639-40.)

Nux-vomica bark (*Kuchila.*) It is now well known that the bark

called by European druggists "*false angustura*," is the bark of the *Strychnos nux vomica*. Though its nature was long suspected, we are indebted to Dr. O'Shaughnessy for first clearly determining it. It appears also from his statement that the same bark is commonly sold in Calcutta under the name of "*Rohun*," and substituted for the harmless bark of *Soymida febrifuga* (Swietenia febrifuga.)

"In vol. iv. Trans. Med. Phys. Society of Calcutta there is a paper by Mr. Piddington on the rohuna bark, and the sulphate of its bitter principle. Mr. Piddington stated that he had applied to the bark the process described by Paris (Pharmacologia, vol. ii.) for preparing sulphate of quinine, and with perfect success. He presented to the society a small specimen of the alleged sulphate of rohuna, and described in full detail the process of its preparation. In 1836, however, it was ascertained by the editor of this work that the salt prepared by Mr. Piddington was sulphate of *Brucine*, and that the bark he operated on was that of the nux-vomica tree. On inquiry in the bazars it was found that in several shops the nux-vomica bark was sold for the rohun." (p. 248.)

We have been informed that a large quantity of the supposed "sulphate of rohuna" (in reality sulphate of brucine or strychnine,) had been prepared at Calcutta for the use of the military hospitals of India, as a substitute for the sulphate of quinine; and that some of it had actually been sent off for use. Fortunately, however, Dr. O'Shaughnessy discovered its true nature in sufficient time to prevent the occurrence of any accident.

Aloes. We have been disappointed in not finding in the Dispensatory some notice of the origin of the aloes exported from Bombay, under the name of *socotorine and true hepatic aloes.*

"We are indebted to Mr. Heddle of Bombay, for three specimens of aloes, marked Socotorine, Kurachee, and Deckan.

"That marked *socotorine* is rich, brown, shining, soft, ductile, and adhesive at 82° Fahr.; of strong and not disagreeable odour, wrapped up in and including leaves. It much resembles the best Bengal opium in general appearance. Water dissolves 82 parts per 100.

"The *Kurachee aloes* is nearly black, opaque, rather vascular, structure of shelly fracture, faint smell, powder blackish, slightly adhesive to the touch. It is identical with the better kinds of Musabhir of the Bengal bazars. Water dissolves only 52 per 100.

"The *Deckan* specimen is deep brown, of earthy and slightly resinous fracture, and faint odour, powder brown. Soluble in water in proportion of 98 per 100.

"The common *bazar aloes* in Bengal is black, brittle, of semi-vitreous fracture, and too impure for medical use." (p. 666.)

Kino. Dr. O'Shaughnessy gives us no information as to the source of the extract called kino, imported into this country from India, and which is the produce of some part of Lower India. The concrete juice of *butea frondosa*, which he calls in one place the *Bengal kino*, and in another *Indian kino*, is a very different substance to the extract known here as kino; and we regret, therefore, that he should have given it these names, since they are calculated to lead to considerable confusion.

Catechu. We expected to have found in the Dispensatory a full account of the various astringent extracts brought from India, and known in European commerce as *terra japonica*, *catch*, and *catechu.* But while Guibourt describes eleven kinds, and others a still greater number of varieties, Dr. O'Shaughnessy names three only, which he calls *Bengal catechu*, *Bombay catechu*, and *Massive catechu.* It is greatly

to be regretted that on this, as well as on other occasions, he should have relied on an European botanist for his pharmacographical details. We allude now to Fée, who, whatever may be his other merits, is, as a pharmacologist, inferior to Guibourt, Martius, and others. And we strongly protest against the practice of writers on Indian Materia Medica, taking their descriptions from European pharmacologists, as it leads to inextricable confusion.

With these remarks we close our notice of the Bengal Dispensatory. From the extracts we have made, our readers will be enabled to form a good idea of the nature and valuable quality of the information to be found in it. The work will be found invaluable to all European surgeons and physicians who are practising or intend to practise in India. To the educated native practitioner it must prove of the highest service. While, lastly, to the scientific European pharmacologist it is an indispensable work.

II. Dr. Thomson's Elements of Materia Medica.

In the advertisement to the present edition, Dr. Thomson states that it "is not intended to afford minute details of the natural history and the commerce of the medicinal agents it treats of; but to supply that general information respecting the chemistry and therapeutical employment of the articles of the materia medica, which is likely to prove practically useful to students and junior practitioners of the healing art." We have strong objections to this plan, in a work chiefly appropriated to therapeutics, for the obvious reason, that the chemical and other details respecting the properties of the various substances cannot so conveniently be given as under a natural-historical or alphabetical arrangement. On this account the form of dispensatory, previously adopted by the author, was much better adapted for the purpose. Again, a work on therapeutics should be restricted to that subject and should not be overlaid with the accumulated rubbish of inert and doubtful medicines, which, however, may be very properly described elsewhere as articles of natural history, applicable to some useful purpose. Such a work ought to concentrate its force upon the really active and efficient remedies.

The classification of medicines adopted by Dr. Thomson is chiefly based on that of Dr. Murray; but we went so fully into the subject, in the analysis of Dr. Paris's work in our last Number, that our limits will not permit us again to enter upon it. We may remark, however, that emetics are placed under the division of agents that act on the muscular and sanguineous systems, and cathartics under those which excite the secerning and exhalent systems. This violent separation of substances so clearly analogous if not identical in their action is absurd: all purgatives act more or less on the muscular tissue of the intestines, and thus increase the peristaltic motion of the intestines. The number of substances in the list of excitants (the first order) is enormous, and it contains agents of very doubtful pretensions, and which are allied to one another by few family resemblances. Among these may be enumerated, nux vomica, strychnia, brucia, veratria, camphor, the preparations of gold and mercury, electricity, chlorine, iodine and bromine with their combinations; besides a number of vegetable substances, not admitted into the British pharmacopœias. We admire the author's industry and re-

search, but we question very much the propriety of uniting into one order, such a mass of heterogeneous materials. The common-place student would be quite bewildered by such a formidable array of forces; and many who, in their study of geometry, passed with ease the *pons asinorum* would here be beset with insuperable difficulties. We shall, however, let the author speak for himself.

"In prescribing any of these material agents, or in taking advantage of the mental affections in this collection of excitants, the general intention of the physician is to sustain the powers of life under diseased conditions of the frame; to rouse action when it is defective; and, finally, to imitate nature in setting up *temporary movements* in the system, resembling disease, or what has been termed *reaction*, in order to restore the balance of the circulation, and that equilibrium of all the functions which constitute health." (p. 234.)

The field indicated in this account of their action is by no means very definite, and might without much stretch of imagination, have included nearly every article of the materia medica; for all are prescribed with the direct or indirect object of restoring the circulation and the other functions of the body to their healthy standard.

Camphor. The following account of the various virtues of this drug affords a good specimen of the style of the work, and shows the loose and unguarded manner in which the author is apt to indulge in giving an account of the qualities of remedies:

"There can be no doubt that camphor, which forms a group by itself, although it has so close an affinity to the volatile oils, is properly placed in this class of medicinal agents. Its excitant properties are confirmed by its influence in stimulating the uterus to renewed action when its efforts have been suspended, during parturition, by the influence of opium, or by hemorrhages, or any other cause; a purpose for which it has long been successfully employed by Dr. Hamilton, the Professor of Midwifery in the University of Edinburgh. . . . . . The diversity of the action of camphor depends on the dose in which it is administered. A modification of its action is produced by combining it with some other medicines; as for example, with antimonials, nitre, and neutral purgative salts. In combination with opium and aromatics, its influence in checking the progress of gangrene and in upholding the powers of the constitution in confluent smallpox, measles, and other eruptive fevers, when these take on the typhoid character, and the eruptions recede, has been well confirmed; and in these cases its effects can only be explained by admitting its excitant properties. . . . . In summing up the properties of camphor, it may be justly stated that it partakes of the properties of many of the other classes of medicines; that it proves excitant, or sedative, or narcotic, or antispasmodic, or diuretic, according to the combinations in which it is administered and the condition of the patient at the moment of administration. In full doses, namely, from gr. x to gr. xx, given alone, in low and depressed states of the vital energy, its excitant power is displayed; in combination with antimonials and mercurials, it lowers action and operates as a sedative: with opium its narcotic powers are developed and those of the opium augmented; with salts of quina it is either excitant or antispasmodic; and with nitrate of potassa the influence of salt upon the kidneys is both ensured and increased. Its powers, in combination, of diminishing the drastic and griping powers of resinous cathartics, at the same time that it aids their purgative property, was formerly mentioned." (pp. 239-42.)

If these statements be correct camphor certainly possesses very valuable properties; but we had no conception that any modern author held such a high opinion of its therapeutic virtues. First, with regard to its power in stimulating the inactive uterus during parturition, we may remark that it may, like alcohol and other stimulating substances, oc-

casionally prove useful, but certainly its merits in such cases are in general little esteemed by obstetrical practitioners. Does the author really believe that camphor, along with opium and aromatics, will check the progress of gangrene? We thought that the more accurate observation of the nineteenth century had almost entirely dissipated this misconception. This agent may assist in supporting the strength, during the progress of gangrene and the various eruptive fevers accompanied with a typhoid type, but, in this property, it is greatly inferior to wine or diluted alcohol, and is, moreover, attended with more risk when exhibited in large doses. We should like to know how ten to twenty grains of camphor united with any antimonial or mercurial would lower action and operate as a sedative? Is its exciting powers completely extinguished by combination? for Dr. Thomson elsewhere states (p. 296) that "sedatives are substances which directly depress the energy of the nervous system, diminishing action in animal bodies without inducing previous excitement." How do the salts of quina render it excitant or antispasmodic, when it possesses inherently these very properties?

As the treatment of fever by alcoholic fluids is a subject upon which various opinions are held, we have pleasure in quoting the following judicious remarks:

"It is, however, evident that wine and alcoholic fluids are seldom or never necessary in the early stages of fever; even in the later stages the propriety of their administration must altogether depend on circumstances and the nature of the prevailing epidemic. The nearer that the symptoms approach those of typhus, the more they are likely to prove beneficial. It is in the stage of collapse, or failure of power, denoted by a rapid, soft, intermittent pulse, the tongue tremulous when protruded, low muttering delirium, tremors, subsultus tendinum, petechial eruptions, and the patient sliding to the foot of the bed, that they are especially required. Nor is the property of the administration lessened by the tongue being dry and streaked with brown or black, and the teeth incrusted with sordes, provided attention be at the same time paid to the local affections, which these symptoms generally indicate, and measures be taken for their relief. Even when the collapse is not clearly indicated; when during the apparent favorable progress of the disease the pulse suddenly becomes soft and compressible, the skin cool and clammy, with a feeling of exhaustion, especially if there be a desire for wine, the necessity of employing some of the articles of this group of excitants is indicated. As far as regards quantity, from six to eight ounces of wine or a pint of weak punch, may be allowed in twenty-four hours at proper intervals." (p. 250.)

The following is part of Dr. Thomson's interesting account of the influence of mental excitants on the cure of diseases.

"With regard to the influence of mental excitants there can be no doubt, although their employment as therapeutical agents has been most unaccountably neglected. No man can practice his profession advantageously, however, who does not make himself acquainted with the anatomy of mind as well as that of the body: it is only such that can form an accurate conception of the influence of mental affections on the bodily frame, and how far not only moral happiness but corporeal health and vigour depend on the due application of mental energies. (p. 290)........Under many circumstances, joy has operated as a therapeutical agent. Alexander Trallianus has recorded a case of melancholia entirely cured by joy; and Corineous mentions an instance of a tertian being subdued by the same means. There are many instances of its curative influence in the writings of Hildanus and Etmuller. The conditions of the habit in which joy is most likely to display its salutary exciting influence are those of decided diminished action, such as occur in melancholia, hypochondriasis, and chlorosis. It may be

demanded, how the highly pleasurable emotions are to be employed as remedial agents? Now, in reply, let me suppose that a medical practitioner is consulted for the relief of a dyspeptic affection, attended with hypochondriasis, which he can trace to moral affliction and disturbance in the nervous system. He finds that the feelings of his patient are quick, sensitive, and powerful, and perpetually harassed by the objects which surround him. His first step should be to remove him from these; and every means, the most powerful which can excite new impressions on the mind, should be adopted to overcome those which have caused and are keeping up the disease. The lively sports of the field, if the patient has any predilection for them; scenes of gaiety and animation; news of an agreeable kind; exhilarating conversation; and every exciting feeling of a pleasant description, must be courted and cherished." (p. 292.)

There is also sound observation in the following remarks; but we are not quite sure about the good dinner and the wine, which, although a temporary cure, might afterwards aggravate the disease.

"A very frequent disease connected with mental depression is nervous cephalalgia. The pains are generally acute over one, sometimes over both eyes. The brain in such a state is in an irritable, not in an inflammatory condition; hence stimulants are indicated, and we find that sometimes even those of a material kind, a good dinner and a glass of wine, will dissipate that which to a careless observer would have demanded cupping and other depleting measures. A much more immediate remedy, however, is any event which can cause pleasurable and cheerful feelings in the mind; under such the irritability and the headach will generally and instantly disappear."

In describing the effects of impetuosity, the author mentions the case of a gentleman in the last stage of phthisis, who died in consequence of excitement from this passion. (p. 294.) The same story is related at page 233, under the general article, "mental influences."

Our limits will not allow us to dwell longer on Dr. Thomson's work. In delivering our opinion as to its general character and merits, we may state that it is one of considerable merit, indicating very extensive research on the part of the author, and containing a large amount of valuable facts and information; but it contains much useless matter, and the style is often diffuse and the doctrines not unfrequently hypothetical or destitute of precision.

III. Dr. Pereira's Materia Medica.

In his preface to this new edition of his work Dr. Pereira tells us, that "among the additions will be found articles on mental impressions, light, heat, cold, electricity, magnetism, diet, climate, and exercise, considered as therapeutic agents." Under the article "light" the author makes the following remarks on dioptric instruments, which we shall quote for the benefit of our weak-sighted readers:

"When vision is imperfect, from defect of focal distance, the remedy consists in the use of dioptric or refracting instruments (eye-glasses, spectacles.) In myopia (i. e. short or near-sightedness,) doubly-concave lenses (whose focal lengths vary from $2\frac{1}{2}$ to 48 inches,) are usually employed to counteract the over-refractive power of the humours; while in presbyopia (long or far-sightedness) doubly-convex lenses (whose focal lengths vary from about 6 to 48 inches) are generally used to obviate the diminished refractive power of the humours of the eye. Lenses for the above purposes are commonly made either of flint-glass or of Brazilian quartz. The latter, called pebble, has the advantage of greater hardness, and its surface therefore is not so readily scratched........

"*Chromatic instruments.* In some affections of the eye (popularly known as weakness of sight) coloured glasses are employed, with occasional relief, to diminish the intensity of the light. Those with a neutral tint (or twilight tinge) prove the most agreeable to the eye........Green, blue, indigo, and violet lights, are much less injurious than either red or yellow. Spectacles of these colours have been made for the use of those suffering with sensitive eyes, but they are inferior to the neutral tint before mentioned; since, after their removal from the eyes, every object sometimes presents for a short period complimentary tints, showing that these colours have fatigued the retina. All dark-coloured glasses, however, and especially black crape spectacles, are objectionable, on account of their greater power of absorbing and radiating caloric, by which they prove heating to the eyes." (pp 6-7, vol. i.)

The following are very judicious and useful remarks regarding the vapour-bath :

"As aqueous vapour, like air, is a worse conductor of caloric than liquid water, its influence, as a source of either heat or cold, is neither so powerfully nor so speedily felt as that of the latter. Hence, therefore, the temperature of the vapour-bath should always exceed that of the water-bath. If, however, the whole body be immersed in vapour, which is consequently inhaled, the temperature must be a little less than if the trunk and limbs alone were subjected to the influence of vapour; because the inhalation of vapour stops the cooling process of evaporation from the lungs. The following is a comparative view of the heating process of water and of vapour distinguishing the latter according as it is or is not breathed.

	WATER.	VAPOUR.	
		Not breathed.	Breathed.
Tepid-bath	85°— 92°	96°—106°	90°—100°
Warm-bath	92 — 98	106 —120	100 —110
Hot-bath	98 —106	120 —160	110 —130

The vapour-bath is distinguished from the hot-air bath by its soothing, relaxing and greater sudorific influence; from the hot-water bath, by its inferior power of communicating heat, by its greater sudorific tendency, and by its causing scarcely any superficial compression of the body, whereby it does not occasion the precordial oppression experienced on entering the water-bath." (p. 15, vol. i.)

A good outline is given of the various substances used as food ; but as the author has since published a distinct work on the subject, we shall have another opportunity of noticing his labours more at length. Besides the additions which have already been referred to, Dr. Pereira has improved the work by describing more in detail the process of the British pharmacopœias, "including those of the new Edinburgh one," and adding a large number of new woodcuts. On a former occasion, we expressed our high opinion of the first edition of this work. The present edition being enlarged and improved is necessarily still more worthy of approbation. As a record of materia medica, it is unrivalled in the combined qualities of fulness and accuracy by any other work in our language; although perhaps for the student and junior practitioner, one more condensed would be found better adapted for ordinary use. No one is so well qualified to write such a manual as Dr. Pereira.

ART. VII.

Recherches Expérimentales sur les Propriétés et les Fonctions du Système Nerveux dans les Animaux Vertébrés. Par P. FLOURENS, Membre de l'Académie Française, Secrétaire perpétuel de l'Académie Royale des Sciences, etc. etc. Seconde Edition, corrigée, augmentée, et entièrement refondue.—*Paris*, 1842. 8vo, pp. 516.

Experimental Researches on the Properties and Functions of the Nervous System in Vertebrated Animals. By P. FLOURENS, Member of the French Academy, Perpetual Secretary of the Royal Academy of Sciences, &c. &c. Second Edition, corrected, enlarged, and entirely recast.—*Paris*, 1842.

THE work of M. Flourens, of which this new edition has recently appeared, is well known to professed physiologists as one of the standards of their science. It appeared about the same time that the discoveries of Sir C. Bell were first made known to the world; and the reputation which it might otherwise have gained was, in this country at least, obscured by the more brilliant rays of our own luminary; but on the continent it has been more fully appreciated, and we believe that to it is mainly due the high position which M. Flourens has obtained in the scientific circles of Paris. In Britain his discoveries have been gradually incorporated with the "received text" of physiological science, without due attention to their source; and we think it but a deserved tribute to his merits to call the attention of our readers to the acknowledged merits of his former researches, as well as to notice those of more recent date and greater novelty.

The volume before us not only includes the whole of the work which appeared under the same title in 1824, together with the supplementary papers which were published in the following year, and others which have been since read before the academy, and which have been published in their transactions; but also the results of some additional researches of great interest which have been more recently made by M. Flourens. To these last we shall presently draw the more particular attention of our readers; but we shall commence by giving a general account of the results of his former inquiries, which the resumé drawn up by himself will enable us to do in his own words. It is necessary that we should explain that M. Flourens has slightly changed his phraseology since the first publication of these memoirs. The discoveries of Sir C. Bell have led him (as we anticipated, see vol. V. p. 537,) to separate more completely the properties of the *nerves* from those of the *spinal cord.* For the first he retains the term *excitability*, whilst to the latter he gives the appellation of *sensibility;* but he expressly states that by this term he means simply the power of "receiving and transmitting impressions," which is precisely Dr. M. Hall's "Reflex Function." Indeed we find a similar use of the word among the French writers in general. Thus, in one now before us,* we meet with the following passage: "All living organized beings are *sensible* or *irritable*, having the power of acting of themselves, and of reacting on the causes which tend to modify their existence. But all are not *sentient*, that is to say, they are not *conscious* of that

* Analyse Physiologique de l'Entendement Humain. Par J. C. Collineau.

which takes place around and within them." The term *perception*, to which we are accustomed to attach a higher notion, is the one by which French physiologists usually designate what *we* understand by *sensation*, or the *consciousness of an impression*. The following is M. Flourens's own general estimate of the tendency of his inquiries. It is to be remembered that they were *contemporaneous* with those of Sir C. Bell, and that in the following summary, therefore, these are only alluded to where they bear upon the author's own:

"It was early perceived that the nervous system is at the same time the organ by which the animal receives *sensations* (impressions), the organ by which it executes or determines its *movements*, the organ by which it *perceives* and *wills*. But does the faculty of *willing* and *perceiving* reside in the same parts with the property of *feeling?* Does the property of *feeling* (receiving impressions) reside in the same parts with the property of movement? Are thinking, feeling, moving, but one property, or are they three distinct properties, having separate organs for their manifestation? These important questions, debated during so many ages, still remained for solution.

"My experiments showed, in a most decisive manner, that there are three essentially distinct properties in the nervous system; that of *perceiving* and *willing*, that of *feeling* (receiving impressions), and that of *moving;* that these three properties have seats as distinct as their effects; and that their organs are separated from each other by definite limits.

"The nerves, the spinal cord, the medulla oblongata, and the tubercula quadrigemina or bigemina, are the only immediate *excitors* of muscular contraction; the cerebral lobes are restricted to the power of *willing* this movement, and do not immediately excite them; and further, that in the spinal cord and in the nerves, the parts which *excite* movements are not the same as those which are *sensible*, and those which are sensible have not the power of exciting movement. (Sir C. Bell.)

"There are, then, in the nervous system three properties essentially distinct; one, that of *perceiving* and *willing;* in other words, *intelligence;* another, that of *receiving* and *transmitting impressions*, or *sensibility;* the third, that of *immediately exciting muscular contraction*, which I propose to call *excitability*. Further, there resides in the cerebellum a property, of which there had been previously no idea in physiology; and which consists in the *co-ordination* of the movements *willed* by certain parts of the nervous system, and *excited* by others. The nerve directly excites the contraction of the muscle; the spinal cord unites the separate contractions into regular movements; and the cerebellum co-ordinates these movements into the regular movements of locomotion,—walking, running, flying, &c.; whilst by the cerebral lobes, the animal *perceives* (feels) and wills." (Preface, pp. x-xiii.)

By this general statement of the opinions of M. Flourens, and the explanation of his phraseology, our readers will be prepared to follow us through his chapter on the *Laws of nervous action;* which, being itself a condensed summary of the results of his experiments, we shall translate without abridgment:

"Three grand laws govern the action of the nervous system: the first is the *speciality* (distinctness) of action; the second is the *subordination of the nervous functions;* the third is the *unity of the nervous system*.

"*Speciality of nervous action*. I. It has been shown that every essentially distinct part of the nervous system has a function or mode of acting, which is equally distinct. The cerebrum does not act in the manner of the cerebellum, nor the cerebellum like the medulla oblongata, nor the medulla oblongata like the spinal cord and nerves. II. Each part of the nervous system has, then, a peculiar and special action; that is, a different action from the others; and we have further seen in what this speciality consists. In the cerebral lobes resides the faculty by which

the animal thinks, wills, recollects, judges, becomes conscious of sensations, and commands its movements. From the cerebellum is derived the faculty which co-ordinates the movements of locomotion; from the tubercula bigemina or quadrigemina, the primordial principle of the action of the optic nerve and retina; from the medulla oblongata, the motor or exciting principle of the respiratory movements; and, lastly, from the spinal cord itself, the faculty of blending or associating into combined movements the partial contractions immediately excited by the nerves in the muscles. III. The important fact of the speciality of action of the different parts of the nervous system—a fact, towards the demonstration of which the noblest efforts of physiologists were for a long time directed, is then henceforth established by direct observation, and the result demonstrated by experience.

Speciality of the nervous properties. I. There are three properties of the nervous system essentially distinct; 1, that of exciting muscular contraction, or *excitability;* 2, that of receiving and transmitting impressions, or *sensibility;* 3, that of perceiving and willing, or *intelligence.* II. And each of these properties has a distinct seat in a determinate organ. *Excitability* resides in the anterior column of the spinal cord, and in the nervous fibres which arise from it. *Sensibility* has its seat in the posterior columns of the spinal cord, and in the nerves that originate (or terminate) in it. *Intelligence* is restricted to the cerebral hemispheres.

Office of each part of the nervous system in locomotion. I. No movement is immediately derived from the will. The will is only the provoking (exciting) cause of certain movements; and is never the efficient cause of any. When an animal wills to move its arms, its leg, or any other part, it immediately moves it; but it is *not* the will which animates the muscles of the part moved, excites them to action, and combines their movements; and these functions are not performed by the cerebral hemispheres in which the will resides. II. The direct cause of muscular contractions resides particularly in the spinal cord and its nerves; the cause which co-ordinates the movement of different parts, resides exclusively in the cerebellum. III. Here then, are three distinct phenomena in a voluntary movement: 1, the willing of the movement, which resides in the cerebral lobes; 2, the co-ordination of the various parts that concur to produce this movement, which resides in the cerebral hemispheres; 3, the excitation of the muscular contractions, which has its seat in the spinal cord and its nerves. IV. Since these three principal phenomena are essentially distinct, and have their seat in three organs which are also essentially distinct, we see the possibility of abolishing one of them, the will for example, without destroying the others; or of abolishing at once the will and the co-ordination, without interfering with the contraction. V. Of this, the experiments contained in the present work afford complete evidence. An animal deprived of its cerebral lobes no longer moves spontaneously or voluntarily; but it moves with co-ordination, and quite as regularly as when these lobes are present. An animal deprived of its cerebellum, on the contrary, loses all power of co-ordinating its movements. Nevertheless all the parts of such an animal, its head, trunk, and extremities, perform movements of their own; but their motions are no longer co-ordinated, nor is a combined result obtained. Such an animal no longer walks, no longer flies, nor holds itself upright: not that it has lost the use of its legs and wings, but because the co-ordinating power no longer exists. In a word, all the partial movements still exist; but the co-ordination of these movements is lost. VI. What I have said of the cerebellum, in reference to the co-ordinate movements of locomotion, may be said of the medulla oblongata, with reference to the co-ordinate movements of conservation. Whilst this part of the medulla exists, they remain; when it is destroyed, they cease. [Under this head, M. Flourens points to the movements of respiration, deglutition, &c.] Hence their regulating principle, or *primum mobile*, has its residence there. VII. With respect to the spinal cord, its function is restricted to the union of muscular contractions, the first elements of all movements, into combined motions; and although all the nerves which determine these contractions and movements issue from it, it is nevertheless not in *it* that the wonderful faculty resides by which these contractions and movements are co-ordinated into determinate

actions, such as leaping, flying, walking, running, standing, &c., or inspiration, crying, yawning, &c. This faculty resides in the cerebellum as to the former of these movements, and in the medulla oblongata as to the latter. VIII. The movements of respiration, crying, yawning, &c. are commonly termed *involuntary*, in opposition to the movements of locomotion, which are termed *voluntary*. We have just seen in what sense the term *voluntary*, as applied to certain movements, must be understood. The will is never anything else than the exciting, external, occasional cause of these movements; still it can provoke them, regulate their energy, and determine their object. Thus an animal can, at will, either move or remain still, can walk quickly or slowly, and in whatever direction it please. It is then absolute master, not of the mechanism of its movement, but of the movement itself. It is the same as to running and leaping, which are only a more rapid kind of walking; as to flying, swimming, crawling, which are only different species of progression; as to standing, which is but a part of walking; and, in a word, as to all the movements of locomotion or translation. On the other hand, respiration, crying, yawning, certain dejections, &c. only depend upon the will up to a certain point, and in certain cases. In general, all these movements take place without its either perceiving them or participating in them; and often in complete opposition to it. Lastly, the movements of the heart and intestines are absolutely and totally removed from the influence of the will. With regard to the will, therefore, as with respect to the mechanism and to the organs of movement, there are three kinds of movement essentially distinct from each other. Those of the first class are entirely under its control; those of the second are so only in part; and those of the third are not so at all.

"*Subordination of the functions of the nervous system.* I. These functions are to a certain extent under subordination to each other. II. There are, in the nervous system, some parts which act spontaneously or of themselves; and there are others which act only in subordination to the rest, or when impelled by them. III. The subordinate parts are the spinal cord and nerves; the *regulating* and *primordial* portions are the medulla oblongata, the seat of the principle which determines the movements of respiration; the cerebellum, the seat of the principle which co-ordinates the movements of locomotion; and the cerebral lobes, the exclusive seat of intelligence.

"*Unity of the nervous system.* I. Not only are all the parts of the nervous system thus mutually connected, but they are all subordinated to one organ. II. The nerves and the spinal cord are all subordinated to the encephalon; the nerves, the spinal cord, and the encephalon are subordinated to the medulla oblongata, or more precisely to the vital and central point of the nervous system, placed in the medulla oblongata. III. It is with this point, placed in the medulla oblongata, that all the other parts of the nervous system must be connected that their functions may be *exercised*. The principle of the *exercise* of nervous action passes along the nerves to the spinal cord, and from the spinal cord to this point; and, having passed this point, it moves in a retrograde direction from the anterior parts of the encephalon to the posterior, and from the posterior to this point again.

"*Unity of the cerebrum, or of the organ which is the seat of intelligence.* I. The unity of the cerebrum, or of the organ which is the seat of intelligence, is one of the most important results of this work. II. The organ, the seat of intelligence, is one. III. In fact, not only do all the perceptions, all the volitions, all the intellectual faculties, reside exclusively in this organ; but all these faculties occupy the same place in it. As soon as one of them disappears by lesion of a given part of the cerebrum, they all disappear. As soon as one of them returns by the cure of the injury, all return. The faculty of perceiving and willing, constitutes, therefore, a faculty which is essentially *one;* and this single faculty resides in a single organ." (pp. 235-44.)

We are inclined to think that, in these latter propositions, M. Flourens attaches too much importance to the medulla oblongata as a distinct and peculiar organ; but it must be confessed that there is much that

yet remains to be explained in regard to the action of the brain upon the motor nerves. It may even, indeed, be questioned whether any of these nerves proceed directly from the cerebrum; and whether they have not all their real termination in the spinal cord. Upon this view, the same fibres might be excited to action either by a stimulus applied to the spinal cord itself, or by one conducted to it by an afferent nerve, or by one produced by the influence of the will through the brain; just as muscular fibre may be excited to perform the same contraction by various stimuli applied in different ways. This we believe to be the idea of Müller, though we must confess ourselves at a loss to form a definite notion of his opinions, owing to a great want of precision in the expression of them. But however it is to be explained, the *fact* that irritation of the cerebral ganglia does not in any case produce muscular movements, except when the upper part of the medulla oblongata is affected, remains as a proof that some *break* intervenes between the cerebrum and the muscle which is to be called into action; and numerous phenomena of *semi-voluntary* motion,—by which term we may designate those actions which were at first purely voluntary, but have since become more or less independent of the will through being rendered habitual by frequent repetition, such as the playing on a musical instrument,—lead to a similar conclusion.

That the cerebral ganglia are the instruments of perception, memory, the intellectual faculties, and the will, may be regarded as a position fixed by the researches of M. Flourens. That the motor principle is *immediately* derived from the spinal cord and medulla oblongata, seems also to be a legitimate deduction from his experiments; and it was by him that the principle was first brought prominently forwards, that this motor principle may be excited to action, by an external impression that does not produce sensation. The determination of the functions of the cerebellum had been previously made, without the knowledge of M. Flourens, by Rolando; and the near coincidence of the two series of results is sufficient to show that, even if neither precisely expresses the truth, neither is far from it. Similar experiments have been subsequently made by Hertwig; with corresponding results. All these experiments lead to the conclusion, that the cerebellum is the organ by which the simple motions of the several parts of the body are blended together, for the performance of the actions of locomotion, and for the balancing of the body when at rest; and this conclusion harmonises so well with the evidence supplied by comparative anatomy, as to the relative development of the cerebellum in groups of animals that are distinguished by differences in the number and variety of such combined actions, that we cannot but regard it as entitled to take rank as a physiological truth. Our phrenological readers need not imagine that the admission of this view necessarily militates against the doctrine of Gall, respecting the seat of the sexual impulse; since it is quite possible that the two functions may coexist in the cerebellum; and there are phrenologists of eminence, who have found themselves compelled by the weight of evidence to admit the truth of Flourens' views. It is not by any means a satisfactory mode of getting rid of such evidence, to say that no value can be attached to vivisections; since an animal that has undergone so severe a mutilation of its nervous centres cannot be expected to keep its

balance, walk, fly, &c. For is not the removal of the cerebral hemispheres an operation of much greater severity? And yet after this, animals may live for months, standing, walking, flying, &c., but doing all as if in a perpetual sleep. M. Flourens relates that a cock from which nearly half the cerebellum had been removed, frequently attempted to have intercourse with hens in whose company he was left; showing that the sexual appetite was not destroyed by this severe mutilation. But he could never succeed, for want of power to execute the necessary combined movements, and to retain his equilibrium.

In the subsequent part of the volume we find several papers on detached subjects of much interest; but which, having been long before the public, do not now require particular notice. One of these contains the results of a series of experiments on the reunion of nerves; in which satisfactory evidence is given of the gradual restoration of function, even when large trunks have been divided; and of renewal of the nervous substance, even when a considerable portion of it has been removed. There is also an interesting paper on the movement of the brain, observed when a portion of it is laid bare in a living animal. This is shown to depend entirely upon the distension of its vessels with blood; and hence to be in part coincident with the arterial pulse, and in part with the respiratory or venous pulse.

The most remarkable novelty in the volume is that which relates to the functions of the semi-circular canals of the internal ear, and of the nerve by which they are supplied. This nerve is regarded by M. Flourens to have an office quite distinct from that of ministering to audition; and to be concerned in regulating the locomotive actions of the body. His former volume contained a memoir on the "fundamental conditions of audition," which was presented to the Royal Academy at the end of 1824. In this memoir, M. Flourens attempts to prove that the cochlea is the essential part of the internal ear; the sense of hearing not being destroyed so long as this remains entire, even though the vestibule and the semi-circular canals have been laid open and their nervous expansion completely destroyed. The laying open of the interior of the vestibule does not, according to him, perceptibly impair the acuteness of the sense, and the rupture of the semi-circular canals seems only to render hearing painful, without making it less acute, whilst it also causes very remarkable movements of the head.

Now we must remark upon the conclusions arrived at in this memoir, that they do not appear to us by any means satisfactory. The evidence of the comparative effects produced by the various operations upon the sense of hearing, is extremely imperfect, and does not appear to us to warrant the deductions which M. Flourens has drawn from it. Comparative anatomy affords much more satisfactory indications on the subject of the essential part of the organ of hearing; for, in descending the animal scale, we find one accessory part removed after another, until there remains the *vestibule* only; and this is evidently, therefore, the chief seat of the sense.

But the peculiar function of the semi-circular canals can only be determined by experiments, and those of M. Flourens are worthy of attentive study. He submitted to the commissioners of the academy, at the commencement of the year 1825, a pigeon, in which he had cut the

horizontal semi-circular canal on the two sides, about two months previously. The immediate effect of the operation had been to cause a rapid jerking motion of the head from side to side, which continued with very little interruption; and also a tendency to turn to one side, which manifested itself whenever the animal attempted to walk forwards. The horizontal movement of the head, though less sudden and violent than it was immediately after the operation, still continued, and manifested itself particularly when the animal was excited to motion. The revolving action also was renewed whenever the animal was caused to move with rapidity. This revolution took place sometimes to the left, and sometimes to the right; but most frequently to the right. The movements continued with the same intensity for several months, during which period the animal retained its faculties, and being fed with care, it became very fat. Its head was then examined, and it was found that the two canals which had been cut, were obliterated at the points of section, and that no part of the train appeared to have suffered any lesion. M. Flourens has since repeated these experiments in a great variety of ways, and the results are contained in two memoirs, one, on the semi-circular canals in birds; and the other, on those of mammalia; which were read before the academy in 1828. The following is a summary of them:—Section of the horizontal canal on the two sides, in pigeons and other birds, is followed by a violent horizontal movement of the head. Section of a vertical canal, whether the inferior or superior, on both sides, is followed by a violent vertical movement of the head. And section of the horizontal and vertical canals at the same time, causes horizontal and vertical movements. Section of either canal on one side only, is followed by the same effect as when the canal is divided on both sides; but this is much inferior in intensity. In rabbits, section of the horizontal canal is followed by the same movement as in pigeons, and it is even more constant though less violent. Section of the anterior vertical canal causes the animal to make continual forward *somersets;* whilst section of the posterior vertical canal occasions continual backward somersets. The movements cease when the animal is in repose; and they recommence when it begins to move, increasing in violence as its movement is more rapid. The connexion of the particular movements, with section of the particular canals, is shown by the constancy with which the same effects follow the same lesion, in different animals.

These facts are probably familiar to many of our readers; and we merely recall them now, that the reasoning founded on them by M. Flourens in the two chapters on "the direction of the movements of the animal, governed by the direction of the fibres of the encephalon," and on the "moderating forces of the movements," which are here published for the first time, may be intelligible. These experiments were reported on to the academy by M. Cuvier, who pointed out the analogy between their results, and those which had been obtained by the experiments of Magendie, on section of the pons varolii, as well as those which the experiments of M. Flourens on the cerebellum had afforded. Injury of the cerebellum on one side, section of its crus on one side, and section of the horizontal semi-circular canal, all produce the same result; and it was natural, therefore, to infer that there is some condition common to all by which this result is brought about. This idea has been followed up by M. Flourens, who points

out that each of the crura cerebelli consists of three fasciculi of fibres, which correspond to the three directions of movement:—1, The transverse fibres which embrace the medulla oblongata, forming the pons varolii;—2, The postero-anterior peduncles, which pass from the cerebellum towards the tubercula quadrigemina, forming the processus a cerebello ad testes, and becoming continuous with the crura cerebri;—3, The antero-posterior fibres, which pass backwards to the medulla spinalis, forming the corpora restiformia. Now it has been shown by experiment that if the pons varolii, containing the transverse or lateral fibres of the cerebellum, be divided, the animal rolls over sideways upon itself. If the crura cerebri, containing the anterior fibres, be cut, the animal forcibly precipitates itself forward, in the same manner as if the anterior vertical semi-circular canal were divided. And if the posterior peduncles of the cerebellum be divided, the animal tends to make a series of backward somersets, in the same manner as if its posterior vertical semi-circular canal were divided. It has been ascertained by M. Flourens that lesions of the portions of the cerebellum, from which these peduncles respectively proceed, produce the same effects. The relation to which this comparison points is certainly a very curious one.

The acoustic or auditory nerve, according to our author, is not a simple nerve, but a complex one, consisting of two essentially distinct trunks, the nerve of the cochlea, and the nerve of the semi-circular canal. The first of these is the true auditory nerve, the cochlea being (according to M. Flourens) the true seat of the sense of hearing. He assures us that he has on several occasions succeeded in destroying the cochlea of rabbits without injuring the vestibule; and that the sense of hearing was totally destroyed thereby. We must take the liberty of regarding these experiments as very unsatisfactory, since their results may be influenced by a great variety of causes; and of preferring the "experiments prepared for us by nature," who takes away the cochlea in amphibia and fishes, so that whatever power of audition these animals possess must be due to the vestibule and semi-circular canals. Still lower down in the scale the semi-circular canals disappear, and the vestibule alone remains. This is the case in some of the lowest fishes, and in the invertebrata. Let it be observed, however, that our argument does not in any way go against the peculiar function ascribed to the semi-circular canals by M. Flourens; but only to show that he has by no means established their absence of participation in the sense of hearing.

The nerve of the semi-circular canals consists of three fasciculi, each of which has a distinct origin and a distinct termination; whilst the true auditory nerve has but a single origin. One of these roots is derived from the transverse peduncles of the cerebellum; another from the anterior peduncles which enter the crura cerebri; and the third from the posterior peduncles, or corpora restiformia. These three fasciculi are distributed to the horizontal, anterior vertical, and posterior vertical canals respectively. A full account of their origin and course is promised by M. Flourens in a forthcoming work on the Anatomy of the Brain. Hence it is evidently from the nervous connexions with these parts of the encephalon, that the semi-circular canals derive the remarkable power which they have been shown to possess.

M. Flourens then goes on to inquire into the mode in which these curious movements are occasioned. He considers that it cannot be said, with justice, that section of such and such a canal, or of such and such a fasciculus of fibres, *determines* the effect, but that it rather allows it to manifest itself. For it is the *absence* of the horizontal semi-circular canal which produces the lateral movement, and its integrity seems to restrain that movement.

"In fact, the action of the semi-circular canals, and of the fibres of the encephalon which are opposed to each other in direction, is rather an action which moderates, governs, controls, than a force which urges or determines. This force is composed of several. There are in the semi-circular canals, and in the opposed fibres of the encephalon, several forces which control and moderate; in fact, there are as many *moderating* forces as there are opposed directions of movement. Thus the anterior vertical canal, and the anterior peduncles of the cerebellum, moderate the forward movement; the posterior vertical canal, and the posterior peduncles of the cerebellum, moderate the backward movement; whilst the horizontal canal, and the lateral fibres of the cerebellum, moderate the lateral movement." (pp. 497-8.)

The nervous system, then, according to M. Flourens, is not only the *exciting principle* of the movements of the body, but is also the *regulating* and the *moderating* principle. These principles have their seat in distinct parts of the system. The *exciting* effect is produced by all those parts, which, by being pricked or irritated, immediately cause muscular contractions; that is, by the spinal cord, the medulla oblongata, and by the nerves. The *regulating* effect emanates from the cerebellum. The *moderating* effect resides in the semi-circular canals, and in the *opposing fibres* of the encephalon.

Now we are very far from being satisfied that any such new principles as those introduced by M. Flourens are required to explain what are certainly the very curious effects to which he has directed attention. It seems to us quite possible that the nerve of the semi-circular canals may be connected with the government of the motions of the body, without being the less a nerve of sense; just in the same manner as the optic nerve is connected with the *consensual* movements of the eyes, and with the contraction and dilatation of the pupil. There is undoubtedly much to be known respecting these consensual movements, which seem to be distinct from the reflex, in that they necessarily involve *sensation;* and which seem to bear a closer resemblance to those of the emotional and instinctive group. Their study, if prosecuted in a right direction, will amply repay the physiological inquirer; and he cannot do better than commence by repeating the experiments of M. Flourens, with all the precautions indicated by him, and others that will suggest themselves. We must own that we have not sufficient confidence in experiments of this kind to take upon trust the results which have been obtained by a single inquirer; for the recent history of neurological research affords abundant reason for hesitation in the reception of such results, until they have been confirmed by further inquiries, instituted by competent and unprejudiced experimenters.

ART. VIII.

Recherches d'Anatomie comparée sur le Chimpansé. Par W. VROLIK, &c. &c.—*Amsterdam*, 1841. Gr. folio, pp. 51. Seven Plates.

Researches into the Comparative Anatomy of the Chimpansé. By W. VROLIK, &c. &c.—*Amsterdam*, 1841.

THIS splendid monograph contains a far more extended account than had yet been published of the anatomy of that interesting group of animals which, in their conformation, most resemble man. The materials for his study, which Professor Vrolik has found in the rich museums of Holland, are such as few anatomists can command, and he has made good use of them for illustrating the anatomy of the chimpansé in comparison, not only with that of man and its immediate congeners the orang and the gibbons, especially the siamang (*Hylobates syndactylus*), but also with that of the whole race of monkeys and those of the carnivora, into whose form theirs appears to merge. We shall analyse that portion alone of the work which relates to the differences and analogies between the human form and those of the chimpansé, orang, and siamang, the three most anthropomorphous species; a subject in which, though it be not in our usual beat, few of our readers can fail to find some interest.

The first chapter relates to the *comparative osteology of the chimpansé.* The elevation of the *frontal bone* and the evenly rounded form of the head are among the most striking characters in which all the three species just named resemble man. With these, too, is connected the form of the coronal suture, which crosses over the head in a direct curve, as it almost always does in man, but which, in the lower monkeys and the animals below them, forms an angle at the vertex, the frontal bone being received into an angle formed by the two parietal bones.

The *temporal bone* is very long in the chimpansé, in correspondence with the great length of the base of the skull, and, especially of the basilar part of the occipital bone. The squamous suture is rectilinear, and is continued directly into the coronal, shutting out the ala of the sphenoid bone from contact with the parietal bone. In the orang, on the contrary, though the squamous suture is straight, the sphenoid and parietal bones are often in contact: and in this respect alone does the orang's skull generally present a more human form than the chimpansé's. The *temporal ridges* in the chimpansé and siamang are far less prominent than in the orang. In the young skull they are barely discernible, but, as age advances, they become more and more prominent and approach each other towards the top of the head. In the orang they form at last the one high ridge along the vertex of the skull, which gives it its strikingly brutal, carnivorous aspect: but in the siamang, though they give a character which contrasts strongly with that of the round smooth head of the Brahmin and Hindoo, they do not become more prominent than they are in some Caffre and Hottentot skulls. The chimpansé in regard to these ridges holds an intermediate place between the orang and siamang. Neither it, nor the orang, has either styloid or mastoid process.

Another character in which the form of all these skulls becomes, as age advances, more unlike that of the human skull, is the apparent flattening and recession of the forehead in consequence of the increasing promi-

nence of the superciliary ridges, and through the growth of the face being, even more than it is in man after the period of childhood, more rapid than that of the skull. The increase is shown especially in the growing prominence of the jaws; it is greatest in the orang, least in the chimpansé. To this enlargement of the jaws is due the length and narrowness of the palate, and also, in great measure, the backward position of the *foramen magnum*, and the occipital condyles. In man these parts are placed so near the centre of the skull that but little force is needed for maintaining the head erect; but in even these highest monkeys they lie in the posterior third of the base of the skull, and thus, so far back that a peculiar arrangement of the cervical vertebræ is needed for the support of the head.

The *nasal bone* in both the chimpansé and the orang are generally single, long, and narrow: in the former, their border is, as in man, somewhat raised: in the siamang there is also but one bone in the adult state, but it is shorter and wider than in either of the other species. In conjunction with this, the inter-orbital space is, in the siamang, wider and more man-like than in any of its congeners. The orbits themselves are in all placed high up, and proportionally larger, though less deep, than in man.

The *intermaxillary sutures* in the chimpansé are obliterated very early, and sooner than they are in the orang; and the anterior palatine foramina are, as in man, close to the roots of the incisor teeth: the palatine suture remains to the adult state.

The *lower jaw* of the chimpansé is large and long, but the chin is prominent and forms a less obtuse angle than in the lower animals: in the orang the jaw is still longer and the chin less prominent: in the siamang the chin is vertical and round, the angle of the jaw is nearly a right angle, and its coronoid process is but little elevated. In all these characters, as well as in the proportionally greater width and less prominence of the face, and in some other external characters of the skull already mentioned, the siamang approaches nearer to the human form than either the chimpansé or the orang.

The teeth are long and large in all these species, and especially in the orang: and in none of them do they form a continuous series. There is, in all, a space by the side of each canine tooth into which the corresponding tooth of the other jaw is received; a character so connected with the mode of life and nature of the food, and, through these, with corresponding peculiarities of other parts, that it may be justly regarded as establishing a generic difference between them and man; understanding by that expression a difference as great as that which exists between any kinds of animals which naturalists are agreed to place in different genera.

In the *vertebral column* of the chimpansé the chief differences are as follows:—the spinous processes of the cervical vertebræ are directed straight backwards and not bifurcated; there are thirteen dorsal vertebræ; the transverse processes of the lowest (the fourth) lumbar, are joined to the ilia: only the upper two and a part of the third sacral vertebræ enter into the formation of the sacro-iliac symphysis: the promontory of the sacrum is but little marked, and its anterior surface is not hollowed.

The *pelvis* of the chimpansé differs more importantly. It is directed

in the line of the axis of the trunk: the ilia are long, straight, and vertical, concave on their posterior (outer), and convex on their anterior (internal), surface: and hence the upper strait of the pelvis is narrow and elongated, and in a horizontal front view one can see the whole length of the sacrum and coccyx. The anterior inferior spine of the ilium is absent; the symphysis pubis very deep; the angle very acute: the tuberosities of the ischii are on almost the same level as the lower edge of the symphysis pubis, and are large, flat, triangular, and curved outwards.

The orang's vertebral column is less like that of man, especially in the great length of the spinous processes of the cervical vertebræ, their direction straight backwards or even, in those below the third, somewhat upwards, in adaptation to the weight of the head naturally hanging forwards. The cervical part of the siamang's spine is more man-like, but still is widely distinguished by the characters of its spinous processes. The orang has but twelve dorsal vertebræ; in which respect it is nearer to man than either the chimpansé or the siamang, for both of them have thirteen. The spinous processes of the upper dorsal vertebræ are, in both the orang and the siamang, unlike those of the chimpansé, directed straight backwards. The lumbar vertebræ of the orang and siamang are four in number, and are distinctly different from those of man in their articular processes being not vertical, but inclined obliquely outwards, so that they are not capable of that characteristic movement in a quadrant which is adapted to the biped gait of man.

The sacrum of the siamang is more man-like than that of the orang or chimpansé, inasmuch as all its four false vertebræ are united in one bone, which is short, broad above and gradually tapering downwards, but forms only a slight promontory, and wants the excavations, both vertical and transverse, which the human sacrum presents. In like manner, the whole pelvis of the siamang has, of those of the three species, most nearly human form, and approaches closely to that of the Bosjes-woman, of which the elder Vrolik has, in his well-known work on the national forms of the pelvis, given so complete an account. The ilia are very broad above; hollowed out on their anterior surface; and have a rounded crest and a rudiment of an anterior inferior spine. The horizontal rami of the ossa pubis form a well-marked crest, and their symphysis is narrower and more oblique than in the other species. The rami of the ischia, moreover, are not transverse, but are directed obliquely towards the ossa pubis, as in the human skeleton; their tuberosities and spines are large; and on the posterior aspect of the ilia there is the same sinuous surface as that of man presents.

The *thorax* has a greater width and a more rounded form, as one ascends from the lower monkeys towards man. The sternum in the chimpansé has a general resemblance to the human form, but its superior border has not a transverse notch, and the three pieces, analogous to those which compose the second portion of the sternum of man, remain separate. In the orang, the sternum is shorter and broader; and, up to a certain period of life, is composed of separate and symmetrical portions, of which five on each side are connected at the middle line. The two portions of the manubrium unite first, and the union seems to proceed from above downwards; but in the skeleton of even an old orang, at Leyden, it has not taken place in the two lower portions. In the siamang

the portions of the sternum unite earlier than in the orang; but it is widely distinguished from that of man by its breadth and shortness.

The *anterior extremities* of all the three species differ greatly from those of man in their length, as compared with that of the posterior extremities. In the erect human skeleton the fingers reach the inferior third of the femur; in that of the chimpansé they reach the upper third of the tibia; and in those of the orang and siamang they touch the feet. And while in man, as he advances from the fetal period, the lower extremities grow proportionally more in length than the upper, in these apes the case is reversed, and the disproportionate length of the upper extremities increases with their years.

The chimpansé's *clavicle* has the sigmoid arch of that of man, but in an exaggerated degree. Its scapula is elongated, has a rounded posterior border, and a very obtuse inferior angle; its spine is almost perpendicular to the plane of its posterior surface, and is directed nearly parallel to the axis of the trunk; and its acromion is larger and narrower than that of man. In the orang, the clavicles are less curved than in the chimpansé—in the adult, indeed, they are nearly straight; and in the siamang the sigmoid curve is not present. In the orang, however, the scapula is broader and its spine is not so nearly perpendicular to its surface; while, in the siamang, the form of this bone approaches more nearly to that which it presents in the chimpansé.

The chimpansé *humerus* has pretty nearly the human form, but is longer and stronger. The same comparatively greater length is found in the *radius* and *ulna*, which are, moreover, more curved in opposite directions than those of the human skeleton are, so that the interosseous space is very wide. Nearly the same characters are found in the corresponding bones of the orang and the siamang.

In the chimpansé the *carpus* has eight bones like those of man, except that the trapezium and trapezoid are proportionally small, the pisiform large. The trapezium, too, though placed in the same line as the others, is not turned so much backwards as it is in man, and hence the thumb, though more separated from the fingers than it is in the other quadrumana, is far less so than in the human hand. The carpus of the orang, besides having the pisiform bone sometimes divided into two, (a condition observed by Mr. Owen, but not by Vrolik,) has *nine* bones; and in this, more strikingly than in any other osteological character, the orang stands nearer to the lower monkeys than the chimpansé does. The ninth bone, which it was reserved for Vrolik to discover, even in the field which had been already traversed by Owen and De Blainville, is the *os intermedium* observed by Cuvier in the carpus of the lower apes, and described by De Blainville as common to all the other quadrumana, but hitherto unnoticed in the orang. It lies between the two rows of the carpal bones, like a separated portion of the scaphoid, which it nearly resembles in form, and it articulates with the trapezoid, scaphoid, lunar, and os magnum.

The *hand* of the chimpansé differs from that of man in the length and narrowness of its palm, the length of the phalanges (especially the last) of the fingers, their curvature, and the existence of ridges along the palmar edges of the first and second phalanges: but, much more obviously, in the shortness and slenderness of the phalanges of the thumb,

which, though it is placed so that it can be fully opposed to the fingers, is rudimental in comparison with that of man. In the orang these distinctive characters are still more marked; and, besides, the metacarpal bone of the middle finger is longer than that of the index. In the siamang, the hand approaches somewhat nearer to the human form, inasmuch as the bones composing the thumb are neither so short nor so slender as they are in the chimpansé and the orang.

The peculiarities of the *inferior extremities* of the chimpansé are, in general, their shortness when compared with the superior. In particular, the head of the *femur* is fixed to the acetabulum by a *ligamentum teres*, a part which is deficient in the orang, though present in some other apes. The *tibia* and *fibula* are both curved in opposite lateral directions; the *patella* is small; and the fibular portion of the ancle-joint is directed somewhat downwards in correspondence with the inward aspect of the sole. The *os calces* is thinner and weaker than in man, but directed straighter backwards than in other apes. The *scaphoid* and internal *cuneiform* bones are placed obliquely, so that the metatarsal bone of the great toe is not on a level with the others. The *great toe* is proportionally larger and less like a thumb than in other apes; its metatarsal bone is shorter and stronger than the others; and the middle toe is the longest of all. The whole sole of the chimpanse's foot is narrower than that of man, and the phalanges of all the toes are longer; but in all these respects the chimpansé differs less from man than any of its congeners.

In the orang, the femur is shorter and much less curved anteriorly (at least in the adult) than it is in man and the chimpansé. The tibia is much curved inwards, but the fibula is nearly straight: the internal malleolus is short and weak, the external is not pointed : the tibio-tarsal articular surface is not concave but slightly convex ; and, from these conditions, the ancle-joint is much more moveable, but, at the same time, less strong than it is in man. The tarsus is large. The upper articular surface of the astragalus is inclined from without inwards; the anterior is very small and also directed obliquely inwards. The os calcis is very thin and curved outwards, as in a human club-foot; the scaphoid bone is broad, but thin; the cuneiform bones are all small, especially the internal one, of which the metatarsal articular surface is too short to reach the ground, and is so directed that the great toe must be separate from the others: the metatarsal bones are very long and inclined obliquely outwards (instead of inwards as in man,) and that of the second toe is shorter than that of the third, while, in man, the former is the longest of all. The first phalanges of the four lesser toes are much curved, and are swollen in the middle of their shafts; the second phalanges are less curved, but are also swollen; the third are long and narrow. The hallux has a short, straight, and slender metatarsal bone, and two phalanges, or, sometimes, only one, which is straight and pointed; and, if the foot be put upon the ground, the metatarsal bones of the second and third toes rise up, and the curved phalanges are turned inwards. In numerous and striking characters, therefore, the foot of the orang differs from that of man even more widely than that of the chimpansé does.

Such are the comparative osteological characters of man, and the three most anthropomorphous apes. Our abstract of them is so brief that we

could scarcely make a briefer summary. It is evident enough from them that, according to the ordinary rules of classification, the difference is sufficiently great to justify the placing man, not in a separate genus only, but in a separate order; and of the three species the chimpansé deserves the place next to man, which Cuvier assigned to the orang. We now pass to the second chapter, which is devoted to the myological description of the chimpansé. We shall point out only the characters in which it clearly differs from man, leaving it to be understood in general that in the particulars not mentioned their conformations are similar.

Of the *muscles of the neck*, the platysma myoides forms one broad and strong muscular layer, extending from the sides of the cheeks over the whole front of the neck to the upper part of the shoulders and chest. The anterior belly of the digastricus is very large. The genio-hyoidei are proportionally stronger, and the mylo-hyoidei thinner than in man: but, as to arrangement, there is no difference in either these or the adjacent muscles of the hyoid bone. The latissimus dorsi is arranged as in man, but, near its insertion there proceeds from the lower margin of its aponeurosis a muscular fasciculus, which descends along the inner border of the triceps, and is attached by a broad tendon to the inner condyle and the olecranon. Near the levator anguli scapulæ there is a muscle (*levator claviculæ* Tyson,) which arises from the transverse processes of the second and third cervical vertebræ, and is attached to the scapular portion of the clavicle, not far from the coracoid process.

The *abdominal* and *lumbar muscles* are exactly like those of man. Of those of the *chest*, the pectoralis major alone differs, in that its sternal and clavicular portions are not separated: (in the orang there is no clavicular portion.)

In the *muscles of the arm*, a fasciculus is given off from the inner edge of the coraco-brachialis, and joins the edge of the triceps. The outer portion of the brachialis anticus is disproportionately large. The muscles of the fore-arm are hardly distinguishable from those of man. In the muscles of the fingers there is the same degree of similarity, and in those of the thumb, the chimpansé exhibits the same superiority over the orang as it does in the bones of that member. All those which are described in man exist in the chimpansé, with the exception of the flexor longus pollicis; and they differ in little, except in being smaller and weaker. The same may be said of the muscles of the little finger.

In the *muscles of the lower extremity*, there are greater deviations from the human form. The psoas is comparatively slender; the iliacus long and narrow. The sartorius is long and remarkably wide at its lower end: it terminates by expanding into a broad aponeurosis which is attached to the inner surface of the upper part of the tibia. The gracilis also is, comparatively, very wide; the pectineus broad and long; but the quadriceps extensor is hardly different from that of man. The glutæus maximus is very narrow, thin, and small; and it descends lower than it does in man, reaching towards the inferior third of the thigh; whence the almost complete absence of buttock in the chimpansé, and of that horizontal fold which divides the human buttock from the thigh. The glutæus medius, on the other hand, is large and strong, and nearly the whole of its surface is exposed. (The other muscles of the buttock could not be dissected.) The points of insertion of all the flexor muscles

of the leg are much lower than in man, and the tendon of the semitendinosus is shorter and larger. The adductors differ only in the large size of the adductor magnus.

The tibialis anticus of chimpansé is a remarkably large muscle. It is composed of two fasciculi, of which the internal corresponds with the human tibialis anticus, and is inserted into the internal cuneiform bone, while the external arises from the head of the fibula, descends by the side of the internal one, passes under the annular ligament, and is inserted into the proximal extremity of the metatarsal bone of the great toe. This arrangement is adapted to the vigorous action of the great toe, to which the fasciculus serves as an abductor; and, in correspondence with it, the fasciculus of the extensor brevis digitorum which is appropriated to the same member is separated from the rest and peculiarly large. There is no peroneus tertius, and the peroneus brevis is broad and strong.

The gastrocnemii descend low down the leg of the chimpansé to the short and broad tendo achillis, and form no abrupt and prominent calf. The soleus arises by a horizontal line from the posterior surface of the tibia, and is not fixed to the tendo achillis till near the os calcis. The flexor longus pollicis is very strong, and larger than either of the other deep muscles of the back of the leg: the abductor pollicis, also, is proportionally large, and the flexor brevis and adductor pollicis are both well developed.

The arrangement of the *muscles of the sole* is very remarkable. Immediately under the skin there is a strong muscular fasciculus, the analogies of the flexor brevis and flexor accessorius digitorum, which occupies a great part of the sole, arising from the os calcis, and passing to a tendon for the middle toe, and some other tendinous bands. Above it is the tendon of the flexor longus digitorum, which divides into several smaller tendons; and upon these are four lumbricales which go to the tibial borders of the first phalanges of the second, third, fourth, and fifth toes. From these tendons there proceeds in the first place an aponeurotic prolongation towards the second toe, on arriving at which it separates into two parts, of which one, dividing, is attached to the borders of the second phalanx, while the other passes along the middle of the toe to the third phalanx. For the middle (third) toe, there are, first, the principal tendon of the flexor brevis, which, dividing, is attached to the sides of the second phalanx; and secondly, the tendon from the flexor longus hallucis, which passes between the two portions of the former to the third phalanx. For the fourth toe, in like manner, there are two tendons; one from the flexor longus digitorum, which bifurcates and is attached to the borders of the second phalanx, and another from the flexor longus hallucis which passes along the middle to the third phalanx. The fifth toe, on the contrary, has only the one tendon from the flexor longus digitorum. Thus, the muscle corresponding to the human flexor longus pollicis gives tendons to three toes, viz. the first, third, and fourth; and there is a tendon for each phalanx of the second, third, and fourth toes; the first phalanges receiving the tendons of the lumbricales, and the second and third those of some of the three other flexor muscles. The little toe, besides its tendon from the flexor longus, has a flexor brevis and an abductor muscle like those of man.

In the third chapter Vrolik compares, at considerable length, the muscles of the champansé with those of the gibbon, orang, several species of macacus and loris, kangaroo, opossum, lion, bear, unau, utia, and zebu; all of which he has himself dissected, and of which his descriptions form part of the materials from which he proposes to publish, at some future time, a complete comparative myology. But, important as the chapter is, it is beyond our present design; and we proceed to the fourth, which contains the

Comparative neurology of the chimpansé. The brain of the specimen dissected by Vrolik was unfortunately decomposed; so, for a kind of compensation, he has given an excellent sketch of a vertical section of the brain of an orang, by which he has completed "the phrenological iconography" of that species, which the works of Sandifort and Tiedemann had, in this respect, left imperfect. His plate exhibits a character of difference from the human brain, which had not hitherto been pointed out; namely, that the corpus callosum, whose posterior border corresponds, in man, to the testes, does not, in the orang, reach even so far back as the nates.

The description of the chief branches of the spinal and of some of the cerebral nerves is nearly complete. The vagus, glosso-pharyngeal, and hypoglossal nerves have almost exactly the same arrangement as in man; but there is an important peculiarity in the accessorius. After leaving the skull it divides, as in man, into an external and an internal branch: the external is distributed almost exclusively to the trapezius muscle; and the internal, instead of joining the trunk of the vagus, passes separately to the larynx, which it enters above the os hyoides. We cannot say what important evidence this fact affords for Bischoff's and Longet's view that the true motor nerve of the human larynx is the accessorius which sends its fibres to that organ through the medium of the vagus.

The arrangement of the cervical nerves of the chimpansé is identical with that in man; and nearly the same may be said of the nerves of both the upper and lower extremities, which do not present one important peculiarity.

There is the same similarity in the arrangement of the blood-vessels, which are described in the fifth chapter; and it extends even to the existence of three trunks arising from the arch of the aorta, in which (as far as is yet known,) the chimpansé and the orang alone resemble man.

The sixth and last chapter is devoted to the comparative splanchnology, and especially to the account of the larynx and laryngeal pouches. Of the latter, Vrolik proves, by the comparison of those of numerous species of monkeys, that they regularly increase in size with advancing age, and that, in general, they are larger in males than in females. He discusses also their probable purpose, and concludes that they are not intended to modify the voice, but for reservoirs of air to diminish the specific gravity of the animal, and thus to facilitate the climbing and almost bird-like movements of some of the species.

Of the other viscera, the heart and lungs are entirely like those of man; the liver is very large, and has no groove for the passage of the inferior cava; the gall-bladder resembles a tortuous duct rather than a bladder; the spleen is elongated with an inferior caudal prolongation; the stomach has the human form; the cæcum has an appendix vermi-

formis differing from that of the orang, in that it is separated from the cæcum by a constriction, as it is in man. The urinary organs present no peculiarity.

The value of this new proof of Dr. Vrolik's industry and knowledge may be estimated from this abstract, but we cannot justly conclude without a remark on the manner in which the work is executed. The plates, engraved on stone from nature by two Dutch artists, C. G. R. Meyer and E. Taurel, are not only exact in anatomical details, but are some of the most beautiful specimens of lithography that we have ever seen: those of the skeleton and of the abdominal muscles might be taken for models by any anatomical artist. The printing and the whole style of the work are equally admirable; they are such as are rarely seen in England, except in a few books printed by rich public bodies.

ART. IX.

1. *Beobachtungen auf dem Gebiete der Pathologie und Pathologischen Anatomie, gesammelt von* Dr. JOH. FRIED. HERM. ALBERS, Professor der Medezin, &c. in Bonn. Dritter Theil.—*Bonn*, 1840. 8vo, pp. 195.

Observations on the Department of Pathology and Pathological Anatomy, collected by Dr. J. F. H. ALBERS, Professor of Medicine, &c. at Bonn. Third Part.—*Bonn*, 1840.

2. *Waarnemingen in het Gebied der Pathologie an der Pathologische-Anatomie.* Door Dr. J. F. KERST, Chir. Mazoor, &c.—*Utrecht*, 1839. 8vo, pp. 214.

Observations in the Department of Pathology and of Pathological Anatomy. By Dr. J. F. KERST, Surgeon-major, &c.—*Utrecht*, 1839.

3. *Bijdrage tot de Ontleedkundige Ziektekennis.* Van F. S. ALEXANDER, Med. Doct. en Prof. to Utrecht.—*Gorinchem*, 1835. 8vo, pp. 38.

Contributions to Anatomical Pathology. By F. S. ALEXANDER, M.D., and Professor of Medicine at Utrecht.—*Gorinchem*, 1835.

I. THE first part of Professor Albers' work was noticed in our Fifth Volume, and we have often, both before and since, had occasion to speak of his writings.* Yet there has seldom been reason to praise the results of his labours; for though there is much accuracy in his descriptions of what he actually sees, yet there is a want of discernment of the differences in the things which he investigates, and an unwarrantable freedom in generalizing from a few facts, which entirely neutralize his claim to merit. The present series of essays clearly demonstrate both these faults.

The first essay is "*on the simple ulcer of the stomach*," which commonly terminates by perforation of the walls of that organ, and fatal peritonitis. On the morbid anatomy of the disease nothing is said beyond what is generally known. The author enters at some length into the diagnosis of its early progress; and his remarks might have been quoted, had he not seemed so much more definite than he is warranted in being by the evidence before him, as to excite suspicion that he speaks rather from what the morbid anatomy of the disease suggests of its progress

* See vol. II. p. 535; vol. III. pp. 220, 246; vol. VIII. p. 544; vol. IX. p. 445.

during life, than from actual observation of the signs of its course. On the experience of only five cases he describes the disease as passing with well-marked characters through several distinct stages; and omits to notice, as if he were ignorant of the fact, that its existence is often not testified by any symptom whatever, till the perforation takes place.

The second paper is "*on somnambulism*, and will be noticed elsewhere. The third is "*on dilatation of the bronchi forming cavities*;" but it contains nothing with which English readers are not familiar. The fourth relates to "*encysted tumours in the perichondrium of the thyroid cartilage*." It contains a description of two preparations, in each of which that part was occupied by a tumour nearly an inch long. In one the tumour was composed of a fibrous tissue in concentric layers inclosed in a dense fibrous capsule; in the other it consisted of a cyst half a line thick, containing a cellulo-fibrous, and a granular, pultaceous substance. The author compares these growths with one of a similar kind found within the larynx, and described in the first part of his work.

The fifth essay relates to a case in which a fluid containing fibrine, and separating after its removal into serum, and a coagulum was drawn from the interior of the abdomen. But there is no evidence to show what was its source.

The title of the last and longest essay is "*on hyperostosis, osteoporosis, and exostosis*." Here, again, we think it unadvisable to notice the author's account of the signs of the diseases he describes; for there is no reason to believe that he observed them during life. On the contrary, he acknowledges that he is acquainted with the appearances of the diseased parts only through the old preparations in the Bonn Museum, and a few examinations made soon after death. Perhaps too his morbid anatomy might as summarily be passed over; but in pointing out his errors, some useful truth may be told.

His primary division of hyperostosis is into hypertrophy of the medulla, or *osteomyelosis*, and hypertrophy of the compact tissue of bone, or *hyperostosis* properly so called.

In his account of the first he confounds two distinct affections; rickets, and atrophy of bone. He speaks of the excessive fat being modified so as to be of a "dark dirty red, yellow, or even ash-gray" colour; and says he found it thus in the interior of the femur of a rickety child. Now the material here described is not fat, but a peculiar gelatinous substance which occupies large spaces formed in the cancellous tissue of rickety bones, and in its general aspect resembles soft provisional callus. This therefore is not hypertrophy of the medulla, nor does it bear any relation to that which is rather less improperly so called. Of this the author says, that the medulla either is very consistent and exactly fills all the cavities, (which, we think, is only what is healthy in a fat man;) or, "the wall of the bone has at the same time diminished, and the osseous lamellæ which form the cells of the medullary membrane, are become thinner and have decreased in numbers. I could," he adds, "in one case remove the medulla out of the cavity without any trouble whatever, and I found only a few lamellæ forming cells; yet the femur belonged to an adult man, who died apoplectic." The description is very accurate, but the change is no hypertrophy; it is the usual condition of bones which have been atrophied through disuse, either because the patient has

been bed-ridden, or (when the change affects only certain bones,) because, in consequence of paralysis, a stiff joint, or some such condition, the muscles have long ceased to move them. In short, the "hypertrophy of the medulla" of Professor Albers, is ordinary atrophy of bone. The patient in whom he found it had, without doubt, been hemiplegic long before his fatal attack of apoplexy.

Hypertrophy of the osseous substance, or hyperostosis properly so called, is divided into four kinds; 1, Simple hyperostosis of the bone; 2, Hyperostosis with a morbid alteration of tissue; 3, Hyperostosis with coincident new formation of bone on the outer surface; 4, Local hyperostosis, exostosis and osteophyte.

Nothing can show better the impropriety of making morbid anatomy the basis of pathology. The author describes the distinctive signs of each of these states of disease; yet the most common condition is that in which the first three exist together. The majority of the bones that have been affected with rheumatism are at once thicker and denser than usual, distinctly altered in their tissue, and beset with growths of bone from their outer surfaces. So are those near which ulcers of the integuments have long existed. And, on the other hand, the state defined in No. 1 may be the result of several different circumstances; for instance, of the natural growth of an excessively used bone, of the growth of bones, such as those of the skull, which sometimes increase as the volume of the organs which they contain decreases, and of simple chronic inflammation. The author's description of these three forms of disease might therefore be merged in one: or rather they should be arranged so as to illustrate each of the several processes of which they are the results.

It would be unprofitable to analyse all that is said of thickened bones, and we will therefore only report what is said of their microscopic structure. It is, that the corpuscles in bone which is merely thickened, are smaller than in normal bone, and that the calcigerous canals are fewer and more minute, and in some parts even entirely absent. The osseous substance moreover is thickly scattered with black spots formed by a deposition of pigment, and varying in size up to that of a pin's head. He believes therefore that the hypertrophy consists, not in an actual growth of bone, but in a mere addition to the earthy matter. On consulting the drawing which is appended to the work, we find, however, reason to doubt this conclusion; for it exhibits the corpuscles as large, and the canals as numerous as they are in the majority of sections of bone, and, if we are not much mistaken, the spots of pigment, as the author considers them, are oblique sections of Haversian canals. They have just the same form, size, and direction, as those canals: they branch like them, and have the same general appearance; and they resemble no kind of pigment whatever. The author's observations therefore do not seem to warrant the conclusions, which is opposed to the results obtained by others, that the addition to bones when they are merely hypertrophied is anything but normal osseous tissue.

In the bones that are thickened with considerable alteration in their obvious structures, the case is different. The new bony substance shows no trace of corpuscles or canals, but an irregular appearance, as of longitudinal fibres with scattered granular dark bodies.

Of exostosis, or partial hyperostosis, the author describes three kinds,

namely, ivory exostosis, cellular exostosis, and exostosis formed by the expansion of one side of the medulla. But we cannot find that he adds anything important to that which is contained in many works on the anatomy of tumours, and of diseased bones.

II. Dr. Kerst's observations are of more modest pretensions. They consist of nineteen cases illustrative of morbid anatomy, and require no further comment than that they are clearly and honestly related. We shall make abstracts of those which are most interesting.

The first case relates to the examination of the eye of a dog which had been acutely inflamed in consequence of severe external injury a year and a half before death. The cornea was flattened and completely opaque; and formed the base of the cone-shaped body into which the globe had shrunken. Immediately behind the cornea there was an elevated fibro-cartilaginous ring, about four lines broad, and going all round the globe; it was produced by a thickening of the sclerotica, which was here from a line and a half to two lines thick. Behind it the globe felt empty; there was no trace of hyaloid membrane or vitreous humour, but, in their place a small quantity of sanious fluid. The posterior part of the retina was very thin, and had a finely-granular surface; the anterior was thickened; it was everywhere inseparably adherent to the choroid, which was itself healthy. At the posterior surface of the lens there was a tough and very vascular membrane firmly attached by its margins to all the adjacent parts; the capsule was thickened and wrinkled. The iris was converted into a black shapeless mass, which filled the posterior chamber; it did not adhere to the capsule, but, in front, it was closely adherent to the cornea, and, at the situation where the pupil should have been, dipped into its substance. The optic nerve seemed healthy. (Kerst, p. 11.)

The fourth observation of an "excessively large fibrous tumour taken for an aneurism" has the more interest from the error having been committed by Dr. Engelhardt, one of the most skilful surgeons in Holland. The patient was a soldier, nineteen years old, who said that about three weeks before his admission, as he was walking, he felt a sudden pain in the left ham, and on reaching home found a swelling there, which in twenty-four hours increased to the size which it presented on his arrival at the infirmary. At this time the left knee was much enlarged, but the pain had not increased. There was a swelling as large as a goose's egg in the ham, partly compact, partly fluctuating, and, on moderate pressure, distinctly pulsating. In the notion that it was a false aneurism compression was employed for some time; but as the swelling increased, and œdema of the leg came on, the femoral artery was tied. (Two ligatures were applied and the vessel was divided between them; a plan which Dutch surgeons are perhaps not aware that the countrymen of the Hunter whom they so admire, have long since abandoned.) After the operation, however, which in itself was successful, the tumour continued to grow as rapidly as before; the patient began to cough as if from organic disease of the lungs, and gradually sunk.

On examining the body the internal organs were found healthy, except the right lung, which was completely converted into a brain-like mass. The tumour in the ham was fifteen inches long and twenty-four inches in

circumference. It was pear-shaped, and lobed, and adhered firmly to the periosteum of the femur and tibia. On its surface were five sacculi, holding between five and six ounces of a sero-sanguinolent fluid, and apparently formed by the separation of the two layers, of which a tough fibrous membrane, enveloping the whole tumour, was composed. The tumour itself consisted, in part, of a dry fibrous reticulated tissue, divided by partitions of looser tissue into lobes, and separated into two chief portions by a plate of bone; and, in part, of a softer substance, easily broken, rose coloured, interspersed with fibrous tissue, and containing many cells filled with serous fluid. The femur and tibia were healthy. So also was the joint, except that there was a small mass of soft substance, like that just described, on the inner surface of the synovial membrane. The artery ran over the tumour, and between two of the sacs of fluid on its surface: and through these its pulsation had been so communicated as to give the sensation of the whole tumour pulsating. (Kerst, p. 27.)

The next interesting case is one of tubercles in the heart. The patient was twenty-one years old, and died excessively emaciated, but without any symptom that excited suspicion of disease of the heart. He had crude tubercles at the upper part of the left lung, and both mediastina were filled by enlarged and tuberculous lymphatic glands. The heart was of ordinary size, and was everywhere as closely adherent to the pericardium as if the latter had been its outer membrane. Between the two, however, there were masses of tubercle, some as large as a hazelnut. At the upper part of the outer wall on the right side, there was, near the surface of the muscular tissue, a tubercle as large as a pea, and at the apex one much larger. All the tubercles had gone into the stage of softening. The muscular substance around them exhibited no morbid change, and the vessels were unaltered; but there was yellow-softening of the outer part of the wall of the left ventricle; a layer of muscular substance, thus diseased, lying between the healthy external and internal layers.

The ninth and tenth cases relate to tuberculous matter occurring in the vertebræ. They are detailed as very rare occurrences, and the author says, that before meeting with them he had sought in vain for examples of the disease. Tilanus also, one of the most experienced surgical pathologists in Holland, in his "Schets der Heelkunde," makes no mention of tuberculous disease in bones. These facts are of much importance, for since scrofula in all its other forms is abundant in Holland, they add to the evidence that the French pathologists and their followers in this country are in error, when they speak of tuberculization as an ordinary change in the osseous tissue. It is one of the rarest.

The eleventh case illustrates one of the most serious, and not an unfrequent result of operations on the testicle. A man, aged forty-one, to all appearance of a healthy constitution, had enlarged testicles. The disease had existed for a year, and they had both degenerated into a kind of medullary substance. They were both removed at once; and in the evening after the operation the patient began to complain of pain and tenderness of the abdomen, which rapidly increased, assumed all the characters of acute peritonitis, with scarcely any appearance of inflammation externally, and proved fatal on the fourth day. At the exami-

nation all the thoracic and abdominal viscera were found healthy; but the whole of the superficial fascia and the subcutaneous tissue of the walls of the abdomen and the sides of the trunk were converted into a grayish-black soft *putrilage*, mixed here and there with pus. The degeneration extended from the scrotum to the axillæ and the scapulæ; but near the latter had not reached so extreme a stage. The spermatic cords and the inguinal canals were healthy.

Shortly after this case similar symptoms occurred in a man who had been tapped for hydrocele, and from whom a portion of the tunica vaginalis had been excised. Suspecting from his experience in the preceding case what had happened, Dr. Kerst at once made free incisions through the integuments of the abdomen; large portions of dead cellular tissue were discharged through them, healthy suppuration was then established, and the patient recovered. (Kerst, p. 90.)

The rest of the observations are of less interest than those which we have abstracted: the last five are cases of fatal purulent phlebitis, after wounds and operations, but they are not sufficient to throw any fresh light on that obscure disease.

III. In Professor Alexander's pamphlet the first three cases contain post-mortem examinations of lunatics, in all of which he found "adhesion of the pia-mater to the surface of the brain; an adhesion which was in many parts so firm that the membrane could not be removed without tearing the substance of the brain." (p. 11.) He regards it as a sign, that in all these cases there had been inflammation of the adherent parts.

The eighth observation is a remarkable one of accumulation of fat in the thorax and abdomen. The patient was a man forty years old, who had lived freely and suffered much from dyspepsia. For a long time before his death he had complained of pain and oppression in his chest, which had been variously treated without success: and to these succeeded shortness of breath, so that he was often obliged to stop when he was walking. No distinct disease, however, could be made out, but he grew worse and the oppression in the chest disabled him from all occupation. Shortly before his death he was affected by catarrh, which greatly increased his dyspnea; he became feverish and excessively restless; he could not be kept in bed, his distress was so great, and, as he was sitting up, he died suddenly.

On examination, his body was found robust; but there was a want of correspondence in the limbs, and the shape of the trunk was more like that of a woman than that of a man. A large quantity of fat was accumulated in the integuments. The right lung was completely adherent; the left rather less so; the tissue of both was healthy. The anterior mediastinum was filled, and the front of the diaphragm was thickly covered by fat of various consistence. The pericardium was covered by two lumps of fat of such a size that they must have impeded the heart's action. The heart itself was very fat, and its vessels were congested, but it was not otherwise diseased. The abdomen seemed to be filled by one shapeless mass of fat of several pounds weight: the mesentery, mesocolon, and all the parts in which it is usually found in moderate quantity were overladen with the same substance. The liver was very large; the gall-bladder very small; but all the other abdominal organs were healthy. (Alexander, p. 30.)

After what was said of the feminine form of the trunk, it is to be regretted that the state of the genital organs was not noticed in this case. We remember examining a very similar case in which the accumulation of fat was referrible to degeneration of the testes.

The last observation is one of supposed perforation of the intestine by an ascaris: but, as in many cases of the kind, there is not sufficient evidence that the perforation was not spontaneous, and that the worm did not escape into the abdomen some time after it took place, or perhaps even after death. This view is the more probable, because the patient had phthisis; the aperture was in the middle of an ulcer, resembling a phthisical one, in the jejunum; and there was another ulcer of the same character in the ileum.

ART. X.

Cases of Peritoneal Section for the Extirpation of Diseased Ovaria, by the large incision from Sternum to Pubes, successfully treated. By CHARLES CLAY, Member of the Royal College of Physicians, &c.—*London*, 1842. 4to, pp. 18.

DR. CLAY'S work is a reprint of several papers which originally appeared in the pages of the Medical Times. It contains the particulars of five cases in which gastrotomy was performed by Dr. Clay, and in three of these cases the patients recovered. This measure of success, backed by the evidence of statistics, which show, according to Dr. Clay, that twelve out of thirteen patients who underwent the operation ultimately did well, induces him to recommend it as "a perfectly legitimate and more than ordinarily successful capital operation." But, not merely does he conceive the propriety of extirpation of the ovaria to be fully established by these cases, but he regards them as forming the commencement of a new era in operative surgery. Nor can we dispute the correctness of this opinion if he should ever put his own intentions in practice, for he tells us: "I feel I should be justified in making extensive incisions into the abdominal cavity, for other objects than the one here related. I would ask, what should prevent the spleen from being extirpated when diseased? Or fatty tumours of the omentum? Or tumours of the fundus uteri?"

We believe, indeed, that the good sense of mankind will answer these questions, and that Dr. Clay's offer to extirpate some offending viscus will be met by most people with the "*minime tangetur*" with which the hero of Slawkenbergius's tale declines all manual interference with his proboscis. But let that pass. Dr. Clay, though in one place he asserts that this operation, "when compared with the results of lithotomy, lithotrity, and even amputation, stands in a far more favorable position;" yet seems to have some lurking misgivings on the point, since in another place he talks of repeating the operation until he has ascertained its real value. We give the whole passage in Dr. Clay's own words, the rather that it shows at once his estimate of his own merits as well as of the operation. It would not have occurred to us to place the author in the temple of fame by the side of Harvey, Jenner, and Cullen, even if his future efforts should be met by the profession in the way he anticipates.

"It is my intention to persevere in this operation until I have produced sufficient evidence to establish a legitimate operation of British (as it is already of foreign) surgery; or until I discover enough to condemn it. In accomplishing this, I shall find much greater difficulty in overcoming the feeling of reluctance generally manifested by the profession towards any new and bold step (though supported by numerous proofs), than I should in meeting with condemnation for the unfortunate result of some one or two isolated cases, which in no way affects the principle of the operation, but affords arguments to those who are prejudiced against its being adopted as a legitimate means of relief. In this, however, I am not singular: Harvey met with the most virulent opposition, for many years, to doctrines since acknowledged to be founded on truth. Jenner's discoveries were as strongly opposed, and as firmly admitted subsequently. Cullen's views were attempted to be driven out of the pale of the professional inquirer by the numerous followers of Brown." (p. 16.)

Before we proceed to consider the statistical grounds on which Dr. Clay defends the operation of gastrotomy in ovarian disease, we must briefly narrate the particulars of those cases in which he was himself the operator. In the management of them great credit is due to him for his sedulous attention to some minor points such as the temperature of the room, &c., the neglect of which may probably have contributed to the fatal result of other cases. The minute details too that he has given of the history of some of the patients and of their progress after the operation are highly praiseworthy, and induce us to acquit him of wilful disingenuousness in his erroneous statistics, though we cannot but consider him guilty of most culpable negligence in not investigating the subject with greater care before proceeding to dogmatize upon it, in the "Sir Oracle" tone which prevades his pamphlet.

Dr. Clay's *first case* was that of a married woman, forty-six years of age, mother of eight children. Three years before coming under his care she had noticed her abdomen begin to enlarge, and at first she thought herself pregnant, but gave up this opinion on finding the menses occur with regularity notwithstanding the progressive increase in the size of the abdomen. The disease was unattended with pain, the sensation being merely that of weight and incumbrance which she felt on the right earlier than on the left side. After consulting various medical men, none of whom gave the patient any hopes of relief, she applied on June 10th, 1842, to Dr. Clay, her abdomen being then as much distended as it is usually at the ninth month of pregnancy. The pelvis was found to be filled with a large immoveable tumour, which appeared distinct from the uterus; and the uterus itself was lifted up higher out of the pelvis than usual, the os uteri resting against the upper and inner portion of the symphysis pubis. Dr. Radford, who saw the case, was of opinion that the tumour was formed by a greatly enlarged ovary, most probably the right; that it was free from adhesions beyond its principal attachment; and that with it there coexisted a small quantity of fluid in the peritoneal cavity. Trial was made of iodine, without any local benefit, while the patient's health seemed to suffer from its use; and the chances of recovery afforded by the operation as well as the hazard attending it having been explained to the patient she resolved to submit to it, and accordingly it was performed on September 12th, 1842. An incision was made along the linea alba from the sternum to the pubes, but leaving the umbilicus a little to the left. The peritoneum was then laid open to the same extent, the ovarian

tumour was found free from any but three or four very slight adhesions, and one very firm connexion with the omentum. It sprang from the right side and was attached by a pedicle three inches in breadth to the broad ligament. A ligature being passed around the pedicle it was divided, but several vessels continuing to bleed, it was found necessary to tie each of them. From making the incision through the integuments to the removal of the tumour out of the abdomen about twelve minutes and a half elapsed, and about fourteen ounces of blood were lost, and the wound having been closed by the interrupted suture, and by strips of adhesive plaster and a bandage, the patient was replaced in bed about forty-five minutes from the commencement of the operation. The tumour weighed seventeen pounds five ounces, its largest circumference was three feet eight inches, its form rather oval. It consisted of one large and several small sacs containing a glutinous matter, and of a solid portion made up of very small cells filled some with pus, some with a brain-like substance, the intestines between them being fibrous and cartilaginous. There were besides about six pints and a half of fluid in the peritoneal cavity.

The pulse, which was 70 before the operation, continued for the next twenty-four hours at about 90, and rose but once above 100. It was soft and compressible, except at the eighth and nineteenth and a half hour after the operation; and at each of these times the patient was bled, first to ℥xiv. then to ℥viij. Enemata were given to act on the bowels; morphia and hyoscyamus to procure sleep: some vomiting and tympanitis were the only symptoms at all troublesome. The wound was not dressed until the fifth day after the operation, when it was found to have united, with the exception of half an inch at the umbilicus and half an inch where the ligature passed out of the abdomen. The absence of all grave symptoms is the chief point in the subsequent history of the patient, who left her bed during part of the day on September 28th, and returned to her household duties on October 3d, the twenty-third day after the operation.

CASE II. A woman aged fifty-seven, mother of nine children, was tapped for ascites on September 27th, 1842, when twenty-five pounds and a half of fluid were removed. A tumour was then detected in the left iliac region; circular, rather flattened, moveable, but connected with the abdominal parietes for the space of about two inches square. The patient had not been aware of the existence of any tumour ten months before; but within that period the fluid had begun to collect in the abdomen. It does not appear that she suffered except from the unwieldiness of her abdomen, and from pains about the umbilicus. Her general health had been good, but she had grown much emaciated within the last six or eight months. On October 7th, 1842, the operation was performed, and Dr. Clay was surprised to meet with adhesions in every direction; he having expected to find the tumour free, except in the vicinity of the umbilicus. The adhesions were divided by the scalpel; a strong double ligature was passed through the central expansion of the pedicle and tied both ways, a proceeding which was found adequate to secure the vessels without tying them separately, as in the former case. The operation was completed in about ten minutes, and the patient was replaced in bed in twenty-five minutes, not having lost more than two ounces of blood.

The tumour weighed five pounds: it was composed of a white tough membranous bag, capable of holding about four pints, and of a flattened round solid mass, the cells of which contained various matters from the consistence of pus to that of cerate, and of considerable variety in colour. This patient had not a single bad symptom. The wound was dressed on the fourth day for the first time. On the ninth day she was allowed to sit up, and on the 16th was considered as cured.

The *third* successful case is that of a woman named Hannah Edge, aged thirty-nine, the mother of three children. She first noticed a considerable enlargement of the lower part of the abdomen, on the right side, seven years previously. Four years before coming under Dr. Clay's care she was delivered of her third child; but after its birth her abdomen continued nearly as large as that of a person at the full period of pregnancy. She had been tapped four times; and on November 3d, 1842, the operation was repeated, when thirty pounds of dark glairy fluid were removed, and, on introducing a longer trocar, other thirty pounds of bright limpid fluid were drawn off. It was determined to perform the operation of extirpation; and this was accordingly done on November 8th. Owing to some oversight of the printers, the particulars of the operation and the description of the tumour are omitted. It appears, however, that the adhesions were numerous, but that the patient nevertheless did well; and from the detailed account given of her progress from the termination of the third day, her recovery appears to have been more rapid than that of either of the other patients, since on the eighth day she sat up, and on the twelfth was so well as to require only occasional attendance.

In neither of the two fatal cases were the histories of the patients such as to induce Dr. Clay to recommend the operation, though he felt justified in performing it in consequence of their urgent solicitations.

The first case was that of a woman aged forty-seven, who, though married eight years, had never had any children. She had first noticed a tumour in her left iliac region seven years before applying to Dr. Clay, but for the first three years it had enlarged very slowly. Its subsequent increase in size had been rapid; and at the time when seen by Dr. Clay her abdomen measured forty-five inches in circumference at the umbilicus. Her sufferings had been considerable; and two years before she had been tapped, but only two pints of bloody fluid escaped. It was her earnest wish to undergo the operation for the removal of the tumour: an incision was accordingly made, but the growth was found so universally adherent that all attempts to remove it were considered unwarrantable, and the wound was closed immediately. The case went on tolerably favorably until the fifth day, when the patient was seized with pain of the left leg, which had much the appearance of a limb affected with phlegmasia dolens. Some relief was afforded by warm fomentations; but the pulse sank very rapidly, and the patient died on the morning of the sixth day, in less than twelve hours from the commencement of the attack. The attack is attributed by Dr. Clay to the woman's husband having given her some gin and garlic on the morning of the fifth day after the operation.

Few particulars only are recorded of the patient who forms the subject of the second fatal case. She was forty-five years old; the abdomen had the size which it presents in a person at the eighth month of utero-

gestation. The abdominal tumour had a hard and lobulated feel; no fluctuation could be anywhere detected in it, but it seemed not to have the slightest peritoneal attachment. An incision was made into the abdomen; the tumour was found free from peritoneal attachments but adherent to both fallopian tubes, and involving a considerable portion of the uterus. Ligatures were applied round the fallopian and uterine attachments, and the tumour was removed. Before this was accomplished, however, the patient had fainted; the ligatures were insufficient to prevent hemorrhage from the divided vessels; the patient fell into a state of syncope, and died within an hour and a half from the removal of the tumour.

Before we proceed to the examination of Dr. Clay's statistics it will perhaps be but right to narrate the particulars of a case in which Mr. Walne, encouraged by Dr. Clay's success, removed a large ovarian tumour by an operation similar to that above described, and in which the patient recovered. She was a married woman, fifty-eight years of age, who had had five living children and several miscarriages, and had ceased to menstruate at the age of fifty-four. Her health had been good; but for more than two years she had been gradually getting larger in the abdomen, in which was a circumscribed fluctuating tumour, but without any signs of general dropsy. She measured thirty-seven and a half inches round, and seventeen inches from scrobiculus cordis to pubes. It was determined to operate; and the operation was done in a room heated to 70°, at 4 P.M. on Nov. 6th, 1842. The incision was thirteen inches long; made in the linea alba, but avoiding the umbilicus. The right ovary was found to be the one affected; a double ligature was placed by means of a needle around the pedicle, which was then divided and the tumour, weighing sixteen pounds, was removed. A large artery in the pedicle was now tied; but as oozing of blood from the pedicle continued, a ligature of stay silk was placed around its circumference. The wound was brought together by a dozen interrupted sutures, and a broad bandage was applied round the abdomen. The pulse after the operation was 76, and the patient was pale and cold. No unfavorable symptom occurred for the first twenty-four hours: almost total abstinence from food and drink was observed. The second night was less good; considerable fever and restlessness came on, and continued during the following day, with occasional vomiting and eructation. Great relief was afforded by an enema of warm water, and an anodyne was followed by some hours of comfortable rest. On the 9th the patient passed urine of her own accord, and was going on well; on the 10th there was some restlessness, with vomiting and griping, which were more distressing on the 11th, with constant nausea and frequent hiccup. These were checked by anodynes, though they did not altogether cease, and returned occasionally; the hiccup not finally disappearing till about the 20th. On the 23d she sat up several hours; on the 27th the ligature which had been placed round the artery of the pedicle was removed; on the 29th the wound was healed, except a small opening at the bottom near the pubes. The greatest circumference of the tumour was two feet ten inches and three quarters: it contained fluid in one or more cysts; a portion, the size of two fists, had a scirrhous hardness, but Mr. Walne did not choose to spoil the preparation by cutting into it.

Including this first case of Mr. Walne's, Dr. Clay reckons thirteen in

which the operation has been performed, of which only one terminated fatally; while five of the ten patients operated on by the small incision, as practised by Mr. Jeaffreson, died. His own fatal cases he places in a different category, as being instances in which an attempt was made to remove anomalous malignant uterine tumours. To this, however, we object, as by no means a fair mode of estimating the results of the operation. It is clear that Dr. Clay thought himself warranted in performing the operation in these cases; that he anticipated from it a prolongation of the patients' lives; but that in this he was mistaken, and that their death was greatly expedited by his interference; and, further, we have no doubt but that if he had been at all aware of the real nature of the diseased growths he would not have attempted their removal. This very impossibility of knowing beforehand the exact condition of the organs which it is proposed to extirpate, (of which Martini's case, and that related by the anonymous writer in Froriep's Notizen, are melancholy examples,) forms in our opinion one of the strongest arguments against the operation; and will always do so until we are furnished with some unerring means of distinguishing between simple encysted ovarian tumours, and cases in which the ovarian cysts are associated with other diseases of those organs. To leave out those cases in forming an estimate of the operation seems to us as unjust as it would be, in calculating the results of lithotomy, to omit all instances in which the patient's danger had been aggravated by disease of the bladder or kidneys.

Dr. Clay's statistical account of cases in which dropsical or enlarged ovaria were extirpated by the large incision, is as follows:

	Successful.		Fatal.
L'Aumonier, France	1	...	0
Dr. Smith, Connecticut	1	...	0
Dr. Macdonald, Kentucky	3	...	0
Mr. Lizars, Scotland	3	...	1
Dr. Clay, Manchester	3	...	0
Mr. Walne, London	1	...	0
	12	...	1

This statement is lamentably and unpardonably incorrect as a representation of the actual state of experience in this matter. On this account we must beg our readers, in forming their conclusions respecting the operation, to substitute the following tables, of which the first displays those cases in which the diseased ovarium was actually extirpated; the second, those in which either no tumour existed, or in which insurmountable obstacles prevented the completion of the operation.

TABLE I.

	Successful cases.		Fatal cases.
Dr. Macdowell	3	...	1
Mr. Lizars	1	...	1
Dr. A. G. Smith, of Danville, Kentucky	1	...	0
Dr. Quittenbaum	1	...	0
Dr. Rogers	1	...	0
Dr. Ritter	1	...	0
Dr. Chrysmar	1	...	2
Dr. Granville	0	...	1
Dr. Clay	3	...	1
Mr. Walne	1	...	0
	13	...	6

TABLE II.

	Patients survived.		Patients died.
Dr. Macdowell . .	1	...	0
Mr. Lizars . . .	2	...	0
Dr. Granville . .	1	...	0
Prof. Dieffenbach . .	1	...	0
Dr. Martini . .	0	...	1
Anonymous writer in Froriep's Notizen	0	...	1
Dr. Clay . . .	0	...	1
	5	...	3

It appears then that, instead of the operation being successful in twelve out of thirteen cases, nine out of twenty-seven persons, or exactly one in three of those who submitted to it, died; and that five of the eighteen survivors had hazarded their lives and undergone much suffering to no purpose. But an examination, even of the successful cases, will show that the laudatory terms in which Dr. Clay speaks of this operation are by no means borne out by fact.

We have rejected L'Aumonier's case, which Dr. Clay enumerates among the instances in which the operation by the large incision was performed with success, because it has in reality no bearing whatever on the subject. With just as much propriety might he appeal to the tale told by Wierus in his book *De præstigiis*, of the sow-gelder, who, scandalized at the light life and conversation of his daughter, removed her ovaries by way of mending her morals. L'Aumonier's case is, to the best of our belief, wholly unprecedented in the annals of operative surgery; a bad pre-eminence which we trust it may long retain. The patient was a young woman, twenty-one years old, who came under M. L'Aumonier's care between six and seven weeks after delivery. She was then much emaciated. suffering from febrile symptoms, with a purulent discharge from the vagina, increased by pressure on the hypogastric region, which was tense and painful, and in which a hard tumour could be felt. The patient's strength had been further exhausted by obstinate diarrhea, and her powers of body were failing. M. L'Aumonier made an incision four inches long in a direction corresponding to the inferior edge of the *obliquus externus* muscle; and came upon a large round tumour, with which the ovary was connected. This tumour on being punctured gave issue to a pint of dark fetid pus, and it was now ascertained that the cavity in which it had collected was that of the Fallopian tube. With the cavity of the Fallopian tube the ovary communicated; an abscess having formed in its substance and burst into the tube. The ovary was not much enlarged, nor does there exist any evidence of its having been the seat of any other morbid process than inflammation terminating in suppuration. M. L'Aumonier, however, feeling "bien certain que les désordres de son organisation étoient irréparables," tore away the adhesions between the fimbriæ of the Fallopian tube and the ovary, which he removed. For the first six hours the patient was extremely exhausted and had slight convulsions; and during the whole of the next day she was scarcely able to speak. Though in a very precarious state she went on tolerably well from the third day till the sixteenth, when violent cerebral symptoms came on; ceasing, however, on the appearance of the menses, and at the date of the last report, forty-seven days after the operation, she might be considered

quite recovered.* Now all that we can say to this is, that M. L'Aumonier and his patient were very lucky, but, surely if Dr. Clay had ever read the case he would not have thought of classing it among those which form the subject of his paper.

Dr. Smith of Connecticut's† case, was not, as Dr. Clay alleges, a case of operation by the large incision, but one in which the tumour was punctured and then drawn out through a small aperture in the abdomen, as in the mode practised by Mr. Jeaffreson. It is true that the incision in this case was four inches long instead of an inch and a half, though the operation was in other respects conducted according to Mr. Jeaffreson's plan. The case, however, terminated successfully, and is therefore classed by Dr. Clay among those operated on by the large incision, though the opening into the abdomen was only four inches long, while Dr. Stilling's‡ case, in which the incision was six inches long, is referred to the category of those operated on according to Mr. Jeaffreson's method, because it terminated fatally. At least, we know not how else these inconsistencies are to be explained.

The details of Dr. Macdowell's cases are meager in the extreme,§ which is the more to be regretted since they are in many points so extraordinary. The account indeed of his first case almost staggers belief, for we are told not only that the patient had no unfavorable symptom, but that when Dr. Macdowell called on the fifth day after the operation he found her making her bed! The particulars of his third case too are very vague; it is stated that the tumour adhered to the left side, but we are not informed how the adhesions were overcome. The ligature which had been placed round the pedicle of the ovary and the Fallopian tube did not come away until the fifth week, after which the patient recovered completely. In his last successful case, operated on in 1817, the patient's life was nearly lost during the operation by the ligature which had been applied round the vessels slipping, and tremendous hemorrhage took place before another could be made secure. The patient recovered from the effects of the operation but continued in bad health.

Mr. Lizars'‖ only successful case is to our minds anything but satisfactory. One of the patient's ovaries was removed, and the other was then found to be increased to "nearly the fourth part of the one removed, and was adhering on the right side of the parietes of the pelvis and to the uterus, but comparatively free on the left side." This ovary, however, was left behind; Mr. Lizars desisting at the request of the gentlemen around him. Hemorrhage, which had nearly proved fatal, came on two hours after the operation, and for the succeeding sixty hours her life was in imminent danger. The account of the patient terminates ten weeks after the operation, at which time she was recovering from an inflammatory attack, which had rendered the employment of local depletion necessary; and was "now able again to get out of bed and to take nourishing diet,"

* Mémoires de la Société Royale de Médecine.—Année, 1782, 4to, p. 296.

† Dr. N. Smith of Yale College, Connecticut. His case was originally published in No. xvii. of the American Medical Recorder. A brief notice of it is contained in the London Medical and Physical Journal, vol. xlviii. p. 449; and the paper is found translated into Italian in vol. xxviii. of Omodeis Annali Universali.

‡ Holscher's Annalen, 1841. Heft iii. Seite 261.

§ London Medical Repository, new series, vol. iii. p. 416.

‖ Observations on the Extraction of diseased Ovaria, folio.—Edinburgh, 1825, p. 9.

but the ligature had not yet come away. Of the subsequent history of the patient we know nothing.

Dr. A. G. Smith, of Danville, in Kentucky,* operated on a negress, aged thirty, the mother of two children; in whom the first symptom of ovarian disease had appeared two years before. He made an incision from the umbilicus to within an inch of the pubes, but finding the tumour still too large to remove entire he tapped it, when several pints of fluid escaped. The pedicle, which was not larger than the breadth of the broad ligament, was now tied, and the ovary was removed. During the operation the patient made efforts to vomit, and for three days afterwards she suffered from very troublesome nausea and vomiting, which could be controlled only by very large doses of opium. No inflammatory symptom appeared; the ligature came away on the twenty-fifth day, and, with the exception of pains in the loins at the menstrual periods, she continued in good health at the time of the report three years afterwards.

We have never met with the pamphlet of Dr. Quittenbaum, entitled "*Commentatio de Ovarii hypertrophia et historia exstirpationis ovarii hydropici et hypertrophici prospero cum successu factæ,*" but it appears from the statements of Mr. Jeaffreson,† who had seen it, that the operation performed was of the kind recommended by Dr. Clay.

Dr. Rogers,‡ of New York, removed the ovary of a woman twenty years of age, who had been tapped for ascites seven times in the course of two years. After evacuating the ascitic fluid, the enlarged ovary was distinctly perceptible in the left side of the abdomen, and was removed by an incision reaching from two inches above the navel to the pubes. The cyst was firmly adherent to the peritoneum for at least three inches around the umbilicus, but by careful dissection these adhesions were divided. The wound is said to have healed in the course of twenty-eight days without a single untoward symptom having occurred. Beyond this statement no particulars of the patient's progress are given in the paper, and, though promised by Dr. Rogers, have not appeared. Twenty-eight days only had elapsed, from the performance of the operation, when he forwarded the account just quoted to the Gazette.

Dr. Ritter§ operated on a woman aged thirty-one, in whom the disease had existed about a year, and during the course of her fifth pregnancy the increase in the size of the tumour was very rapid. Sixteen weeks after delivery paracentesis abdominis was performed, and twenty-six pounds of fluid were evacuated, and a hard irregular body was then felt extending from the right iliac region to above the umbilicus. A fortnight afterwards this body, which was the enlarged ovary, weighing twelve pounds, was removed by the large incision. During the course of the operation three vessels were tied, each as large as a goose-quill, and adhesions in the epigastric region, at the umbilicus, and in the left iliac region, required to be divided. The patient was very faint during

* London Medical Repository, new series, vol. iii. p. 416, reports the case from the North American Medical and Surgical Journal for January, 1826.

† Transactions of the Provincial Medical Association, vol. v. p. 245.

‡ Medical Gazette, October, 1829.

§ Case reported by Dr. Ehrhart von Ehrenstein in Med. Jahrb. d. K. K. Oester.-Staates, Band xi., 1832, p. 256.

the operation; on the next day she had frequent syncope, shivering, and laborious respiration. On the third day violent febrile symptoms came on, and the patient's life was in imminent danger, until the eighth day, when a discharge of bloody serum from the wound occurred, and was attended with great relief to her symptoms. Her subsequent improvement was slow, but at the end of the ninth week the wound had closed, and her recovery might be regarded as complete.

Dr. Chrysmar,* of Wangen, removed the ovary of a lady thirty-eight years old, who was the mother of seven children. The first signs of the disease had appeared in her seven years before, and for the last three years the increase of the growth had been attended with considerable impairment of her health. The tumour, weighing eight pounds and a half, was removed by an incision extending from the sternum to the pubes. The operation was followed after the lapse of a few hours by slight shivering and hiccup, but no severe febrile symptom occurred at any time. At the end of six weeks she was quite well, and continued so at the date of the report eight years afterwards, having in the interval given birth to another living child.

All must, we think, agree with us in the opinion that the mere announcement that these thirteen persons recovered from the operation would by no means suffice to enable us to estimate it aright. The sufferings endured during its performance, the pains of a protracted convalescence, and the imminent danger in which life was placed in some instances, ought all to be taken into account. The sufferings and the danger too, in these cases, were neither few nor small. Twice (in Mr. Lizars' patient and in one of those operated on by Dr. Macdowell) the patient was exposed to great danger from hemorrhage. Dr. Ritter's patient nearly sank from the shock of the operation, and the violent fever which ensued well-nigh cost her her life. In Dr. Clay's first case, in Dr. A. G. Smith's case, and in that recorded by Mr. Walne, the symptoms were at one time of a very serious nature, though not such as to betoken immediate danger. We are not furnished with details of Dr. Clay's third successful case for the first three days after the operation, and unfortunately we do not know the history of the person whose case is related by Dr. Quittenbaum. In five cases (two of Dr. Macdowell's, one of Dr. Clay's, Dr. Rogers's, and Dr. Chrysmar's) the patients went on favorably from the very beginning; but in Macdowell's and Rogers's cases there is a most blameworthy deficiency of information on all points relating to the patient's progress. In six of the thirteen cases then, there is evidence of the life of the patient having been placed in some degree of jeopardy, while in three only of those in which the patients are alleged to have recovered without any bad symptom, are we furnished with such details as we have a right to require. Moreover, Dr. Macdowell's first case, that of Dr. Chrysmar and of Dr. A. G. Smith are the only ones in which a length of time had passed since the operation, sufficient to test the permanence of the cure or to show that the other ovary did not become the subject of disease. In Mr. Lizars' patient it is expressly stated that at the time of the operation both ovaries were diseased though only one was extirpated, and we have Dieffenbach's authority for the opinion that this would be the case

* Case reported by Dr. Hopfer, in Graefe und Walther's Journal, Band xii. Seite 60.

in many instances. The appearance of the solid part of the ovary in the two successful cases by Dr. Clay, in which he has described the tumour, would lead to the apprehension that the disease in both instances partook of a malignant character, and that consequently no lasting benefit will result from the operation.

There are objections, however, to the operations far more conclusive than any which can be deduced from the inadequate nature of the testimony in its favour. Not only did six of the persons, whose ovaries were extirpated, die from the effects of the operation, but in eight instances, after the abdominal cavity had been laid open, the removal of the tumour was found impracticable, and the lives of three of these patients were sacrificed to the fruitless and ill-judged interference of the surgeon.

We will now proceed to an analysis of these melancholy cases, tedious though it may be to our readers, for we feel it to be a sacred duty when erroneous statements have been made on a subject so deeply interesting, to expose their fallacy at any cost of time or labour.

We will first examine those six cases in which the extirpation of the ovary was effected at the expense of the patient's life; and the fearful character of the operation will be obvious from the fact that these six cases give an average duration of life after its performance of only forty-five hours.

In Dr. Macdowell's fifth case, operated on in the year 1818, the tumour adhered by long and slender attachments to several of the adjacent parts, and on being divided several of them bled, and required to be secured by ligatures. Violent peritonitis came on, and the patient died on the third day after the operation, having suffered from severe pain in the abdomen and obstinate vomiting. The peritoneum was found violently and extensively inflamed.

In Professor Lizars' third case, the tumour adhered so strongly to the parietes of the abdomen, to the colon, and to the brim of the pelvis, that the operator despaired of being able to detach it, but at length succeeded by dissecting and occasionally tearing the adhesions with caution. From the moment when the operation was completed the patient's symptoms were alarming in character and grew more serious until her death, which took place fifty-three hours afterwards. Of this patient Dr. Clay observes that she "had previously suffered from an attack of cholera, which, with equal probability with the operation, might be considered as the cause of death." We hardly know how to designate this assertion of Dr. Clay's. The patient, indeed, is stated by Mr. Lizars to have been very dangerously ill from cholera when he first saw her, on the 13th of March, but a few days after he found her "at the fireside, cheerful and having more strength than any one could have anticipated;" and the operation was not performed until March 22, at which time there do not appear to have been any remains of her former indisposition. To remove all doubt from the minds of our readers, however, we will transcribe the account given by Mr. Lizars of the appearances found on examination of the body, all allusion to which Dr. Clay suppresses:

"On cutting the stitches of the wound," says Mr. Lizars, "it was found adhering in some points, particularly at the upper or sternal part; at the lower part a little serum and purulent matter issued out; and on reflecting back the parietes of the abdomen, the omentum and intestines were found adhering to the neigh-

bourhood of the wound. The peritoneum investing the parietes, which adhered to the tumour, and also those portions of this membrane investing the colon and small intestines which adhered to the tumour, were of a blueish black appearance, and tore with ease under the fingers, being evidently gangrenous. Small patches of inflammation were observable in other parts of the intestines, particularly the colon. In the pelvic cavity there were a few ounces of serous fluid, with flakes of coagulable lymph floating in it. The pedicle was then seen to be the broad ligament of the uterus, and the uterus itself was found perfectly healthy; the other ovarium was of its natural size, had a coating of coagulable lymph, but was tolerably firm when bisected; the fallopian tube was turgid and red in colour."

We would hope for Dr. Clay's sake that he had not been at the trouble of reading the account of the post-mortem examination when he made the statement we have quoted.

Dr. Chrysmar's first case was that of a person aged forty-seven, in whom disease of the ovaries had existed ever since the cessation of her menses four years previously. Her abdomen had for a year attained as large a size as that of a woman in the ninth month of pregnancy, and her health had become very greatly impaired. Ascites was known to coexist with the ovarian disease, but paracentesis had never been performed. On opening the abdomen, five quarts of greenish-yellow fluid escaped. The tumour was closely adherent to the transverse colon and to the great arch of the stomach, and when removed was ascertained to weigh $7\frac{1}{3}$lbs. Its removal was followed by great anxiety and frequent vomiting which nothing could quell; the powers of the patient sank hourly, and she died thirty-six hours after the operation. On the post-mortem examination peritonitis was found, with effusion of lymph and pus into the abdomen, and adhesion of the intestines to each other. The peritoneum in the whole neighbourhood of the ovary was gangrenous, and gangrenous patches were found on the colon and stomach at the places where the adhesions had existed.

The patient in Dr. Chrysmar's third case was thirty-eight years old. Her health had been habitually indifferent, and for eight years she had suffered from anasarca with ascites and enlarged liver, and for at least as long a period there had existed signs of enlargement of the left ovary. Paracentesis of the abdomen had been performed once when eight quarts of fluid were let out, but without any amendment of her condition. On opening the abdomen three quarts of yellowish fluid were let out, and the tumour was then found to be adherent only towards the promontory of the sacrum, and its pedicle was only four inches wide. Its removal, however, was followed almost immediately by syncope, which returned frequently, and the patient died in thirty-six hours after the performance of the operation. A post-mortem examination disclosed traces of peritonitis with purulent effusion into the abdominal cavity, and gangrenous patches on the peritoneum.

Subsequently to the publication of Dr. Clay's pamphlet, Dr. Granville sent a brief notice to the Medical Gazette (Jan. 13, 1843,) of a case in which he removed a diseased ovarium, but in which the patient died three days afterwards. Dr. Granville asserts, but furnishes no kind of proof, that her death was caused by her having been over depleted by her medical attendant. Be this as it may, however, it will probably be allowed that it is but a fair inference to assume that symptoms of peritonitis existed, sufficiently severe to render the employment of some degree of depletion necessary.

Lastly, we may mention Dr. Clay's fifth case, in which the patient all but died on the operating table, and will now pass to those instances in which the operation was from various causes left incomplete.

Shortly after his first successful operation, Dr. Macdowell* attempted it a second time, but found the ovary so firmly connected with the bladder and the fundus of the uterus, that its extirpation was impossible. He therefore merely made an incision into the ovary, from which a considerable quantity of gelatinous fluid and blood escaped, and closed the wound, but not until more than a quart of blood had escaped into the abdominal cavity. This was removed before the wound was closed, the patient recovered and returned to her usual occupations. No information is given as to the symptoms which attended the patients convalescence, but we learn that at the end of six years the growth had regained its former size.

In the first case on which Mr. Lizars operated, no tumour existed; the life of the patient was jeoparded to no purpose, and for some days afterwards the symptoms were extremely alarming; yet because life was not actually lost by the operation, Dr. Clay appeals to it as an instance of its successful performance.

In Mr. Lizars' fourth case, the blood-vessels of the omentum were found to be enormously enlarged, and running on the surface and into the substance of the tumour, which accordingly was thought most prudent not to meddle with. Being anxious, however, to reduce the bulk of the tumour, Mr. Lizars punctured it at the lower part, but only pure blood flowed.

We again refer to Mr. Lizars' own account of the symptoms which followed the operation. The patient's sufferings were severe, but she appears to have escaped with her life, though her history is not carried beyond a fortnight after the operation, at which time she had not left her bed, and the wound though healing was not healed.

This too is one of Dr. Clay's successful cases, and not a hint is given by him that in this or in any other instance, (except in that of Mrs. Dillon operated upon by himself,) the attempt to remove a diseased ovarium has failed from the extent of adhesions. And yet Dr. Clay has read Mr. Lizars' book, and makes quotations from it when they serve his purpose! On such want of good faith we forbear to comment.

Dr. Granville† attempted the operation in the year 1826, but desisted after having laid bare the tumour, on account of the number and extent of the adhesions. The patient recovered rapidly from the effects of the operation. One point mentioned in the details of the case is important as illustrating a grand objection to the operation. It is stated that "Dr. Granville passed his hand into the abdominal cavity, and ascertained that although the tumours felt loose when examined externally, they were in fact adhering in various directions by strong bands."

Professor Dieffenbach‡ attempted the removal of an ovarian tumour in the case of a Polish lady, forty years of age, in whom the disease had been the slow growth of ten or twelve years. Considerable difference of opinion as to its nature existed among the medical practitioners who saw

* This case is one enumerated as successful by Dr. Clay!

† London Medical and Physical Journal, vol. lvi. p. 141.

‡ Rust's Magazin. Band xxv. Seite 339. (Jahrgang 1828.)

the lady, but the extreme mobility of the tumour and the excellent health of the patient induced Professor Dieffenbach to attempt the operation. It was found, however, after laying open the abdominal cavity that the tumour was connected with the vertebral column, that its pedicle was very broad, and contained vessels which pulsated with great force. A puncture made into the tumour was followed by hemorrhage which it required compression to arrest. The extirpation of the growth being deemed hazardous, the wound was closed and a bandage applied. Violent pain, hiccough, and vomiting, all the most aggravated symptoms in short of a strangulation of an intestine came on immediately; the abdomen became tense and tumid, and the size of the tumour was evidently increased. These symptoms yielded after the employment for some days of a strict antiphlogistic treatment, but were followed by great debility, accompanied with a yellow tinge of the skin and a rapid and hurried pulse. By degrees, however, as the discharge from the wound diminished, these symptoms subsided and the patient ultimately recovered her health.

In three instances, one of which is the first fatal case related by Dr. Clay, the attempt to remove the tumour was not merely unsuccessful, but occasioned the death of the patient. A second case occurred to Dr. Martini of Lübeck, (Rust's Magazin, xv. Band, Seite 436,) who attempted the removal of a diseased ovary from a person twenty-four years old, in whom the first signs of disease had appeared fifteen months previously, three months after she had given birth to a dead child. Paracentesis was performed four times, and after the fourth puncture, an attempt was made to excite adhesions between the walls of the cyst, by injecting an irritating fluid into its cavity, but without success. The attempt was repeated after a short time, but on making a puncture with this view, no fluid escaped, and it was inferred that the contents of the tumour had become solid. Dr. Martini now resolved to attempt its extirpation, but after opening the abdominal cavity he ascertained that the sac which formed the upper part of the tumour was connected inferiorly with a firm growth of a cartilaginous hardness which was so firmly adherent to the bladder, rectum, and walls of the pelvis, that it could not be detached. The sac when punctured gave exit to three pints of fluid, and Dr. Martini was forced to content himself with cutting off the sac from the solid tumour with which it was connected. The patient fainted during the operation; on the following day, however, she had rallied, but on the morning of the third day, her abdomen became distended, she began to sink, hiccup came on, she fell into a state of syncope and died fifty-six hours after the operation. There was a considerable quantity of bloody fluid in the abdomen, and a great exudation of lymph immediately under the incision through the abdominal parietes; but the intestines generally were pallid. The hard tumour in the pelvis which was formed by the left ovary, had become soft since the performance of the operation, and now contained cavities full of pus, and a steatomatous matter.

An anonymous writer* details the particulars of a case in which an attempt was made to remove the ovary of a woman, forty-eight years old, in whom the first symptoms of disease had appeared a year before.

* Froriep's Notizen. Band xiv. Numero 300. Seite 215.

After the tumour had existed six months paracentesis was performed, and in the course of the ensuing half year was repeated five times. On attempting the extirpation of the cyst it was found not to be seated on a narrow pedicle, but to have a broad base, and to be firmly connected with the os innominatum. Nothing more therefore could be done than to remove the upper two thirds of the sac, the walls of which were from two thirds of an inch to an inch in thickness, and the fluid it contained was extremely thick and gelatinous. The subsequent history of the patient is detailed very briefly, but it is stated that fever and tetanic symptoms came on, of which the patient died on the sixth day after the operation. The peritoneum and intestines generally were uninflamed, but the omentum was inflamed, and the remains of the sac formed a cavity which was filled with purulent matter.

It was our intention to have examined the comparative merits of other operative proceedings which have been at different times adopted for the cure of ovarian dropsy, but we have already exceeded our limits. The erroneous statements, however, by which Dr. Clay endeavours to bring the operation practised by Mr. Jeaffreson into disrepute are so remarkable that we cannot pass them by unnoticed. Not content with omitting all mention of the successful cases recorded by Mr. Gorham in the Lancet for October 1839, he asserts that Dr. Dohlhoff operated on three persons by the small incision, and that of these three two died, and in the third who recovered no tumour was found. Now we deny that in any of these three cases the operation was at all similar to that practised by Mr. Jeaffreson.* In the first patient the incision was indeed originally only two inches long, and the tumour was punctured by a trocar. Its contents were found too viscid to flow through the canula; the incision was then very *considerably enlarged*, and the tumour laid open sufficiently for the operator to bale out its contents with a cup. After its bulk had been thus reduced, the tumour was removed. This patient died sixteen hours after the operation, and peritonitis with effusion were found on a post-mortem examination.

The incision in the second case was sufficiently large for the bystanders to perceive that the vessels of the omentum were morbidly dilated, that small white tumours of the size of a bean covered the surface both of the omentum and the peritoneum lining the abdominal walls. It likewise allowed them to see that the tumour had very intimate adhesions to deep-seated organs, on which account the attempt to extirpate it was discontinued, but eight hours afterwards the patient died.

In the third case we are informed that after the abdominal cavity had been laid open, and the hand had been introduced into it in search of a tumour, none could be found; [nach eröffneter Unterleibshöhle und nach dem Eingehen mit der Hand in dieselbe fand sich keine Spur einer Geschwulstvor.] This person continued in a dangerous condition for some days, but ultimately recovered.

Now, on what principle these cases are classed among those operated on in the method practised by Mr. Jeaffreson we are perfectly at a loss

* See the particulars of these cases in Rust's Magazin, Bd. li. Jahrg. 1838, S. 77; or in Kleinert's Repertorium, 1838, Heft vi.

to conceive; for to our minds they far more closely resemble cases operated on by the large incision.

It is not, however, our object to advocate the operation by the small incision, for our estimate of any such proceeding coincides exactly with that expressed by the late Dr. Hamilton, in his Practical Observations on Midwifery. We have merely sought to exhibit the real amount of danger which attends *one* of the operations that have been practised for the cure of ovarian dropsy, *one* which its advocate describes as yielding results far more favorable than those of amputation. Our readers may judge for themselves how far this statement is consonant with truth—to our thinking the facts need no comment. We earnestly hope that they will prevent the younger members of the profession from being dazzled by the *alleged* success of an operation, which though it may excite the astonishment of the vulgar, calls neither for the knowledge of the anatomist nor the skill of the surgeon. In some continental universities the candidates for the doctor's degree takes an oath "Nullius unquam hominis vitam ancipiti tentaturum experimento;" a fundamental principle of medical morality which we conceive is outraged whenever an operation so fearful in its nature, often so immediately fatal in its results, as gastrotomy, is performed for the removal of a disease, of the very existence of which the surgeon is not always sure; of the curability of which, by his interference, he must be in the highest degree uncertain. There are unfortunately no means of gaining notoriety in the practice of medicine so speedily as by the performance of some bold operation, which the aged and the timorous are reluctant to attempt, and we can fully sympathise with those who, just setting out in their medical career, are unwilling to let slip any opportunity of advancing their reputation. We would, however, entreat all such to ponder well before they jeopardy the lives of their patients by operations such as that the claims of which we have been investigating, and we would say to each one, in the words of Hufeland "Thine is a high and holy office, see that thou exercise it purely, not for thine own advancement, not for thine own honour, but for the glory of God, and the good of thy neighbour. Hereafter thou wilt have to give an account of it."*

* Since the preceding pages were written, Mr. Walne has performed a second successful operation; the announcement of which appears in the Medical Gazette for July 7. He has favoured us with the following particulars of the case: "The patient experienced an attack of inflammation of the iliac vein, producing enlargement of the inferior extremity; in short a complete phlegmasia dolens of that side on which the tumour was attached to the uterus, and of course the pedicle tied in the operation. Yet she has recovered, except being still feeble from this attack."

Art. XI.

J. B. van Helmont's System der Medicin, verglichen mit den bedeutenderen Systemen älterer und neuerer Zeit; ein Beitrag zur Entwickelungs-geschichte medicinischen Theorien; nebst der Skizze einer Theorie der Lebenserscheinungen. Von Dr. G. A. Spiess.—*Frankfurt am Main*, 1840. 8vo, pp. 520.

J. B. van Helmont's System of Medicine compared with the more remarkable Systems of ancient and modern Time; a contribution to the history of the development of medical theories; together with a sketch of a theory of the phenomena of life. By Dr. G. A. Spiess, Practising Physician.—*Frankfort on the Main*, 1840.

We lately gave some account of the works of Paracelsus according to the estimate formed of them by Professor Marx, of Göttingen. By a happy coincidence an occasion is now afforded for a consideration of those of Van Helmont, who in the history of the reformation of medicine from the errors of the Greek and Arabian schools, stands next to Paracelsus in time, and equal or superior to him in dignity and learning. The title of this work so fully tells its scope, that we may proceed at once to the analysis of its contents, and, following the order of the learned author, we shall review, first, the general features of Van Helmont's life; secondly, his doctrines; thirdly, their relation to the views of his predecessors; and, fourthly, their relation to those of succeeding ages and of the present time.

i. Johannes Baptista Van Helmont, Lord of Merode, Royenborch, Oerschot, and Pellines, was born in 1578, the scion of a noble Netherlands family. He was the youngest child of his father, who died in 1580; but he received, through the affection of his mother, a careful education, and in his seventeenth year had completed the course of philosophic instruction in the university of Louvain, and already called himself "helluo librorum." Many, perhaps, surpassed him in real knowledge, but few had equalled him in industry. He had studied earnestly mathematics, algebra, and astronomy; but when he learnt from Copernicus that the heavens turned in a manner altogether different from that which, in much labour, he had before been taught, astronomy fell at once into contempt with him, and he held it for an empty pretence of science. Such were already, too, his scepticism and diffidence, that he would not take his master's degree because he felt that he did not yet deserve even to be called a scholar; and he left the schools as places where the truth he earnestly longed after had never dwelt.

A rich canonry was offered him if he would devote himself to theology, but he dared not, as St. Bernard said, "live by the sins of men." The Jesuits for a time enslaved him by their philosophy, but at last, in all they taught, he could find but "empty straw." He studied Seneca and Epictetus; and, for a time, he suspected that the very sap of truth must be in ethics. He thought the Capucin monks were the true Christian stoics, and though, for his weak health's sake, he dared not enter their order, yet he was convinced that if he could but adopt, in their Christian completeness, all the principles of the stoical philosophy, he should reach both to the knowledge and the love of pure truth. The notion lasted till

he had a strange dream. He seemed to be transformed into a huge empty bubble reaching from earth to heaven; over him hung a sword, below was an abyss of darkness; and he felt how the philosophy had made him like a great inflated being floating over the abyss of hell. Terrified at his having dared to attempt all things by his own will and power, he left the stoical philosophy as that which might do well enough for heathens, but to Christians must be hateful blasphemy. He felt that God alone could give wisdom, and that prayer was a better instrument for its attainment than self-willed study.

In his uncertainty what course of life he should pursue, Van Helmont turned now to the study of laws and government; but he soon found that what was called right depended only on the varying opinions of men, and he thought it would be mere loss of time to learn what this or that man had happened to command. At length, wearied with his vain search for truth and happiness, he took up Matthiolus and Dioscorides, thinking that nothing could be better for man than to admire God's goodness in the herbs, and to employ them all in their right uses. But it was presently clear to him that, since the time of Dioscorides, the study had not essentially advanced, and that, at the best, the therapeutic herbal was a very confused unsatisfying book.

This study, however, led Van Helmont into that of medicine, and he wondered whether there were a book in which he could learn its principles and rules; for he supposed that medicine, being a science and a good gift from heaven, would have its certain axioms. So he read the Institutes of Fuchsius and Fernelius, and he could not help laughing with himself at their poverty and weakness. Then he read through Galen twice and Hippocrates once; he read the whole of Avicenna and the other Greek and Arabian authors—in all, near six hundred books. With great industry, too, he set down all that seemed important; and, at the last, when he read through his notes, he found only reason to grieve that he had thrown away such mighty labour, for the poor ill-founded principles which he first read and laughed at contained nearly all that he had learnt.

He now determined to join a practising physician and see patients with him; but here, again, he found nothing but uncertainty; all the diseases which did not cease of themselves seemed to be classed among the incurable. Disgusted with medicine, and with himself for having, in spite of his noble origin and against the wish of his mother, consented to study it, Van Helmont gave his inheritances to a widowed sister, and his books to poor students, and determined to leave his country for ever, and to roam in foreign lands in the hope that God would yet guide his course for good.

But the more he fled from medicine, and the more obstinately he regarded it as mere trickery, the more occasion did he find for practising it, and the more marvellous were the cures he effected. In his travels, too, he met with a plain man who knew little more than the manipulations of pyrotechny, but who taught him so much chemistry that in two years he had made a whole store of mineral medicines, and could employ them all with marvellous success. This seems to have revived a hope that something might yet be done in medicine, and his essays encouraged him; for, on his way, many great persons asked his aid in their diseases, and

they recovered in his hands; many times too he found pestilences raging in the towns he visited, and, moved with compassion for the poor wretches whose neighbours and physicians had alike deserted them, he frequented all their haunts, and found that while he was studying diseases his mere presence comforted and sometimes cured the sick. At length after ten years of study and of travel, in which he passed some time in England, he returned in 1609 to Vilvorden, near Brussels, where he determined to seclude himself for prayer and for the study of nature, and especially of pyrotechny. Here too he read the works of Paracelsus, which, though he at last discerned their many errors, made a deep impression on his mind. And hither, though he would have avoided it, numbers came from far and near for his advice.

The last thirty years of Van Helmont's life were thus spent in a retirement, in which his study and his meditations were interrupted only by his attendance upon those who needed his charity, or whom his fame had attracted to seek his aid. During this period too, he wrote nearly all his works, and they were published by his son after his death. He died in 1644, of some chest-affection. On his last day he wrote standing, for he could not lie down for fear of suffocation, to a friend in Paris, "praise and honour be to God for ever, whom it has pleased to take me from this world. As I think, my life will not continue more than four and twenty hours, for I have just suffered the first attack of fever from weakness and want of vital action, so I must conclude;" and his prophecy proved true.

This brief account of Van Helmont, in which his own language is for the most part imitated, may convey some notion of his character. Dr. Spiess, who must however be regarded rather as a panegyrist than as an impartial biographer, points out as its most striking features, acuteness of judgment enabling him to detect the most hidden weakness of the medical systems of his time, deep-felt sense of religion producing a genuine philanthropy, independence, and self-confidence. And these qualities Van Helmont unquestionably possessed; they are assigned to him even in the matter-of-fact estimates of Haller, and they will be more obvious in all that is said of his doctrines. But they were not unalloyed; vanity and personal ambition are as distinct in all his writings as these better qualities are. His sharp discernment of errors was rarely employed in testing the statements on which he supported his own system; in all that favoured his own views he was as credulous as any of his predecessors. Witness, for example, his belief in all the imaginary facts of natural magic, even to the tale of the transplanted nose falling off on the death of its original proprietor, his satisfaction with the results of his own medical practice, and his discontent with that of others. And again, if his religion produced philanthropy, so also it often degenerated into mere superstition; a belief in all the pretended miracles of the Romish church, a reliance upon dreams and the visions of a disordered fancy, an acquiescence in many of the absurdest mysticisms of the Neo-Platonists.

We proceed to an analysis of his systems of natural science and of medicine, for the two are so closely interwoven that they cannot separately be made intelligible. And here also we shall write in something like Van Helmont's style, displaying the system as in an uninterrupted

discourse, and only asking the reader to bear in mind that the laws of gravitation, electricity, and chemical affinity, and nearly all the facts of organic physiology were unknown when the original was written.

II. Van Helmont's system of philosophy and medicine.

A. General considerations. The prime cause of all natural phenomena is not, as the Aristotelians and nearly all men maintain, "a principle of motion and rest in bodies, in which it exists *per se* and not *per accidens*;" but nature is the command of God, by which each thing is what it is, and does that which it is commanded to do. Everything in nature has its origin from the direct union of matter, (the substratum of all natural things) and a *causa efficiens*, which is also named *causa seminalis*, *principium dirigens*, *faber*, *Vulcanus*, or in the widest sense, *Archæus;* and into this Archæus, God has infused the knowledge of its own ends and habitudes. But the matter and the efficient cause must not be regarded as separate; they are in every natural being most intimately connected, and they determine its existence; for with their union a determinate *form*, or mode and capacity of existence,* is always given, which depends on the efficient cause.

Whenever, by the union of matter and an indwelling power, or internal efficient cause, a determinate form is given, there is also *life;* and thus all nature is enlivened, for in every natural being there is matter with a determinate plan of existence. Moreover, all life may be designated *light*, and in so far as it appears in determinate form, as formal or form-giving light (*lux formalis;*) and further than this, we cannot apprehend the intimate essence of life, "for life in the abstract is the very God incomprehensible."

Diversities in the relative proportions of life or light, and of matter, in different beings do not alone constitute their differences. It is true that "certain forms shine, as in stones and minerals; and certain are resplendent with augmented light, as in plants; while others are even luminous, as in animals;" but "there are also as many *kinds* of vital lights as of living creatures, inasmuch as these lights are the very lives, souls, and forms of living things." So that, though the differences of their respective lives constitute all the differences among natural things, yet those differences may be both quantitative and qualitative.

With regard to the *origin of things natural*, we must believe that there are but three elements; for "in the beginning God created the heavens and the earth;" and the earth was formed from water. The heavens, moreover, consist of air and water; therefore these two are the primary elements, and earth a secondary one. Water is the material substratum of all things; air, *æther*, or *aura vitalis*, indicates the vivifying principle.

Thus was matter formed, as water. But it had to be vivified by the union of an efficient cause with it. And the efficient or seminal causes were, and still are, scattered everywhere in profusion by the Creator, as *ferments*.† They are infinite in number and in kind;

* This meaning of *form* must be borne in mind, for the term will be often again used in the same sense; *shape* will imply the ordinary meaning of form.

† The term was not used by Van Helmont in its present meaning, as a substance to excite fermentation, but rather in the figurative sense in which we still sometimes use it; or, in general, as a *dynamic principle.*

there were and are as many of them as there are vital actions in the natural world; and they are all incessantly active, ever exercising their power upon the matter around them, and producing, as they did at first, minerals, and the seeds of even those plants and animals which are also capable of a more special propagation. Moreover, while the efficient cause of every living thing is transitory, ceasing to be with that which it enlivens, these special ferments are permanent, and can never cease to work, each in its peculiar way, upon common matter.

However, the new creature, whether its seed is formed by the action of a ferment on common matter, or by proper generation, is not perfected without the especial act of God.

"Animal does not generate animal, but the seed in order to the animal. And the seed is to the form of the animal as a disposing architect, but not as the maker of the form. The archæus is borrowed from the parent, but not the form, nor the light of life by which the form shines. (Van Helmont, quoted p. 10.)

"In every seed, indeed, there is a proper power, a special archæus, on which the type of its kind is impressed from the parent organism, (or is received from the special ferment;) and which has, and is capable of, all that is required for the formation of the new creature up to a certain point; but the essential form of the creature, which is one with its life, and in the higher beings is one with the perceptive mind (*anima sensitiva,*) the archæus cannot of its own power acquire. This is breathed into each individual being as the breath of life from God, as soon as by the action of the archæus in its seed it has attained a certain stage of development. And now first it possesses a form of life of its own; for hitherto it had been, in a measure, a part of another being, and had only a borrowed life. This is true not of plants and animals alone, which exhibit so distinct and determinate expressions of life, but also of the lowest creatures and even of the minerals; for though these do not propagate by seeds, yet they possess a determinate form of life, an *aura vitalis,* an archæus; for, of necessity, whatsoever is generated must have a disposer within it." (Spiess's Paraphrase, p. 11.)

Thus are all the individuals in nature generated. We come now to their *maintenance.* The inspiration of the determinate form of life, in the manner just described, is, in the case of the higher organisms, associated with an important change in the archæus of the new being:

"Hitherto the archæus was, to a certain extent, under the government of the parent life, whose product it was; but now it enters into the service of the new form of life, which, though it be in kind the same as that of the parent, is yet another. The archæus is now no longer a *causa efficiens,* acting of its own power and after its own type (though an impressed type;) but it becomes luminous, that is, it immediately illuminates, it becomes the servant of life, the sole and immediate witness, executor, and organ, as well as the lodging, of the life. Before, it appeared to us as power, as a dynamic principle, *in opposition to* matter; now it appears, in the higher organized and really animated beings, as *belonging to* the body or matter, and life enters in as a higher dynamic principle, determining the proper form over and above the archæus. The archæus is indeed still power, but it is the power inseparably connected with and constantly acting in the matter, and governed and directed by a higher and quite incorporeal essence, the life which is identical with the mind (*anima;*) in short, it is the corporeal servant and mediator of the life, and that by which all the exercises of life are executed." (Spiess's Paraphrase, pp. 14-5.)

Now, if these things be true, an archæus must dwell in every part; and every particle of the body of the more composite creatures must have its own archæus or power, working according to a model or to determined laws. And all these *archæi insiti* must be subject to the

one *archæus influus*, which in its turn is subordinate to the life or the soul.

"Thus, in development, the archæus perambulates all the dark places and recesses of his seed, and begins to transform the matter after the fashion of his model. Here he places the heart, and there marks out the brain; and in each place of his own universal monarchy he appoints a permanent residing president, according to the need and the destinies of the parts. And that president remains curator and internal pilot (of the part) even to death; while the other, the chief archæus, lucid and never idle, moving about and assigned to no one member, keeps watch over the several pilots of the members." (Van Helmont, quoted p. 15.)

Throughout life the archæi are the proper agents of the vital processes. With regard to nutrition the old notion is erroneous, that out of the nutriment that which is appropriate is taken by each part, and that which is unfit is rejected; there is in truth a continual transmutation of the simplest matters by the power of the archæus in each part. As in the seed, the archæus forms all the organs from the one simple substance; so in the nutrition of each part, its archæus, according to its type, changes and fashions the common material, sap or blood, and assimilates it. Moreover, as every substance has its own archæus, it follows that it is not mere matter which is appropriated, but matter and an archæus; in other words, not inactive but active matter, or matter with peculiar properties and forces. And in this appropriation, either the archæus of the appropriating body, being the more powerful, subdues that of the appropriated; or, being the weaker, it is itself subdued. But in neither case does the weaker archæus lose all its proper tendencies; it still exercises an influence on the stronger, and in some measure alters its condition by the retention of its own properties. Hence are explicable the action of medicines, whose archæi are not annulled in the recipient, but retain some of their force and influence the stronger archæus—the influence of various soils on the same plant—the effects of the causes of many diseases, &c.

As to the *corruption* and *passing away* of beings, corruption is no mere negation or privation, as Aristotle thought; it is not a cessation, but a change. Neither does this corruption or change affect the form of any being, but only the matter, which passes into another form of life.

"Forms are not ever corrupted; though, indeed, they cease to be (*intereunt.*) The human spirit alone passes away, safe and whole; all other forms perish. But matter never passes away, nor ceases to be; it is corrupted. And so corruption is of matter alone.......Corruption therefore is a certain disposition of matter deserted by its weary pilot, Vulcan (its archæus.) For so long as a body sails with its pilot in good plight, it listens not to alien ferments." (Van Helmont, quoted p. 21.)

Not that the archæus gives way of itself, but only that it falls under the attack of some other ferment; and then corruption, beginning with a substitution of one ferment for another, goes on till it has attained its end.

B. Special physiology. These general considerations imply and guide to many doctrines in special physiology different from those which the ancients have held.

Natural functions. In regard to these the ancients held that *diges-*

tion, in which is included everything from gastric digestion to nutrition and excretion, consists in three stages, viz. a first, or *concoctio ventriculi*, taking place in the whole digestive canal, and having for its product the chyle, for its excrement the fæces; a second, or *concoctio in jecore*, taking place in the liver, its product being the blood, its excrement the urine (which they did not distinguish from the serum,) and the yellow and black bile; and a third digestion, or *concoctio in partibus singulis*, of which the organ is the whole body, its product the four secondary humours, four degrees of assimilation of the blood, and its excrement the sweat.

Now, in the first place, heat is not, as the ancients maintained, the prime cause of the first digestion; it is an excitant, but not an efficient, of digestion. The true author of digestion is "a certain vital faculty, a ferment it may be called, which truly and *formally* transmutes the food." Instead of three there must be reckoned six stages of digestion, each of which has its proper organ, in which the change is effected by a proper ferment or indwelling vital force acting in it, not in a material, but in a formal dynamic manner. Each ferment is indeed united to a certain matter, as the *fermentum stomachi* to the *acor* of the stomach; but it is the ferment, not the matter, which is the real agent in the process.

The first digestion is the gastric, in which the gastric ferment converts all the food into chyme.

The second digestion is in the duodenum, in which the ferment of the bile changes the chyme from *sal acidum* to *sal salsum.* The bile is not, as hitherto supposed, mere excrement; but "a noble and vital viscus," the bearer of a vital ferment. In this stage also the watery part of the food is separated, and, with various salts, passes to the kidney as urine; and the indigestible parts become fæces.

The third digestion begins in the mesenteric veins, and is especially effected in the veins of the liver, whose ferment changes the chyle into *cruor.* This is the only office of the liver, and the old notions of the formation of yellow and black bile and urine in it are absurd. Neither is the liver nourished, as has been supposed, from the cruor, nor the stomach from the chyme, nor each organ from any substance carried to it, as Fernelius imagined; but the food must have passed through every stage of digestion before it is fit for the nutrition of any part.

The fourth digestion is in the heart, whose ferment makes the thick dark blood of the vena cava brighter and more volatile; changing *cruor* to *sanguis.*

The fifth digestion transmutes the arterial blood into the vital spirit of the archæus; and in neither of these two is any excrement produced.

The sixth digestion is nutrition proper. "It is perfected in the several kitchens of the members; and there are (for it) as many stomachs as there are fed members. In it the spirit indwelling in each part digests for itself its own aliment." There are not four stages only to assimilation; there may be hundreds before the blood is changed into a solid part.

It is to be noticed that the serum (*latex* or *latex humor*) is not, as is commonly supposed, either mere urine or sweat, or any other mere excrement, but a proper product of the formation of blood; and it serves

to dilute and moderate the cruor, to dissolve the salts received for excretion, as well as the nutritive material, and to contribute to the formation of all the watery excretions, as well as of the sweat in which it ultimately passes off; each organ, by its proper ferment, forms from the serum its peculiar excretion. Moreover, by its unhealthy changes, or by injurious materials added to it, the serum is a frequent excitant of disease.

Of the vital functions, (circulation and respiration.) There is no *spiritus naturalis* or *spiritus vitalis*, such as the ancients have imagined to be separate existences formed in and moving with the blood; nor any *spiritus animalis*, such as they imagined to be sublimed in the brain. The cruor generated in the liver possesses itself the properties hitherto ascribed to the *spiritus naturalis* supposed to be formed with it. And when the *cruor* is, by the ferment of the heart, converted into *sanguis*, and is in a measure enlightened by the soul, it becomes itself the *spiritus vitalis*, the carrier of life to all the parts of the body; not however, by its material composition, but by its being enlivened and inspirited by the soul. And as to the *spiritus animalis*, its supposed offices, sensation, motion, and the higher actions of the mind are due to the one *spiritus vitalis*, which is capable of assuming the character of each of the parts to which it is sent, and of presiding over their several functions. That which goes to the tongue exercises taste; but the same spirit in the fingers does not taste, but feels.

Again there is no *calor innatus* in the heart, such as the ancients supposed to change the natural spirit or vapour of the blood into the vital spirit; nor, consequently, do the pulse and respiration serve for the cooling of this heated spirit. The fuliginous excrement of the spirit also, supposed to be sent from the right heart to the lungs and discharged in expiration, as well as from the skin at each contraction of the arteries, is a mere fable; and so is the nutriment of the spirit supposed to be derived from the atmospheric air drawn into the blood in inspiration and in each dilatation of the arteries in their pulses. The true purposes served by the pulse and the heart's action and the motion of the blood are, "1, the translation of the cruor from the sinus of the vena cava to the left cavity of the heart; 2, the increase of heat; 3, the fabrication of arterial blood; 4, the production of vital spirit; and 5, the distribution of the primordial life residing in the spirit of the heart through the whole body." And in respiration are effected, 1, the evaporation of watery vapour (not smoke) formed in the conversion of cruor into sanguis in the heart; 2, the entrance of air and all it contains, including often the causes of disease, into the body.

Animal functions. Besides sensation and motion, which almost alone the ancients include, in this class, there are other functions which they have nearly neglected, which are observed only in the higher organisms, and by which the connexion and oneness of the phenomena of life are maintained. Every natural being is the expression of *one* indwelling idea. The foundation of this unity is the life itself, which in animals is identical with the sensitive mind, the *anima sensitiva*, governing every single function and inducing their agreement and unity. But this life or light cannot act directly upon matter, except when the latter has a dynamic principle in it; and on this, the archæus, the life acts. In the composite

animals, where each part has its archæus, the *archæus influus* is over all, and is at the same time the medium between mind and body. Through the medium of the superior archæus the mind governs all parts of the body, and by its vital ray is everywhere present and president.

But this dependence of all the parts on one central point does not completely constitute their unity. They are further combined; first, by their mutual dependence one upon another, each preparing or doing something necessary for the rest; and, secondly, by the influence which the alteration of one part has upon the functions of another, as shown by observation of facts: (in a word, by sympathy.) The ancients could not explain this, for they supposed that something material must connect the sympathising parts, or pass from one to the other. But the sympathy depends on a peculiar mode of action of the whole economy, an *actio regiminis* or *influentialis*, which works without any corporeal contact whatever—by a *continuation of virtue*—as the stars act upon each other and upon terrestrial objects, and as magnetism and sympathy (in things out of our bodies) work. Hence are dispelled all the *vapours* which the ancients supposed to pass from one part to another.* They are all results of the *actio regiminis*, in which, as the soul by a nod indicates its will to any part, so the state of any part is conveyed dynamically and through the medium of the archæus to another part in which corresponding changes of action are induced.

c. Psychology. The true key to the whole of Van Helmont's system is furnished by psychology, and its principles are these:†

The spirit of man is the only part really created in or after the image of God, and it is therefore immortal and unchangeable. The mind is but as the shell of the spirit (*siliqua mentis;*) it belongs to the earthy transitory life, and all its functions,—perception, memory, induction, judgment, discourse, imagination, fancy, are under the strict laws of necessity to which all nature is subject.

"At first there was an immediate connubial union of the immortal spirit with the archæus......Before the fall of Adam there was no sensitive mind in man. ......But after the fall the soul withdrew itself, like a nucleus, into the centre of the sensitive mind, to which it was then bound by the chain of life." (Van Helmont, quoted p. 59.)

The understanding and discursive faculty shine not obscurely in the brutes; and these faculties are therefore not to be held at so high an estimate as they have been hitherto. The mind, indeed, is not itself corporeal, nor the product of the body, nor matter; but all its functions are the results of life working in that which is corporeal, in the same manner as all the phenomena of life are the acts of matter and a power, the archæus, which again is, strictly speaking, only a part of life. Nay, "life and the mind are [in brutes], as it were, synonymous." But

* Van Helmont is unusually rich in his facts regarding this sympathy, especially that of other parts with the stomach and uterus; he even speaks of influences *reflected* from the uterus.

† In the following section, that which Van Helmont calls *mens* and Dr. Spiess *geist,* we translate *spirit; anima, anima sensitiva,* and *Seele,* we render *mind,* or *sensitive mind; ratio* and *discursus* and *intellectus,* (which, with Van Helmont, are synonymous,) and which Spiess renders *verstand,* are, *understanding;* in Coleridge's meaning, "the faculty judging according to sense." We would particularly draw attention also to the frequent similarity of Coleridge's and Helmont's views.

neither of them is self-existent; the spirit alone is truly subjective. The mind can of itself originate nothing; all its knowledge is acquired through the senses; its thoughts are all sensuous, and "the more thinking approaches towards abstract reasoning, the more does it partake rather of the life of the soul than of the peculiar vitality of the sensitive faculty." (Van Helmont, quoted p. 63.)

There is, however, a great difference between the mind or life of the brute and that of man. Both are imparted by divine in-breathing in the formation of the creature, as already expressed. But the mind is the *whole* and *sole* life of the brute, all whose functions depend on it; and when it has accomplished all it was ordained to do, it "passes away into nothingness like the light of a taper." It is otherwise with the mind of man; he has an undying spirit, which, after the death of the body, exists for ever as a *substantia luminosa.* As in brutes the mind, so in man the spirit, is the true light of life. His mind (which he first acquired in the fall) is indeed also a vital light—all the vital acts of the body depending on it—but it is not the life itself; it borrows its life from the spirit, as the moon from the sun.

"And as in the moon the light of the sun loses its warmth, and puts on a coldness foreign to itself; so also in the sensuous life (or mind) the ray of the spirit, though in its purity it is intellectual, passes into the domain of the sensuous, and finds there a terrene law opposed to the law of the spirit." (Van Helmont, quoted p. 66.)

There are thus three stages: 1. In the lowest stages of life the archæus is the life or light, and acts of itself, though according to laws impressed upon it. 2. In the higher, that is in brutes, the *anima sensitiva* is the life; it is the determining and form-giving power, and the archæus is its servant. 3. In the highest, in man, there is an immortal spirit, clothed upon and obscured by the mind, through which it imperfectly acts, and which again acts through the archæus.

Now, since the mind's influence is felt in every part of the body, and the mind or life is not divisible (though parts of the body may be removed without damage to it,) it follows that the mind must have some proper central domicile. "As the sun is not truly anywhere but in its own place in the heavens, though its light is wherever it looks; so is the judgment of the mind given from a central place." This place cannot be the heart, that is too restless; nor the brain, for many reasons. It is the *duumviratus*, the stomach and the spleen, which belong essentially to each other; for "the spleen serves to prepare the ferment of the stomach, and is its sun, its concoctor, and director; and therefore the conspiring of the two viscera shall be called *duumvirate*." These are to the animal what the roots are to the plant; and as in the latter the beginning or principle of life is in the root, so in the animal must it be in the region of the stomach. Besides, thoughts are said to rise *to* the heart, not to arise *in* it, as if they were engendered in it; nor to descend to it, as if they came from the brain. And, again, the workings of the passions are felt most in the precordia. But the best evidence I (Van Helmont) gained when in an extasy under the influence of napellus: "I *felt* that nothing is done in the head; and that, in an unspeakable manner, the whole mind thinks in the precordia."

From this its residence which it never leaves, the mind radiating, is,

after a manner, present everywhere. But, since it is not a pure spirit, but only the spirit operating through the sensuous faculty, it cannot of itself generate or do anything, but requires for every act a distinct organ. This is true of the properly mental acts as well as of the corporeal already considered. Each of these has its proper organ. The will has its seat in the heart; the organ for the mental acts, and especially for all those of objective thought, is the brain; not, however, that the brain can of itself think anything, but only because in it the memory has its seat. The true agents of thought are the mind and the spirit within it in the precordia, whence their light radiates. The brain then is, through the afflux of rays from the precordia, the organ of all thoughts of objects (all subjective thought is spiritual and precordial;) it is also a kind of record-chamber for the thoughts of the mind: "in regard of motion it is the executive organ of the mind, presiding over the nerves and muscles; in regard of sense it possesses in itself the faculties of memory, will, and imagination." (Van Helmont, quoted p. 73.)

As for the spirit, "it is a spiritual *substance* [purely subjective;] a vital luminous creation."

> "Its continuous and undisturbed operations are insensible; for that which in itself is sensible cannot be altogether spiritual and merely abstract...... Happy the man to whom it is permitted to perceive those insensible operations of the spirit, and to reflect on them against and above the powers of the sensuous mind! ...... Would that it could be granted to an atheist, at least for a single moment, to taste what it is to reason rationally, that he might, as it were by touching, feel the immortality of the spirit!...... The proper operations of the spirit cannot be more distinctly propounded than by the speech of silence; since that which is most properly the genial operation of the spirit is altogether abstract and is believed...... The spirit is the immediate image of divinity; therefore, as the eye beholds nothing more absolutely than the sun himself, although it cannot tolerate his brightness, and beholds all other things by him; so the spirit, properly, principally, and intimately, thinks on and contemplates nothing but that divine unity, and all other things by it...... But who am I who write these things? Verily I fear lest I should be the bell calling the faithful to the temple, but itself remaining out of doors in the height of the tower." (Van Helmont, quoted pp. 75-9.)

d. General pathology. "Disease and death are diametrically opposed to life. Hence, every disease must implicate the life itself, and since it can do this only by coming into contact with it, it follows that every disease must have its seat in the life itself. But since the real vital principle, which is identical with the mind, is incorporeal, and acts on the body only through its organ the archæus, therefore it is the archæus through which every disease manifests itself, and which is the proper seat of all disease. That which makes the unity of all the parts is the life acting through the archæus; this is altered in disease; and in it therefore must be the seat of all disease. The mind has its seat with the archæus, in the precordia, and here also must be the head-quarters of disease.

"But what is *disease?* It is not a mere negation, a mere absence of completeness or of health, but something positive; it is not a diathesis, nor an accident disturbing actions, much less is it that disturbance itself resulting from the conflux of injurious causes with our corrective powers. But disease is an *ens reale*, having a material and an efficient cause, excited by occasional causes." (Van Helmont, quoted p. 84.) It is "an

entity verily subsisting in the body," and working according to definite laws; and this may be proved especially by the phenomena of periodical and latent diseases, in which the disease continues to exist even when there is no disturbance of function.

Disease then is a peculiar form (or mode) of life; and cannot, therefore, exist in the same point with the proper life, i.e., with health. And since every life works according to one determined *idea*, (or according to definite laws,) which is impressed upon the archæus,(or, in animals, upon the archæus influus, and through it upon the archæi insiti;) it follows that when disease (whose consequence is a disturbance of the vital acts, and whose seat is in the life or in its organ,) ensues, it must be because the archæus has had some new foreign *idea* (or model of action) impressed upon it. And this new idea may be impressed on either the life or mind; but more generally it is impressed on an archæus influus, or an archæus insitus.

It has been said that in every disease there is both a material and an efficient cause; and again, the archæus (in the animal body) is no mere dynamic essence, but a power so inseparably connected with matter, that an alteration in the one is of necessity immediately followed by a change in the other. Hence, either the material or the efficient cause of disease may act upon the archæus; that is, either the matter with which the archæus is united may be primarily changed, or the archæus itself may have a morbid idea impressed upon it; in other words, the cause of disease may be either dynamic or material. It is to be understood also that though diseases are in so many respects analogous to other forms of life, yet they differ in this; that they can manifest themselves only in other living organisms; they have no self-standing life. They are archæi acting after a wrong plan; and their symptoms are the phenomena consequent on this wrong acting.

Disease originates whenever any noxious impression is made upon an archæus; and this, whether the impression be entirely dynamic, or the result of a noxious matter either from without, or generated within the body. By this the archæus is excited, frightened, or disturbed; and under this disturbance the morbid idea is generated. This is always the first step; and next, but immediately, the idea (like every idea of the archæus,) incorporates itself, as the *causa efficiens seu seminalis morbi*, in some adjacent or appropriate matter, and thus attains a definite existence as actual disease. Diseases are thus produced like other beings, by the incorporation of an idea in matter; but with this difference, the ideas of other natural beings are embodied in seeds at the creation, or exist as ferments at all times; while the *ideæ morbosæ* are first generated in the archæus of a living being and can only in some cases be propagated.

The occasional causes of disease are all those things which are prior to the incorporation of the morbid archæal idea. Wounds, poisons, noxious food, contagions, &c. are of this kind. However intimately they may have passed into the body, they are yet *external* causes; they do not constitute the disease; nor do they produce it till they have excited the archæus, and engendered the morbid mode of action. So also the products of a disease; they are not the disease, though by their noxious influence they may be the occasional causes of secondary diseases. These products moreover, or material changes of structure or composition, are

produced when an archæus insitus is affected, and this may take place either directly or through the archæus influus; so long as the latter alone suffers, symptoms of disease and mere disturbances of function are alone produced.

Thus originating, diseases in general are of two kinds. 1, Those which have their morbid idea in the archæus influus, which are general and transitory, or, for the most part, acute diseases; and 2, Those in which the idea is in an archæus insitus, which are local and persistent or chronic.

Another mode of division of diseases is according to their origin, in which view all are either, 1, Archæal, (properly so-called,) in which the morbid idea *originates*, (in some unknown mode,) in the archæus; or 2, Archæal, but secondarily so; the morbid idea being generated by something troubling the archæus.

Of the diseases which are archæal in their origin there are four kinds. 1. Hereditary diseases, in which, in generation, a morbid idea is impressed upon the archæus of the seed, and remains for a certain time, sealed up in it. 2. Silent or latent diseases, such as epilepsy, which have no material foundation, and in which the morbid idea lies long at rest and concealed in the inmost part of the archæus. 3. *Torturæ noctis*, periodical diseases, which manifest themselves chiefly at night, and seemingly, under the influence of the moon. 4. Those due to *robur inequale*, in which, by an hereditary or an acquired defect, the vital force is imparted more or less abundantly to one than to other parts of the body; as one often sees, in whole families, a particular organ weaker, and, though not diseased, yet more prone to disease than the rest.

Of the diseases which originate in occasional causes disturbing the archæus there are many more kinds, but they may be divided, in general, into two great classes; those namely of *recepta* and *retenta*.

The *recepta* are always primary diseases; they result from noxious influences which get into the body as it were by stealth, and disturb the archæus till a morbid idea is generated. And these influences are of four kinds, viz. 1st, *injecta a sagis*, such as are thrown in by witches and charms: 2d, *concepta*, those which have their origin in disturbances of the passions and affections, which, thus disturbed, communicate a morbid idea in the archæus, and through it produce diseases of the body as well as of the mind: 3d, *inspirata*, which are taken in with the air, such as all vapours, and which act, not upon the lungs, but, by penetrating the diaphragm, upon the stomach: and, 4th, *suscepta*, such as act mechanically, as wounds, blows, &c.; but these are causes of disease only when the archæus is excited for the reparation of the injury.

The *retenta* are always secondary diseases; the noxious influence which disturbs the archæus being the product of a previous disease, or something which should have been removed from the body, but has been retained through a diseased state of the part destined to remove it. The noxious influences may be placed in two divisions. 1. *Assumta*, in which matters imperfectly digested or assimilated in consequence of disease of the digestive organs, act injuriously on the archæus, whether they be food, drink, poison, or physic. And in this class also are the products of former diseases. 2. *Retenta innata*, which again are divisible into three kinds, viz.—*a*, *Retenta relicta*, when, through previous disease,

the various secretions and excretions of the several stages of digestion, instead of being in due time cast out, are left in the body; β, *Retenta transmutata*, in which morbid matters, or defective compositions of the fluids, are produced by previous disease and pass into the blood; γ, *Retenta transmissa*, when the *transmutata*, instead of being excreted, pass to parts of the body to which they are not properly destined.

E. GENERAL THERAPEUTICS. The philosophy of healing is consentaneous with that of all this system. God, in his goodness, has given remedies for all diseases; and it is our place to know and use them. The old notion of *signatures*, or likenesses, as indications of remedies is mere nonsense: man is created after the image of God, not of nature.

Medicines, like all other things, may act in two ways, viz. corporeally, as all purely chemical and mechanical means; and abstractedly or formally, by a pure dynamic quality. The general properties of medicines may be named *savours*, not because we can taste them, but because all organs seem to possess a sense by which they can discover whether anything that comes in contact with them is friendly or hostile. In this sense there are two kinds of savours: 1, the *sapores rerum*, by which things are bitter, sharp, salt, &c., or, in general, *salia;* and, 2, the specific, formal savours, by which their intimate essence is revealed, though not to our senses. In medicine, the material savours cannot act on the disease itself, which has its seat in the immaterial life; they can only correct some of the causes of disease, such as the noxious substances generated in the modes already mentioned. In this manner some act at once in the stomach; others, such as diuretics, must first pass through the second digestion; both, by removing the occasional cause of the disease, give opportunity for the archæus to act.

But the most important are the medicines which act by a specific formal power; and to know these is the physician's highest attainment. That there are such medicines which act, not by any material combination, but by their mere presence, their contact, or, as it were, by a glance, is proved by many facts: e. g. water in which mercury has been boiled kills worms, though the mercury has lost no weight in boiling. Their real mode of operation cannot, indeed, be discerned; for even the specific savour or odour by which the archæus knows that they are beneficial, is but an evidence of their virtue, not the virtue itself; and even this savour is not cognizable by our senses. The active power is an entirely hidden property, identical with the essence of that to which it belongs.

The whole of our knowledge of the action of medicines may be thus expressed:

"Every remedy acts immediately and principally upon the archæus of the stomach; and the archæus in succession acts according to the disposition which he imbibes, and which is generated in him by the virtue of the remedy. Further, also, it follows that every remedy whatever acts by approximation, after the manner of a mirror; since above and beyond its material composition it acts by mere approach to the archæus. Now, the archæus himself first perceives the virtue imbibed from the remedy, and in that sensation forms for himself an idea of things to be done by him in following the dispositions of that virtue; whence, consequently, engendering in himself peace or rest or anger, and, assuming the ideas of these, he presently disperses them among the viscera which are listening to the

actio regiminis, performing agreeable or opposed services according to the command of the vital archæus." (Van Helmont, quoted p. 144.)

III. We have thus, in the briefest possible space, given a general view of the famous system of Van Helmont; and little room is left for the other department of the work. However, the chief points in which this system is distinguished from those of the Greek and Arabian schools have been noticed in the course of the analysis; and for those in which Van Helmont differs from Paracelsus, a comparison of this article with that on the works of the latter will afford a sufficient clue. It cannot be denied that Paracelsus had imagined the foundation of Van Helmont's system; yet he had expressed himself so obscurely, and had surrounded his truth with so much error, that re-invention would have been nearly as easy as a comprehension of his works. Besides, his followers had so misunderstood and perverted his views; they had so neglected that which was good, and made so ill an use of that which was already bad; and his school had degenerated into such a mere cabal of mad fanatics; that when Van Helmont came into the field the doctrines of the schools were almost as universally received among the more learned of the physicians as if Paracelsus had never lived. The whole battle had to be fought again; and Van Helmont's victory was lasting. To him belongs all the honour of setting in motion that scientific inquiry into the laws of life as of a peculiar principle, which has now led to the discovery of many laws and a doubt of the existence of the principle. We call it scientific, though in Van Helmont's time the inquiry was conducted in a manner which would not now deserve that epithet; but, considering the state of knowledge when he wrote, the absence of all certain principles in all those which are now sciences, the volumes of falsehoods universally admitted as truths, the cramped and fettered modes of reasoning which were then in vogue, and the difficulties of inquiry of every kind, we cannot but wonder at the success which Van Helmont attained. His system, in the midst of all its error, contains the elements of numerous great truths; divesting it, even more than we have done, of its strange words, it might be read as a work written but half a century ago. For archæus, read nature or vital principle, and it would in many parts describe very fairly the popular medical theory of the present day. And, on the other hand, there is no weak resemblance between its metaphysical part and that which is now generally current in the German idealism-schools: with a few mutual adaptations of mere expressions the two systems might be made essentially alike.

To all who are interested in the history of medicine, Dr. Spiess has rendered a truly admirable service. With enormous labour, he has given a clear and intelligible view of a system which, in the original, is most obscure: he has distinctly and briefly interpreted that which has hitherto been utterly misrepresented; and he has rendered due honour on just grounds to one who, if he has hitherto at all received it, has received it from those who were but imperfectly acquainted with his merits. That, in such a task, the learned interpreter should have given too bright a view of his author, should have often put aside or veiled his faults, and have passed by his deep obscurities, is but a venial fault.

Art. XII.

On Spasm, Languor, Palsy, and other Disorders, termed Nervous, of the Muscular System. By James Arthur Wilson, m.d., Fellow of the College of Physicians, and Physician to St. George's Hospital.—*London*, 1843. Sm. 8vo, pp. 201.

We hold it incumbent on any man who puts himself forward as the advocate of a new Theory to show, in the first place, that it is *called for*, by pointing out conclusive objections to that which was previously in vogue, and by demonstrating (or at least attempting to demonstrate) the freedom of his own from similar difficulties. When we took up Dr. Wilson's book, therefore, and gained from a cursory glance into its contents that the author has a new explanation to propound of the disorders of which it treats, we naturally looked for such a contrast. Having been unsuccessful in finding it—and having gathered, from a subsequent careful perusal of the work, that Dr. Wilson has given so little consideration to the physiological doctrines at present in vogue as to be incapable of making it,—we shall endeavour to supply the deficiency.

The readers of this Review need not be informed that we have, on all occasions, advocated the doctrine that muscular fibre possesses the property of *contractility* on the application of a stimulus, as an independent endowment; the existence of this property being dependent upon the maintenance of its organic structure by the nutritive process, and, therefore, upon the fitness of the blood to afford the requisite supply of duly-assimilated materials; and the readiness with which it may be called into action being dependent on a due supply of oxygen, which is also conveyed by the blood, and (in vertebrata at least) especially by the red corpuscles. Hence the contractile property of muscle may be impaired by causes acting through the blood; an insufficient supply of well-assimilated fibrin will prevent the due nutrition of the tissue, and an imperfect arterialization of the fluid will deaden the contractility so long as it continues.

Moreover there can be no doubt that poisons acting through the blood may impair or even destroy the contractility of muscle, by their direct influence upon the tissue. This is the case, for example, with lead, which is found to be deposited in the substance of the muscles which it paralyses; it is the case also with nitrate of potass and other neutral salts, which paralyse the heart as soon as they find their way (in sufficient amount) into its muscular substance through the coronary vessels. Many other substances, there can be no doubt, have an equally direct influence upon the muscular tissue when carried by the circulation into its capillaries; and if Dr. Wilson had applied himself to determine the operation of these by well-devised and well-conducted experiments, he would have done good service to science. That there are substances also which increase the contractility of muscles, there can be as little doubt; but, so far as we at present know, these substances act either by improving their nutrition or by increasing their supply of oxygen. The vegetable tonics may effect the former by their influence on the digestive and assimilating processes; whilst the agency of iron seems to be specially ex-

erted in increasing the number of red corpuscles, and thereby aiding in the introduction of oxygen into the system.

But that any substances, with whose operation we are familiar, have the power of suddenly exalting the contractility of muscular fibre, to such a degree that the most violent and general spasmodic actions may be produced without any exaltation of nervous power, we altogether deny; and it was incumbent on Dr. Wilson to have *proved* this, before making this assumption (for it is no more) the foundation of his whole pathological system. It is true that he asserts that strychnia operates in this manner, but he brings no further evidence of it than that it produces violent spasmodic actions when circulating in the blood. No attempt whatever is made to controvert the received doctrine that it operates by inducing a highly-increased excitability of the Nervous system, especially of the spinal cord; a doctrine which appears to us fully established by the facts to be hereafter mentioned of its producing well-marked tetanic effects when directly applied to the spinal cord alone after the circulation has ceased. In order to prove the direct operation of the exciting cause of tetanus or any other agent upon the muscles, it would be necessary to examine how far the contractility of these muscles is increased by the circulation of these substances in the blood, *after* the muscles have been completely separated from their nervous connexions; and to observe their influence upon the heart and muscular coat of the alimentary canal, whose ordinary movements are but little affected by the nerves. Neither of these experiments have been performed, so far as we can learn, by Dr. Wilson, yet, upon the basis of a small number of facts, every one of which (as we shall presently show) is equally well explained upon the received doctrines, he erects the new hypothesis that, in the greater number of cases at least, general spasmodic, as well as paralytic disorders, are to be attributed to the direct morbid influence of certain agents, circulating in the blood, upon the muscular tissue.

Let us now turn to the nervous system; and examine its agency upon the muscles, and the influence of external agents upon it. The contractility of muscular fibre may be called into action by stimuli applied directly to itself; this is seen in the heart and alimentary canal, and also in individual fasciculi of the voluntary muscles. But no stimulus that we know of can produce a regular combined contraction of the latter, except one that acts through their nerves. In the living system, then, we cannot doubt that every contraction of a voluntary* muscle is occasioned by a stimulus acting through its nerves; whether this stimulus originate in the will, in an instinctive or emotional impulse, or in a reflex action of the spinal cord. The operations of the nervous centres, by which these effects are produced, are far more immediately dependent upon the blood than are those of muscular fibre. We check the circulation in the brain, and instantaneous suspension of its functions is the result. Impaired states of nutrition have an obvious influence upon the energy of the nervous system; and agents conveyed by the blood to the nervous centres, manifest their influence in a still more remarkable degree. Who can doubt that alcohol increases the activity of the operations of the brain, and that opium diminishes it? And why should we

* We use this term for convenience, although not strictly correct; since many of the contractions of these muscles are involuntary.

entertain any greater doubt that strychnia increases the excitability of the spinal cord by its direct influence on the nervous tissue, so that causes which would be in general inoperative to produce reflex movements, call them into violent operation?

We assert, without hesitation, that every one of the facts adduced by Dr. Wilson, to prove that the state of the blood has an influence in producing spasmodic disorders, may be explained equally well upon the received doctrine, that the morbid influence is exerted upon the nervous system. Consequently, there seems to us to be no occasion to quit this doctrine for one which, however specious, will be found to be entirely deficient in solidity at its foundation.

Dr. Wilson's views may serve, however, the good purpose of directing the attention of practitioners to the importance of considering general spasmodic diseases, and some forms of paralytic disease, as constitutional in their nature; and as originating, not so much in a local cause, as in a generally impaired state of the nutritive system, or in the retention of some injurious matter in the blood. It seems to us peculiarly necessary to bear this in mind, when applying Dr. Hall's views to practice; lest we should be led to look too exclusively at the state of the spinal system of nerves as the seat of the disorder, when the original fault is in the blood. We may explain our meaning by reference to hydrophobia. It is generally admitted that, in this terrible disease, a poison is introduced into the blood, which, after a longer or shorter period of latency, acts like a ferment upon the whole mass, and alters its character. The blood thus depraved, exerts an injurious influence upon certain parts of the nervous centres, and increases their excitability to such an extent, that the slightest causes are sufficient to induce the most violent spasmodic actions. It is quite possible that in traumatic tetanus, the similar result may be traced to an absorption into the blood of some disordered secretion from the injured part; for the *general* excitability of the spinal system of nerves on both sides of the body, which manifests itself, when the disease is once fully established, seems to us to point to the conveyance of a poison by the blood, through the whole of the spinal cord. In idiopathic tetanus, the constitutional or *humoral* origin of the disordered condition of the nervous system appears still more evident. And in those extraordinary forms of hysteria, which simulate these and other types of spasmodic disease, it is quite possible that we are to look for a similar cause; possibly, in many instances, the retention in the blood of matter which ought to have been got rid of through the genito-urinary organs.

That the nervous system has received, both from physiologists and pathologists, an undue share of attention, we are quite ready to admit. So far from being itself the source of life, as was long believed, it is only one manifestation of that life,—a manifestation of which one vast class of living beings is entirely destitute. But still the animal physiologist justly regards it as the instrument of certain functions, which increase in their variety, their importance, and their influence upon the organic life, as we trace them from the lowest animal up to man. And the pathologist, without any disposition to exaggerate its influence, must recognize in its disordered states the *immediate* cause of a vast number of phenomena, which ultimately may be very justly referred to the morbid

condition of the blood, which is at the same time the *pabulum* of its nutrition, and the *stimulus* to its action.

With these preliminary remarks, we proceed to consider some of Dr. Wilson's leading doctrines in more detail.

Dr. Wilson is of opinion that the influence of muscular action on the blood, and, through the blood, on the function of organs in general, has been much underrated. "It is still with the flesh," he observes, "as it long was with the blood. Both have been unduly neglected in modern pathology." In the work before us, Dr. Wilson proposes to remedy this comparative neglect of the muscular system. He asserts the importance of the latter, and insists that it is at least on a par with the nervous system, of which it is in a great degree independent. That, in short, the muscular system is much more under the influence of the blood than the nerve:

"From within as from without, from the air we breathe, from the food we swallow, as from the inmost recesses of the most distant structures, there is a channel by which, more directly than by its nerve, most of what influences the muscle is received into its fibre. It is by the continuous universal blood, which while it bathes the fibre touches the air and mixes with the food,—it is by the blood in mass and in current that the muscle maintains its great and constant relations with the external agencies of matter, as with the elementary texture of every organ in the body." (p. 4.)

Dr. Wilson avers, contrary to the established opinion of physiologists, that this action of the blood on the muscular system needs not the intervention of the nerve to explain it. The effects are direct upon the muscle, but (with some inconsistency) he adds, and therefore upon the nerve as being one of the parts of its entire organized structure. There is some truth in the following opinion:

"Neither in health nor in disease is the muscle to be regarded merely as the organ of motion, as ministering only to an occasional and mechanical function; but, collectively and in the mass, as the most extensive of living structures, as continually employing and employed upon a large proportion of the entire mass of the blood, thus elaborating the great common material of the body and preparing it for circulation elsewhere. And this, be it remembered, is true of the muscle at all times, whether in action or repose...... The muscles, indeed, in their constant function of nutrition are with respect to the blood as glands, ever busy in separating from it the materials of their own growth, and restoring it in an altered state to the general current of the circulation." (p. 7.)

We experienced no little disappointment in finding, on a further perusal of Dr. Wilson's volume, that no development of this truth was intended. The changes effected in the composition of the blood by muscular action are almost unknown; consequently, an inquiry into these changes, viewing the muscles in their ultimate operation on the blood as glands, would have been exceedingly interesting. It is rather the changes induced in the functions of the muscular system by the blood, in a morbid state, that Dr. Wilson proposes to consider. Spasm, tetanus, chorea, muscular pains, languor, and palsy, are thus considered consecutively. In addition, there are a few words on mesmerism and magnetic sleep, on baths and mineral waters, and on exercise. A "memoir of fatal convulsions with renal disease, read before the College of Physicians in 1833," and woodcuts descriptive of the hot-air bath used in St. George's Hospital conclude the volume.

We will select tetanus as an example of the mode in which our author developes his views:

"The great character of this disorder is one of active defiance to the will in organs synonymous with the habitual exercise of its power. The contractions of the muscular fibre are never stronger than in the tetanic spasm, when by volition they are most opposed. In seeking for the principle of this countervailing agency in the muscle, we are led by our former inquiries at once to the blood. The blood, it is known, is independent of the will. It has the power of arranging its own particles in its own way,—it contracts and expands by a function inherent and peculiar to itself. It stirs instantaneously and simultaneously throughout its entire mass, thus swaying by direct movement the muscle which in bulk it forms. By the operation of certain external forms of matter on the blood, tetanus can immediately and at any time be induced. Again, it follows as an occasional effect of disturbance in the general business of temperature and nutrition, of which the blood is the chief agent and sole material, and with which the will has no concern.

"By thus considering tetanus in its wide and constant relations with the blood, we cannot fail to know it in its true character of a great constitutional disorder. Because it begins and ends with spasm of the voluntary muscles, tetanus has hitherto been classed with affections of the 'nervous system;' an undue limitation of the disease, for which there is no sufficient warrant by the symptoms or in its general pathology. Like fever, it pervades the entire system; and is special in the flesh, being common in the blood. A fever in truth it is, spasmodic and remittent in its character," &c. (p. 72.)

From a consideration of the symptoms generally, Dr. Wilson finds proofs that the disease is a fever. "In familiar phrase," he says, "tetanus may be termed the muscle fever of pathology." The toxicological action of strychnia on the system, is adduced as a triumphant proof of the doctrine that the blood is the first recipient and principal agent of the material influence under which tetanic spasm is produced. Dr. Wilson further asserts that "there are no good reasons for believing that the spasm, and other symptoms occasionally observed to follow on slight local injury, are induced by mere effects of 'irritation' from the nerves of the injured part." (p. 77.)

If Dr. Wilson intends to advance in the preceding quotation that a morbid state of the blood is necessary to the production of tetanus as a predisposing cause, we are not prepared to differ with him in opinion. But if it be advanced (as we think it is) by our author, that there is a specific poison or *materies morbi* in tetanus, circulating with the blood, as there is in the exanthematic and contagious fevers, and, by being brought with the blood into contact with the muscular fibre, developing tetanus, we at once join issue with him. There is no proof whatever that even when a poison (as strychnia) is introduced into the circulation and convulsions follow, its primary action is on the muscular fibre; but there is proof that it acts on the nervous system. Flourens showed by experiment, some years ago, that strychnia acted directly on the cerebellum, and in a case of poisoning by nux vomica, Orfila and Ollivier found much serous effusion over the cerebellum, and its structure softened. And anatomy has repeatedly demonstrated morbid changes in the nerves and spinal cord in cases of traumatic tetanus, and traced the morbid changes upwards from the irritated nerve to the central axis. For examples of this kind we refer Dr. Wilson to the cases communicated in 1827 to the Royal Academy of Medicine, by Lepelletier. Dupuytren, Beclard, Descot, Swan, Curling, and Froriep, will also afford Dr. Wilson

convincing proofs that the irritation of the nerve passes directly to the central axis, and from thence is carried to the muscles.

We might add other observations of a similar character, but pass them by to notice Stilling's recent experiments with strychnia. This physiologist removed the entire viscera, namely, the heart, lungs, stomach, &c. from a great number of frogs; laid bare the brain and spinal cord from behind; and dropped a few drops of a solution of acetate of strychnia upon the latter. In *every* frog so treated, general tetanic spasms came on in about five minutes, and were re-excited by a touch, &c., just as if the animal had been poisoned by strychnine in the usual way. In these experiments the agency of the circulation was entirely excluded, yet if a small portion only of the spinal cord was touched with the solution of strychnia, the spasms were universal.* These experiments constitute a complete answer to the following assertion of Dr. Wilson's. "By no irritation, by no torture, however ingeniously devised, of any nerve or of any number of nerves in the body, would it be possible to establish a complete tetanus in the muscular structure under a given period of time, varying from forty-eight hours to three weeks." (p. 78.)

To say that "the blood entire is sensitive as the individual nerve of external impression, instantaneously and simultaneously perceived through all its distributions," (p. 86,) and thus to attempt to explain the peculiar phenomena of tetanus, is to found its pathology on what is really and truly a baseless vision, and we should think exclusively the property of Dr. Wilson.

The above exhibits Dr. Wilson's pathological views: let us now look at his treatment. Chorea in its acute form affords, according to our author, another variety of the "muscle fever," of which tetanus is the type. How does Dr. Wilson treat this "fever?"

"A young gentleman, aged thirteen, was brought to my house on the morning of October 5th, 1840, with severe chorea affecting the voluntary muscles of both sides of the body; and reported of about ten days' duration. On September 26th, while at school, he became unusually silent; and was observed to be continually engaged in unbuttoning his waistcoat, with general restlessness of manner. For some weeks previous his temper, naturally quick, had been remarked as irritable. The boy's health had always been considered delicate. On October 10th, when he was again brought to me, the spasm of the limbs was much aggravated; and he was unable to articulate. On October 13th I was summoned to him at his father's home, and found him convulsed as by tetanus or hydrophobia. His face was frightfully distorted; his tongue, black and dry, was forcibly protruded. By the violence of the spasm he was thrown repeatedly from the couch, on which he found no sleep. He was continually rending his clothes, and howling as if in agony." (p. 112.)

This patient died on the nineteenth day, of what we should have entered in our case-book as cerebral meningitis, commencing probably

* Untersuchungen über die Functionen des Rückenmarks und der Nerven.—Leipzig, 1842, p. 40. In 1838, Van Deen made many similar experiments on frogs, but with this difference, that the solution of acetate of strichnine was placed in the mouth. The results were the same. So early, however, as April, 1809, Majendie showed, that if the spinal cord be cut across, and one of the cut surfaces be placed in contact with a small quantity of upas, those parts only became convulsed which receive nerves from the portion of the spinal cord so treated.

at the base of the brain. Be this as it may, we need not comment on the following account of the treatment:

"The treatment, in the first instance, was by brisk purgatives, with tonic alterative medicines, including the Fowler's solution of arsenic. Latterly, morphia was given in small and repeated doses." (p. 112.)

It is evident to us, that in treating his subject Dr. Wilson has not had it fairly before him in all its relations. He has not apparently even made himself acquainted with the best established and now generally known neurological theories of the day. He writes almost as a physician of 1743 would write if he could rise from his grave and grasp a pen. For example: the cramps in the lower extremities from which pregnant women suffer, are now supposed to be dependent upon direct irritation of the lumbar and sacral nerves. Dr. Wilson thinks this explanation unsatisfactory, and he mounts his muscular hobby-horse to controvert it:

"It should be remembered," he says, "that in the process of gestation the general health is involved, and that the muscle is thus brought under influences affecting it in its entire structure, from which, in their effect on the blood and the fibre, spasm may ensue with as much likelihood as from pressure by the enlarged uterus on the trunk of the distant nerve."

We are quite ready to agree with Dr. Wilson in his opinion that the pathology of the blood has been hitherto much neglected, and that important results may be expected to follow from the study of pathological chemistry; but surely nothing can result from such loose hypotheses as he adopts, unsupported by experiment, and most illogically and obscurely stated. How can we possibly believe that the passions excite sensations independently of the nerves, and altogether through the blood? "Why," asks Dr. Wilson, in his enthusiastic devotion to the blood, "why should we suppose in such cases an agency of the nerve, intermediate between the material affording the impression and its physical effects first observed in the blood? Is anything in the body nearer to external influences than the blood, spread out as it is in the lungs in direct contact with the air, and with all that the air contains? Can this be affirmed of the nervous structure in either of its great divisions; of the double symmetrical nerves, or of those forming the ganglionic system?" (p. 38.) If Dr. Wilson had only asked himself the question, whether the blood had five senses or not; and whether it could hear, or see, or smell, or taste, or feel, he would surely have never published to the world such notions as these.

The quotations already given show Dr. Wilson's style of writing to be curiously quaint and obscure: it sometimes offends in other ways against good taste. The following sentences are examples:

"There is no state of the 'nervous system,' in its structure or function as generally understood, with which spasm of the voluntary muscles, however violent, is not compatible. Cæsar, great Julius, rose from earth-bound fits again to command the world; and he, the foremost among living men, like Cæsar in these unconscious struggles, still triumphs over himself." (p. 45.)

"By the term muscle, therefore, in our proposed inquiry will always be understood the living flesh in its combination of many parts, with the blood predominant in all; and by muscular action we shall never mean less than a result of the triple agency of nerve and fibre with the blood." (p. 8.)

Art. XIII.

Recherches Anatomiques, Pathologiques et Thérapeutiques sur la Phthisie. Par P. C. A. Louis, Médecin de l'Hôtel-Dieu, &c. Deuxième Edition.—*Paris*, 1843. 8vo, pp. 688.

Anatomical, Pathological, and Therapeutical Researches on Phthisis. By P. C. A. Louis, Physician to the Hôtel-Dieu, &c. &c. Second Edition.—*Paris*, 1843.

The reputation attached to M. Louis' name, through the first edition of his work on Phthisis in the Adult, is likely to be very materially increased by the second version which now lies before us. The additions, and these are numerous, are invariably of that soundly philosophic character which characterizes all the productions of their author; and the full consideration of the more directly practical branches of the subject discoverable in this edition must win him the unreserved suffrages of those who, while they acknowleged his merits in the pursuit of pure science, questioned his capacity for its practical application. We shall endeavour in the following pages to make the reader acquainted with the prominent features of the work, while we at the same time describe, as completely as our space will permit, some of the chief phenomena of the all-important malady it investigates.

I. Morbid Anatomy. That the symptoms of pulmonary consumption arise solely as the result of the deposition of a certain adventitious product, called tubercle, in the lungs, is now matter of universal conviction. And if the *direct* researches of various observers of phthisis, and the general result of these researches—that individuals have never been known to die phthisical without the existence of that product being established after death—might some years since have been demurred to by cavillers on the ground that the train of functional disturbances attending the growth of cancer in the lungs, had not been made out with precision, even this source of objection is at the present hour removed. The symptomatology of pulmonary cancer has been ascertained with very considerable accuracy, and the differences between its course and that of phthisis placed in sufficiently strong relief.

Tubercle in the lung then constitutes the anatomical character of phthisis. The term is a bad one; it leads to the idea that form is an, or indeed the essential point in the physical aspect of the product—an idea quite at variance with fact, as its shape is a mere accident of position, the intimate nature of the material remaining the same, whatever be the outline—rounded, flattened, branched. &c.—which circumstances may have impressed on it. It is more correct then to speak of tuberculous matter than of tubercle; and this matter when fully developed is opaque, yellowish, firm, yet friable and of little tenacity, caseiform, occasionally somewhat unctuous to the feel, inelastic, collected into masses varying in size from that of a pin's head to a pigeon's egg, without smell, of uniform aspect all over its surface, unmarked by vessels, insoluble in water, and if mixed with this quickly subsiding to the bottom. When these characters exist, tubercle may always be recognized; if some among them be so modified by circumstances as to make the nature of

the product on first sight doubtful, closer examination will commonly succeed in removing the difficulty.

But in the great majority of cases tuberculous matter as just described is found associated in the lungs with certain small bodies not much larger than a millet-seed or very young pea, of shining aspect, lilac or slightly pinkish colour, rounded or angular form, and considerable firmness: these are the semi-transparent gray granulations of many writers. So frequent is the association that tubercles were only found in two cases by M. Louis without gray granulations; and the latter without the former in five. Laennec regarded these granulations as the early condition of the yellow tuberculous matter; a doctrine supported by Louis on the following grounds. 1. Like tubercles the gray granulations are larger and more numerous at the summit than at the base, and limited to the summit, if they are not scattered over the lung generally. 2. At a certain period they present a yellowish point in their centre, which is larger in proportion as it is nearer the apex of the lung. 3. The two products are, as already mentioned, almost invariably associated. 4. The gray matter occurs in infiltrated masses also, and then too presents yellow tuberculous points in its interior. 5. In some instances gray matter undergoing the yellow transformation, is found in other organs besides the lungs. We believe that these arguments (the fourth excepted which, as will be by and bye seen, possibly involves an error,) have not been satisfactorily set aside by any of the advocates of the mutual independence of the gray granulation and yellow tubercle. And the notion of Dr. Carswell especially, that the gray granulation in the lungs is tuberculous mixed with mucous matter, which two materials subsequently separate from each other,—the former accumulating towards the centre, the latter at the periphery of the mass,—appears to us destitute of all probability.

This question, as well as all others appertaining to the intimate history of tubercle, would appear perhaps to be capable of easy elucidation with the assistance of the microscope. But the contrary is unfortunately the truth; at least the most diligent micrographers give accounts so discordant upon this point, not only in seeming but sometimes in reality, as to justify us in concluding that the study of tuberculous matter is one beset with no ordinary difficulties. Kuhn, Gluge, Vogel, Henle, Gueterbock, Wood, Gruby have laboured in this field without succeeding in establishing the micrology of tubercle, or satisfactorily aiding the pathologist in his inquiry into its mode of origin; and amid the conflicting statements tendered, perhaps that of the essential characteristic being a finely granular substance is the only one that can be received without hesitation. The granules composing this, dark coloured, closely packed, and of minute size, measure from 1-12000th to 1-15000th of an inch in diameter. This is the condition of tuberculous matter which we just referred to as clearing up any doubts that might be entertained of its nature. M. Louis gives certain results, obtained by a M. Lebert, from the microscopical investigation of pus, cancer, and tubercle; but as these results do little more than prove their author to be in the noviciate of his studies of minute structure, it is certainly needless to detail them.

It will not be necessary for us to follow closely the anatomical history of tubercle of the lung in all its bearings; more especially as our space

may be more profitably employed in the extended consideration of some particular and debated points. One of these is the nature of the homogeneous, grayish, "semi-transparent," shining, firm mass, encountered in many cases around tuberculous cavities, and more rarely independently of these. M. Louis retains the precise views respecting this appearance formerly promulgated by him,—he regards it in part as the result of infiltration of semi-transparent tubercle (of the material of the gray granulation) through the pulmonary tissue; in part as the produce of chronic inflammation. We confess we can see no very urgent motive for considering that the appearances referred to are ever other than the results of chronic inflammation: the fact of its frequently presenting gray granulations or yellow tubercles on its surface proves nothing, for there is assuredly no reason (to state the matter in the least dogmatical manner) why those formations should not occur in parts infiltrated with the product of simple chronic inflammation. The characters of the pulmonary tissue thus affected are certainly not obviously and materially different from those observed in simple chronic pneumonia occurring without tuberculous disease. The author doubts the identity of the gelatiniform tuberculous infiltration of Laennec with the gray semi-transparent matter; no doubt correctly,—Andral long since contested the accuracy of Laennec's views upon this point.

M. Louis makes no alteration in the statements promulgated in the former edition respecting the greater proneness of the left than the right lung to suffer from tuberculous disease. Numerous facts have nevertheless been collected of late years showing that some doubt may be entertained of his perfect correctness in this matter; fuller allusion to these will be found in a former volume (Brit. and For. Med. Rev., Vol. IX. p. 334,) where we also made it evident that M. Louis' own cases do not so obviously support his opinion as he himself appears to imagine.

The duration of the several anatomical stages of the malady is approximatively estimated by M. Louis; but the difficulty of its determination has not appeared less to him than it has done to others. The time necessary for the growth of the gray granulation to the size of a pea is, for example, impossible to determine in general terms; certain cases of acute phthisis appear to show that it may attain this bulk in the course of two or three weeks; while, on the other hand, the author has met several instances in which persistent cough and occasional hemoptysis had existed for years, and yet gray granulations of the size mentioned, or even smaller, were the only pulmonary lesion discovered. Nor is the period of softening less subject to vary,—in some cases occurring from the twentieth to the fortieth day, in others after a much greater lapse of time. The end of the third or beginning of the fourth month are the earliest periods at which M. Louis has established the existence of empty cavities.

The distinction of simply purulent from tubercular cavities, though commonly easy and justified by a multitude of conditions familiar to the pathological anatomist, is in some instances a matter of real difficulty. M. Louis' third case exemplifies this. Here was a young woman who had coughed for seven months previously to her admission, and dying, seventeen days after, presented at the summit of the right lung an excavation of moderate size, partly filled with a turbid, greenish fluid, and

containing a piece of lung perfectly free from all connexion with the rest of the organ. The cavity was lined with a false membrane of moderate consistence, a quarter of a line thick, and in contact with *healthy* pulmonary parenchyma. There were neither tubercles or gray granulations in the lungs; nor ulcerations in the larynx, trachea, or intestines. M. Louis nevertheless regards the excavation as tuberculous, because the purulent matter it contained and the false membrane lining it were perfectly similar to those of acknowledged tuberculous cavities, and because "one of the cervical glands was in part tuberculous." The last argument the author regards as not the least conclusive of the three: in this we coincide, if the matter was *really* tuberculous, but we cannot grant that, even with its aid, the tuberculous character of the cavity is fully made out. Its anatomical characters differed in no wise from those of an abscess; and the existence of a detached fragment of lung in its interior, a circumstance without a parallel, as far as we are aware, in the history of undoubted phthisical cavities, is not so uncommon a condition in simple purulent collections. The existence of the excavation at the apex might, on first consideration, appear to support the idea of the tuberculous nature of the lesion, but the fact is that abscess is more common in the superior than the inferior lobes. Thus, Grisolle (De la Pneumonie, p. 53,) found that of 19 pulmonary abscesses 9 were in the upper lobe, 5 in the inferior, 1 in the middle; and in four instances there were several purulent collections in different situations. Nor do the commemorative symptoms bear out M. Louis' view. The patient was admitted fifteen days after delivery, which had been natural and at the full time; but at the close of pregnancy, rigors followed by heat and sweating had supervened and ceased after parturition. Hemoptysis had never occurred.

The great additional experience acquired by the author since the publication of his first edition has not enabled him to alter the statement there made, of his never having observed in the midst of *healthy* pulmonary tissue cavities communicating with the bronchi and lined, like *old* tuberculous cavities, with a grayish, semi-cartilaginous, and incompletely opaque false membrane. He admits, however, that such appearances have actually been observed by Laennec and many of his successors, and considers the notion of their signifying the cure of phthisis confirmed by the circumstances of the case to which we have just made brief allusion. But we have shown the deficiency of proof of the tuberculous nature of the disease in that instance; and must observe that the experience of Louis, as above stated, goes to support, in the very strongest manner, the view which we believe a deep study of all the alleged cases of production of innocuous cavities from tuberculous destruction of the lung (without the presence of tuberculous disease in other forms,) will amply justify, namely,—that these cases are nothing more than examples of excavation following the evacuation of simple inflammatory collections of pus.

Nor has M. Louis during his researches, pursued through a period of twenty-five years, met with a single example of those masses of condensed cellular tissue in which bronchial tubes more or less dilated are alleged to terminate, and which Laennec regarded as cicatrices of tuberculous cavities. He is far, however, from denying either the reality of their occurrence or the justness of Laennec's views of their curative signification. A mode of cure, admitting it to be such, which does not exhibit

itself once in such an enormous number of cases as must have presented themselves to M. Louis during so lengthened a period, can, practically speaking, be little more than valueless.

The depressions frequently observed at the upper part of the lungs, along with corrugation of the surface around, appear to M. Louis to be unconnected with any determinate lesion; like all other pathologists he has seen them on lungs the substance of which was perfectly healthy.

The only addition we discover to the details descriptive of the bronchi, consists in the account of a "tuberculous false membrane," found in a single instance in the interior of one of the larger bronchi, and lining the tubes from place to place to the very periphery of the lung. The anatomical characters of this production are not given with much precision.

The readers of Dr. Carswell's fasciculi are well acquainted with his description and accompanying figure of atrophy of one lung, produced by the presence of tuberculized bronchial glands on its main bronchus. This condition observed in the monkey has not yet been noticed in the human subject. Dr. Carswell ascribes the atrophy to deficient entry of air into the vesicles, and the consequent functional inactivity of the organ. Possibly the position of the bronchial arteries is in that animal such as to expose them more readily than in the human subject to extraneous pressure; if so the atrophous shrinking of the lung may have, in some measure at least, depended upon this cause. M. Louis in commenting on the case, (which was originally seen, and has since been described by Reynaud,) notices the possibility of a similar condition of things arising in man, and points out the almost inevitable error which would be incurred in its diagnosis during life.

M. Louis conceives that the most important addition to the morbid anatomy of tuberculous disease, made since the publication of his former edition, is comprised in the results obtained by Schroeder Van der Kolk, and Guillot, respecting the vascular condition of the lungs. The Dutch author has in this matter all the merit due to its original investigator; but as the observations of M. Guillot are the more complete, these we shall condense. It is ascertainable that the branches of the pulmonary artery stop or cease to be permeable at a distance of three, four, or five millimeters from tubercles or gray granulations; the length of vessel impermeable increases with the augmentation of the tuberculous masses, so that when these are considerable, or when they have given place to cavities, a sort of investment, two centimeters thick, may be found around them, presenting not a single ramification of the pulmonary artery. By injection and microscopical examination, it is further discoverable that this total absence of vascularity is only temporary; after a time, red lines, tapering off at either end, and in their widest part equalling a millimeter in diameter become discernible. At first these vessels are perfectly isolated, but in process of time communicate with the bronchial arteries, or with those of the walls of the thorax. The latter communication is effected by means of new vessels developed in the pleural false membrane, (the particular discovery of Van der Kolk.) The amount of new vascularization effected in this way increases greatly with the progress of tuberculous destruction; the rete spreads eventually, it may be, through a great part of the affected lung, and replaces the system of the pulmonary artery which has ceased to be discoverable.

An enquiry naturally presents itself, as to the influence on oxygenation exercised by this novel condition of supplementary vessels. It is in truth aortic blood, that by means of the bronchial arteries and new system of vessels spreads through the lungs; and M. Guillot has ascertained that this blood must return to the heart by the bronchial, pulmonary, and azygos veins, as he ascertained that the substance of injection thrown in by the aorta is found in these veins. Now this condition of circulation is one that manifestly cannot subsist without materially altering the blood of phthisical subjects, and thereby affecting their organization generally. The main result in respect of function may be expressed thus,—that in proportion as tuberculization advances, the lungs acquire increasing capacity for arterial, and loose it for venous blood.

The researches of M. Guillot upon the elementary and primary seat of tuberculous matter, lead him to follow the views of Dr. Carswell, and place this in the bronchi as soon as the accumulation is sufficiently great to be distinctly and satisfactorily examined. In the earlier stages he thinks it a just inference from what is thus ascertained in more advanced ones, that the ultimate terminations of the bronchi are the seat of the change. M. Louis esteems the attempt to limit the lesion to a particular texture, a labour which can lead to no useful result,—an idea, as it appears to us, of most questionable wisdom.

The author invokes the authority of Dr. Carswell, (as employed by M. Valleix in an essay on the subject,) in support of his own objections to the notion of M. C. Baron, that tubercle originates in modification of extravasated blood. He observes that the researches of the English pathologist have placed beyond a doubt the existence of the gray semi-transparent granulation in perfectly rudimentary tubercles. M. Valleix has altogether mistaken and led M. Louis into a mistake respecting the opinions of Dr. Carswell. He is in truth one of those who most strenuously resist the notion of a necessary connexion between the gray granulation and yellow tubercle; and regards it in the lung as mere bronchial mucous, separated by filtration from the tuberculous matter, in company with which it has been accidentally secreted.

Pneumonia is far from uncommonly the ultimate phenomenon of phthisis; but there is nothing characteristic in this, the proportion of cases in which it occurs not being materially greater than in other chronic diseases taken indiscriminately. In other words, tuberculous disease does not *per se* exercise any special influence in generating the *final* pneumonia met with in a certain number of cases of phthisis,—hence it is rather a phenomenon evidential of the *chronic* than of the *tuberculous* character of the malady. Not so, however, of the pneumonia which frequently occurs as an intercurrent affection in phthisis; here the tuberculous influence is manifest.

The two pleuritic lesions which the researches of M. Louis mark as peculiar to phthisis are, "the sort of cartilaginous cap covering the apex of the lung when excavated," and tubercles developed in the false membranes of the pleura. With regard to the latter point, M. Andral states that "in *almost* all the cases where he has met with tubercles in the pleura, he has also detected them in the lungs;" from which, of course, the reader is to infer that he has actually seen some cases of tubercu-

lization limited to the pleura. Three years ago, (Brit. and For. Med. Rev., Vol. IX. p. 325, April, 1840,) we drew attention to a statement of similar purport by M. Fournet, and allowed our own doubts as to the really tuberculous nature of the alleged pleural tuberculization to be inferred. We have, since that period, met with some instances of the condition by which MM. Andral and Fournet have been betrayed into the bootless contradiction of one of the most important practical laws of pathology, and easily ascertained that the so-called tubercles were nothing more than common coagulable lymph, deposited in an irregularly granular form. M. Louis upon other grounds similarly denies the reality of tuberculization limited to the pleura.

The section devoted to the morbid anatomy of the larynx, trachea, and epiglottis is enriched by the analysis of a very large number of examinations of those organs made by the author since 1825. We shall throw these as far as possible into a tabular form, for the purpose of economising space.

No. of cases.		Ulcerations in Trachea.		
190	80 Females	21	=	$\frac{1}{4}$
	110 Males	55	=	$\frac{1}{2}$
		76	=	$\frac{1}{3}$ and upwards.

No. of cases.		Ulcerations in the Larynx.		
193	80 Females	19	=	$\frac{1}{4}$ about.
	113 Males	44	=	$\frac{1}{3}$,,
		63	=	$\frac{1}{3}$,,

No. of cases.		Ulcerations in the Epiglottis.		
1	47 Females	8	=	$\frac{1}{6}$
	87 Males	27	=	$\frac{1}{3}$ about
		35	=	$\frac{1}{4}$

No. of cases.		Ulcerations in the Bronchi.		
49	19 Females	5	=	$\frac{1}{4}$
	30 Males	17	=	more than $\frac{1}{2}$
		22	=	$\frac{1}{2}$ nearly.

These facts show the remarkable degree of frequency of ulcerations of the air-passages generally in this disease, the gradual increasing frequency with which they are discovered, as the parts examined are more and more deeply seated, and their comparative rarity in females. We are utterly at a loss to account for the influence of sex signified by the last-named peculiarity; that ulceration of the tubes should be more common in the immediate neighbourhood of cavities than higher up the air-passages, is nothing more than may be readily understood from the more protracted sojourn of the sputa in the former than in the latter situation,—at least, if we admit the justness of M. Louis' views as to the causation of ulcers by the irritation of these sputa. And there is, in truth, every motive for accepting those views as explanatory of the *general* fact of ulceration; however, M. Louis himself points out that ulcerations *may* exist in rare instances in the mucous membrane of the air-passages, without the presence of cavities. What their cause may be under such exceptional circumstances, it is not easy to determine, for

the laborious author has never in a single instance detected tubercles in the mucous membrane of the upper air-passages.

The ulcerations we have been now referring to, though never directly of tuberculous character, were nevertheless set down by M. Louis as peculiar to phthisis, because never met with except in subjects labouring under that disease: syphilitic ulcers are of course excluded from this statement. In upwards of 500 non-tuberculous subjects dying of various chronic diseases examined since the publication of the former edition, M. Louis did not find one example of laryngeal ulceration; it is therefore but just to conclude, (and the experience of every physician whose practice leads him to study these maladies seriously, will, we believe, harmonize with the conclusion,) that chronic laryngitis with ulceration, is but a part, a secondary lesion, of phthisis.

We discover no new statements in the chapter relating to the state of the heart; the author simply refers to the corroboration of his former results, respecting the diminished size of that organ in phthisis, derived from the minute investigations of Bizot. (Vide Brit. and For. Med. Rev., Vol. VI. p. 43.)

The pharynx and œsophagus were almost always in the natural state; in 120 subjects the former was ulcerated in but four cases,—the latter in six. The œsophagus was pretty frequently coated with pultaceous exudation,—a condition not productive of any particular symptom.

The size and position of the stomach undergo modification in phthisis with moderate frequency. Of 96 subjects in whom these points were carefully examined, 9 presented enlargement of the organ to double or treble its natural size, and in 6 of these cases the great curvature was on a level with the crista of the ileum. In all these instances the liver was enlarged and depressed also. Now in 230 subjects dying of various other (acute or chronic) diseases, these peculiarities were only encountered twice: M. Louis, then, regards them, when so highly marked as we have described, as almost proper to phthisis and probably induced by the constant efforts of cough.

The opinions formerly held by the author on the subject of "softening with attenuation of the mucous membrane of the stomach," were materially, if not completely modified, as the readers of this Journal are aware, in the last edition of his treatise on typhoid fever, (vid. Brit. and For. Med. Rev., Vol. XII. p. 25,) the essentially chemical and cadaveric nature of the appearances was then admitted, although it was very fairly urged that there are some circumstances connected with the conditions under which solution takes place, as yet undiscovered. The other kinds of anatomical change found in the *mucous membrane* in 96 cases carefully examined were as follows:

Attenuation	19
Redness, and sometimes thickening, mammillation and softening anteriorly	8
Softened, and of obscure red colour in the fundus	17
Mammillated, grayish, sometimes reddish, thickened, &c.	19
Ulcerated, without other lesion	2
Softened, without alteration of colour or thickness	4
Redness more or less florid over entire surface, without alteration in point of thickness or consistence	6
Viscid fluid underneath it	1
Presenting cicatrices	1
	77

These lesions are none of them peculiar to phthisis; the author found them in one half of the subjects he examined, dying of all other chronic diseases indiscriminately. However, as appears from the above table, they are much more frequent in phthisis, existing in four fifths of its victims. The presence of tuberculous matter either in the substance of, or underneath the mucous membrane here, is singularly rare: M. Louis has examined 400 stomachs without once detecting it, and M. Andral encountered it but twice in several hundred cases. This circumstance is dwelt upon by the author as indicative of the non-dependence of tuberculization on inflammation, for, as we have just seen, the stomach is very frequently the seat of the latter morbid state.

The author has found the proportion of cases of duodenal ulceration, (9 out of 60,) higher than that formerly announced. In one case he observed a few small sub-mucous tubercles. He makes no remark regarding the state of Brunner's glands; organs which, according to the statements of a recent German writer, are sometimes distinctly tuberculized.

The condition of the mucous membrane of the small intestine in its various changes, from the first deposition of tubercle in its systems of agminated glands to perforation of the bowel, is detailed with that graphic precision which constitutes one of the most remarkable features of all the author's writings. There are no novelties in the details, however, with a single exception to be immediately considered, and we therefore content ourselves with strongly recommending their minute and complete study. The point we desire to refer to here is the occasional occurrence of cicatrization in some one or more of the ulcerations of the small intestines. On first thought, such cicatrization might appear a fortunate circumstance for the individual in whom it occurred, but a little reflection upon the anatomical constitution of cicatrices of mucous membranes, and their influence on surrounding tissues, will lead us to doubt even the innocuousness of a cure of the kind. In truth the plastic exudation forming the staple material of cicatrix, is endowed with a property of contraction, the tendency of which must be to produce diminution of the caliber of the gut. And the observed fact is occasionally so; such contraction has arisen in not a few cases on record, and in one added to the number by M. Louis, the symptoms of obstructed bowel were so marked, and so pertinacious, (eventually they proved the cause of death,) that they diverted attention from the state of the lungs, which was not inquired into,—the pulmonary disturbances being masked by the intestinal. Here then we have a new—at least a newly-known—mode of death by tuberculous disease; and by the fact of its possibility, we are led to infer that it is better for phthisical subjects *not* to have their intestinal ulcerations cured, than run the risk of that cure causing their destruction. There is, of course, only a risk, not a certainty,—for contraction is not an unfailing attribute of intestinal cicatrization. The merit, however, (we have much satisfaction in adding,) of pointing out the danger of cicatrization of ulcers in the small intestine, does not belong to the French school; Dr. Carswell, years ago, observed and described in one of his Fasciculi this very condition, productive of the fatal issue of a case of ulcerative continued fever about twelve months after imperfect convalescence.

It is to be observed that ulceration of the small intestine is almost peculiar among chronic diseases to phthisis; at least 85 subjects dying of all other chronic affections presented but 6 examples of such ulceration, and in three of these six there were tubercles in the lungs.

Intestinal ulceration is frequently independent, originally, of inflammation. The development of the small tuberculous masses cannot itself be ascribed to inflammation, because the mucous membrane adjoining them is unaffected so long as they remain unsoftened.

M. Andral inquires, in the last edition of the Clinique Médicale, how the very common belief in the frequent coexistence of fistula in ano with phthisis has arisen,—he himself having only ascertained its existence once in 800 tuberculous subjects? M. Louis states that he has not been more successful than his countryman in the search after fistula, but gives no numerical statement of his experience. This appears the more remarkable as the frequency of ulceration in the large intestine is so great that M. Louis found this change more or less extensively developed in 88 cases out of 108 cases. We have long been satisfied, from the repeated interrogation of tuberculous subjects regarding the state of their anus, that the common opinion is absurdly exaggerated, and that probably fistula in ano is not more common among them than in all other kinds of unhealthy individuals taken indiscriminately; but, on the other hand, we are firmly persuaded that, in this country at least, fistula is more common than M. Andral's experience would lead us to suppose. We have ourselves met with at least 4 cases in about 200 tuberculous subjects, observed in hospital within the last twelve months; and we should think that the proportion witnessed by us, in private practice (although we speak without positive records,) must be considerably greater than this.

The chapter relative to the lymphatic glands is very considerably enlarged in the present edition. The frequency of tuberculization of these glands in the different regions of the body is shown by the subjoined table:

Lymphatic Glands.	No. of cases examined.	Tuberculous in
Cervical . .	80 ...	8 = $\frac{1}{10}$
Bronchial . . .	70 ...	$\frac{1}{2}$*
Mesenteric . .	102 ...	23 = $\frac{1}{4}$
Meso-cecal and meso-colic	"a little less frequently than the mesenteric."	
Lumbar . .	60 ...	5 = $\frac{1}{12}$
Axillary . . .	...	1

Tuberculous enlargement of the cervical glands does not commonly induce notable symptoms; in one case, observed in the infant by M. Tonnelé, the vena cava superior was pressed upon by a tumour of this kind, and a stasis of the blood, extending to the jugular veins and sinuses of the brain, entailed.

* There is a typographical error in the original, either in the number of cases or the proportional frequency; the context renders it much the more probable if not actually certain that it is in the former, we therefore give the latter. It is to be observed, with respect to the state of the bronchial glands, (and, indeed, the circumstance must invariably be borne in mind throughout this article,) that the researches of M. Louis refer to the disease as it exists in subjects aged upwards of 15—younger individuals being excluded from the hospitals in which he observed. Tuberculization of the bronchial glands is in infancy more frequent even than that of the lungs.

Although bronchial tuberculization is much less common in the adult than infant, yet it is a sufficiently interesting condition; and one of the peculiarities attending its progress in infancy have been well made out by MM. Rilliet and Barthez. The bronchial glands are generally invested by a cyst with thin walls, to which the tuberculous matter closely adheres. At a certain stage of advancement the cysts contract adhesions with adjoining parts, much more frequently with the bronchi than with the pulmonary substance or vessels. After this union takes place the softening of the tuberculous matter generally follows, and subsequently the establishment of communication between the tuberculous cyst and the bronchi. At this period the cyst becomes lined with a red false membrane, intimately united with the mucous coat of the bronchi. In 26 cases such communication of tuberculized glands and the air-passages was discovered 18 times; 12 times on the right side, 5 on the left. Pneumo-thorax (without corresponding symptoms, however,) followed in one case of perforation of the cyst. M. Louis appears to regard these observations as completely new; the truth is, that Dr. Carswell, years ago, figured a case (Fasciculus, TUBERCLE,) illustrating the whole process of evacuation of altered tuberculous matter through the trachea and bronchus, from the interior of the tuberculized bronchial glands; and exhibiting the condition of cicatrized trachea which sometimes follows, and which, from the details of our author, appears not to have attracted the attention of the French observers. M. Berton once saw perforations of the pulmonary artery similarly produced.

Those who are acquainted with the arguments used by Dr. Carswell in support of his notion of the independence of the gray granulation in the lung and yellow tubercle, are aware that he (and others who adopt his views) refers to the total and invariable absence of the gray semi-transparent matter in the lymphatic glands as a point of very material importance in their favour. In all M. Louis' cases of tuberculous disease of these glands, it would appear that but two instances occurred of such development; but these are enough to show that the position of Dr. Carswell is untenable. And we are persuaded that the relative frequency of gray matter in the mesenteric glands is sometimes greater than the statements of M. Louis would signify: we have detected it in four or at least three (for one instance might be cavilled against) cases out of some twenty post-mortem examinations, in which the state of those glands was closely examined.

Tuberculization of the lymphatic glands never occurs, according to M. Louis (and we believe him of *true* tuberculization,) after the age of fifteen, unless in phthisical subjects.

The peculiar alteration of the liver which M. Louis' previous volume contributed especially to make known, under the name of "fatty transformation," is extremely frequent in subjects dying of phthisis in France. Forty out of 120 individuals presented this condition of liver. The organ, when thus affected, is of faded-leaf tint, enlarged, but unaltered in general shape; of diminished specific gravity, and capable of greasing bodies brought in contact with it. The lesion is four times as frequent in females as in males, and appears to be almost peculiar to phthisical persons. The transformation may be very rapidly effected, according to M. Louis, because he has discovered it in cases of phthisis which had run

their course in fifty days; herein the author assumes its dependence upon and the impossibility of its existing without phthisis, as granted. It produces no symptoms of appreciable character, and the author regards enlargement of the liver (as discoverable by protrusion of the organ beyond the free border of the ribs) as the only circumstance capable of leading to its diagnosis during life. But this protrusion is insufficient in itself to prove the fact of enlargement; it may announce depression only. And it is to be observed that fatty liver is much more rare in this country than in France,—a circumstance which we are at a loss to explain. Indeed, the whole etiology of the condition is infinitely obscure.

Four subjects only had biliary calculi; in one of these cases the usual symptoms had existed during life, in the three others they gave rise to no inconvenience.

The kidneys were perfectly natural in respect of consistence, colour, and size in three fourths of the cases. In 16 out of 90 cases they were slightly redder than natural; in 3, of much greater consistence than usual; in 4 subjects they contained small serous cysts; in 3 instances were the seat of tuberculous matter, speading in one of these into the corresponding ureter. This unity of tubercular disease in the kidney is confirmed by the author's subsequent experience, for 80 post-mortem examinations supplied but two examples of it. Rayer's statement would lead us to suppose the condition of more frequent occurrence than these results of M. Louis warrant. In a single instance among 214 post-mortem examinations the author found the kidneys in a state of fatty transformation. And in nearly 500 subjects dying of all other diseases except phthisis, not a single example of tuberculous or fatty affection of the kidney occurred.

The penis presented nothing remarkable in the few instances in which it was submitted to examination : in 40 subjects in which the prostate, vesiculæ seminales, and vasa deferentia were closely dissected, these parts were all three found tuberculous once and the prostate alone twice. M. Louis has never seen the urethra tuberculous; Rayer, however, quotes two cases of the kind.

Morbid change is rare in the genital organs of female tuberculous subjects; the size of the uterus is, however, commonly below the healthy standard. As in persons dying of other affections, the author found uterine polypi in a certain number of cases (we regret he has not specified the proportion, for no slight difference of statement exists in different works upon the point.) In one case referred to in the former edition, the internal surface of the body and neck of the uterus was invested with a stratum of tuberculous matter, and the tissue of the organ, otherwise healthy, studded with minute yellow tubercles. Among more than 200 uteri of phthisical women examined by the author since 1825, three examples only of tuberculous disease were discovered. The fallopian tubes were, in a case described by Reynaud, crammed with the morbid matter. Ulceration of the vagina appears only to occur when the tuberculous substance escapes from the organ and is found along that canal,— a mode of ulceration analogous to that already described in the trachea. These various conditions have been delineated by Dr. Carswell.

The importance, both pathologically and practically, of the chronic peritonitis of phthisis is well established by the chapter on lesions of the

peritoneum. In the cases where that affection existed the other organs of the body were, comparatively speaking,—this is true even of the lungs,—in an incipient state of disease; hence the peritonitis evidently had a considerable share in hastening the fatal event. This view is confirmed by the circumstance that in several of these cases the disease ran its course very rapidly,—in one instance in forty-four days. The development of this peritonitis is not fairly attributable either to the influence of sub-peritoneal tubercles or to that of intestinal ulcerations, for in several cases these lesions were altogether wanting, or were present to a very limited amount. The author concludes the action of some special cause (as in the case of various of the other secondary lesions of phthisis,) aided by the violence and constancy of febrile action. Whatever the circumstances are which favour the generation of chronic tuberculous peritonitis, they influence the pleura also: for of 13 cases of the former, 4 were attended with tuberculous pleuritis. It is tolerably well known that, in his former edition, M. Louis maintained chronic tuberculous peritonitis to be peculiar to phthisis, as he had never seen the state in non-consumptive subjects. All his subsequent experience corroborates the announcement then made; among upwards of 300 individuals dying of other chronic diseases besides phthisis, not one presented the anatomical characters of tuberculous peritonitis.

Although the functions of the brain are almost always unaltered in phthisis even to the last hours of life, yet some lesion or other of that viscus or its membranes is discoverable after death in a considerable number of cases. The cerebral arachnoid was frequently thickened partially, and contained non-tuberculous granulations in greater or less numbers, especially in the neighbourhood of the falx. It was invested in two cases by a soft yellowish false membrane. The sub-arachnoid cellular tissue was infiltrated with serosity, and the lateral ventricles distended with the same fluid in three fourths of the cases. In a certain number of instances the sub-arachnoid tissue contained (especially in the fissure of Sylvius) a variable quantity of semi-transparent or tuberculous granulations. These granulations, which form the anatomical character of tuberculous meningitis in infancy, (at which period of life they are sufficiently common) have scarcely been submitted to the fitting study in the adult. The inquiries of M. Lediberder have merely excited interest in the matter without establishing the facts connected with it. In a seventh part of M. Louis' cases the brain was more or less injected; in the twentieth part of them, softened throughout its entire mass, once very remarkably so. It seldom contained tubercles.

The general result of all the author's experience since 1825 is to confirm the remarkable and well-known statement then made by him to the effect that after the age of fifteen tuberculous matter never presents itself in any tissue or organ unless it exists also in the lungs. To the general law he admits he has observed one, and refers to the existence of a second, exception; but the exterior paucity of such exceptional occurrences places in most prominent relief the importance of the author's discovery. And we may add that having become acquainted with the law in 1833, we have not ourselves either seen or heard of a single authentic case falsifying its provisions.

II. Symptomatology, &c. Too much stress can scarcely be laid upon the importance of acquiring an accurate notion of the mean duration of phthisis, and yet no task of greater difficulty probably can be assigned the physician. He who has practically and, at the same time, conscientiously endeavoured to ascertain the period of origin of the disease in a considerable number of cases, will fully bear us out, we doubt not, in this view of the difficulty of the attempt. We are indeed persuaded from our own experience that, especially among patients of the class frequenting hospitals, it is not seldom a matter of impossibility to satisfy one's self as to the accuracy of the averments made respecting the outset of disease. And it is for this reason that we are gratified to find M. Louis speaking of his results as drawn from cases in which the duration of the disease was ascertained "with all the precision possible," and not dogmatically presenting them as positively and absolutely accurate. It is besides obvious that the *real* duration of the malady from the first hour of anatomical change can never—no more than in the instance of cancer, for example—be accurately made out; all we can hope to arrive at by the rejection of doubtful cases and the collection of a vast number of facts, is the mean duration of the disease after it has given symptomatic evidence of its existence,—in other words, its average *apparent* duration. The latter is fortunately that which the practical observer has the closest interest in the establishment of.

In the former edition of his work M. Louis gave the duration of 114 cases; in the present he adds that of 193 more. We shall place together these two series of results:

Duration.	No. of cases.	Duration.	No. of cases.
22 days	1	10½ months	1
24 „	2	11 „	10
30 „	1	12 „	13
35 „	2	12½ „	3
39 „	2	13 „	2
40 „	1	13½ „	2
45 „	1	14 „	0
50 „	2	14½ „	2
52 „	1	15 to 16	10
60 „	1	17 to 18	7
70 „	1	19 „	5
75 „	1	20 „	4
80 „	1	21 „	2
81 „	1	23 „	3
84 „	1	24 „	18
3 months	7	28 „	1
3½ „	5	30 „	3
4 „	17	36 „	9
4½ „	2	39 „	1
5 „	19	48 „	11
5½ „	4	60 „	6
6 „	25	66 „	1
6½ „	2	72 „	4
7 „	16	84 „	1
7½ „	6	90 „	1
8 „	17	10 years	1
8½ „	1	12 „	2
9 „	20	14 „	1
9½ „	2	20 „	1
10 „	10		

Hence it follows that out of a mass of 307 patients 4 died within the first month, 14 within two months, 26 within the first three months, 98 or about one third of the whole number, within the first six months;—facts sufficiently demonstrative of the rapid progress of the disease in a large proportion of cases. In two years 264 of the total number are gone, leaving only 43 to drag on their weary existence. The author has not calculated the mean duration of the malady from his individual facts: we find it to be from seventeen to eighteen months, taking the entire mass of cases; but if the five cases in which the disease lasted ten years or upwards be omitted from the calculation, the average duration is reduced to from fourteen to fifteen months. It is to be observed that these are calculations of apparent duration, as established by means of symptoms,—the real duration is possibly *materially* greater.

The series of cases given in the present edition furnishes a lower mean than that of its predecessor; seven years and a half is the longest duration noted in the former,—all the instances of a protracted course of ten, twelve, fourteen, and twenty years are extracted from the first.

The mortality produced by phthisis, in the wards of M. Chomel, where the author's first collection of cases was taken, was really enormous, being in the proportion of one to two to the total amount of deaths. And if to the number of persons actually dying from phthisis be added that of subjects who, perishing of other affections, had nevertheless tubercles or tuberculous cavities in the lungs (forty persons appeared in this category,) we have a total mass of 163 tuberculous deaths (if the expression be admissible) out of 358 occurring in the wards of La Charité. There is too much reason to believe that the proportion would be found equally high among ourselves, had we some physician, possessing the devotion and the opportunities of M. Louis, ready to investigate the question on the same principles in the wards of our hospitals. It is obvious that the questions borne upon by such investigation are of a totally different character from those answered by the invaluable reports issued by the Registrar-general, and illustrated by Mr. Farr.

The consideration of the duration of the disease is followed by a particular description of each symptom occurring during its progress: these descriptions will furnish us from time to time with material for comment.

The author states, on the evidence of cases recorded in the work, that decumbiture on the side of the principal excavation was insupportable from the increase of cough it entailed; to such a degree that, independently of more certain means, the decumbiture of the patient will aid the observer in discovering the side at which the disease is most advanced. Generally speaking this is true, but there are other circumstances which influence the patient in choosing his posture; and where a very closely similar amount of disease has been found in both lungs we have known the patient steadily maintain one position.

The opaque, globular, striated, ragged sputum, containing no air, is justly regarded by the author as extremely significant of the disease; nevertheless he has in two instances seen this condition of expectoration independently of tuberculous affection; and in three cases of phthisis, on the other hand, the sputa remained mucous, aerated, whitish or slightly yellowish or grayish, and semi-transparent to the end.

The variation in respect of quantity of expectoration, both in different

cases and in the same case, at different times is well known to be remarkable. We have ourselves observed instances in which a total suspension of expectoration persisted for several days at a time, and this without any obvious coexisting improvement in other *symptoms*,—the phthisical *signs* were of course altered. The author refers to one case, indeed, which ran to the end without the patient having spit at all; though the lungs contained large cavities. Such a fact as this appears inexplicable except on the supposition that, as is almost universally the case with children, the patient, in this instance an adult, swallowed her sputa; nevertheless this view of the case is, on the other hand, not without its difficulties. We have seen one such case; the subject of it was a boy, aged fourteen. Sir James Clark, too, states that he has met with one example of total absence of expectoration. It is important that the practitioner should be aware of the reality of such cases.

Occasionally during the second period of the disease patients void an enormous quantity of puriform-looking sputa, which cannot be ascribed to the sudden evacuation of a large mass of softened tuberculous matter. When effusion into the pleura has coexisted with the parenchymatous disease in such cases, the abundant expectoration has commonly been ascribed to escape of the pleural fluid through a perforation of the lung, but this is by no means a necessary occurrence or an invariably just explanation. The coincidence referred to, was observed by M. Louis about a year since at the Hôpital Beaujon; here the subject, who had suddenly voided an enormous quantity of puriform matter, had not presented the symptoms of perforation, nor had the level of the effusion fallen after the copious expectoration, which must inevitably have been the case had that effusion been its source. M. Louis concludes that a *sudden and temporary increase in the secretion of the bronchi and cavities* is capable of producing the occurrence in question.

The expectoration of calcareous particles from the lungs of tuberculous subjects is not by any means so common, as might be anticipated. M. Louis has never met, either in hospital or private practice, with a single example of the fact. We have ourselves, in a certain number of instances, been assured by patients, and this very circumstantially, that they had expectorated such matter, but we are little disposed to confide in accounts of the kind, and we have *seen* but one example (the specimen is before us) of the occurrence.

The general history of the hemoptysis of phthisis is perfectly well known; two or three points regarding it, however, deserve particular notice. The common belief of the frequency with which hemoptysis proves the immediate cause of death in phthisis would appear, from M. Louis' experience, to be an incorrect one; at least no such case had presented itself to him at the period his first edition was published, and out of three hundred phthisical persons since observed three only perished of hemoptysis. We cannot help believing that this proportion is somewhat below the ordinary standard; however, it being admitted that hemoptysis may prove the cause of death in some cases and may also be the first symptom of the disease, M. Louis infers that this discharge of blood might actually be at one and the same moment the *first* and *last* evidence of phthisis. We have never heard of a case of the kind actually occurring.

The author reiterates the statement made in his former edition as to the *almost* invariable dependence of hemoptysis, if at all considerable, upon tuberculization of the lungs, except in certain cases where the bleeding has followed external violence or suppression of the catamenia. Our own experience confirms the accuracy of M. Louis on this point, while it testifies to the error of Laennec, in regarding pulmonary apoplexy as the chief cause of abundant hemoptysis. Often are the anatomical characters of apoplexy of the lung obvious in the dead subject, when there has been no hemoptysis during life; in the epidemic yellow fever of Gibraltar the author ascertained that the pulmonary lesion in question was very common, while not a single individual had spit blood at the time.

The marked influence of sex on hemoptysis was exhibited in the former edition of this work; the statements there made, that of forty-two women carefully interrogated thirty-six had suffered from this symptom, while twenty-one only out of thirty-eight males had spit blood, does not, however, receive the corroboration of additional cases. It were much to be wished that the point had undergone investigation in this country, as we cannot divest ourselves of an impression that here, at least, the male sex suffers more frequently than M. Louis' figures would lead us to believe. It is curious, as mentioned by the author, and as general experience witnesses, that hemoptysis occurs with extreme rarity in phthisical *children.* The more special tendency of the malady to implicate the bronchial glands may in part account for the fact, but certainly not fully explain it; of nearly two hundred tuberculous children observed by M. Guesnard (Thèse, p. 12), two only had had hemoptysis, little girls aged nine and eleven years.

M. Louis notes the infrequency with which, comparatively speaking, any immediate cause for the escape of blood can be ascertained; our own experience upon this point coincides with the author's. Nor is the actual anatomical mechanism (if we may be allowed the expression) of the hemoptysis more readily traceable, and if before the formation of cavities we are constrained to refer it to exhalation, so too even after the establishment of ulceration must we provisionally recognize them as its common source, so singularly rare is it to detect a ruptured vessel in the interior of the lung.

The author insists upon the fact that night perspiration was not influenced, in respect of violence or persistence, by the state of other organs or tissues besides the skin; that, for example, the supervention of diarrhea, however copious, had no effect in diminishing the abundance of cutaneous exhalation. M. Louis has, in a word, seen no reason for accepting, as a general truth, the doctrine of "balance of the functions," taught by various systematic writers; the *occasional* cessation of perspiration while the intestinal flux acquires redoubled violence, he conceives himself justified in regarding as a mere coincidence. And he is desirous of impressing these results upon practitioners, as the treatment pursued by not a few persons is founded upon a very deep-rooted conviction of the reality of this fancied "balance of the functions."

The conditions of the tongue in phthisis are well known to be extremely variable; the author gives the results of comparison of its state with that of the mucous membrane of the stomach in a considerable number of cases. These results may be expressed as follows:

(*a.*) *In* 19 *cases of softening with attenuation of the mucous membrane of the stomach:*

Tongue natural in 9 cases; in the remaining 10, red at the tip or edges; in 4 of them for 15 or 20 days; in 6, for 2 or 3 days only.

(*b.*) *In* 8 *cases of inflammation limited to anterior surface of the stomach:*

Tongue pale or red an equal number of times; and in one instance the redness was quite temporary.

(*c.*) *In* 17 *cases of inflammation affecting the whole or part of the fundus:*

Tongue natural in 10 cases; in 7, slightly red at the edges, either towards the close of life, or for a few days at an earlier period.

(*d.*) *In* 19 *cases of mammillation more or less general and marked:*

Tongue of a more or less florid red colour for a variable time in 8 cases; natural in 11.

(*e.*) *In* 14 *cases of various associated lesions of the stomach:*

Tongue unnaturally red for one or several weeks in 6 cases; natural (it is to be inferred) in the rest.

(*f.*) *In* 19 *cases in which the mucous membrane was perfectly healthy in respect of colour, consistence, and thickness:*

Tongue more or less red in 10 cases; in 1 of these the redness persisted through the whole course of the disease, and was attended at one period with dryness; in the remaining 9 (it is to be inferred), natural.

These results indicate a degree of independence of the conditions of the tongue and stomach, for which the general statements of writers, especially of the Broussaisian school, ill prepare the reader; the truths they display are of deep practical importance. Another condition of the tongue occurring in one eighth of the subjects,—albuminous exudation on its surface,—is of more rare occurrence in this country than in France, as far as our own experience goes. The author has in a similar manner established its want of connexion with any particular state of the stomach. When it does occur, it is a phenomenon of singularly bad augury, as it rarely appears until a few days before death.

Of 112 patients, 5 only escaped diarrhea. In the eighth part of the cases it set in with the main disease itself, and persisted till the fatal termination, lasting thus from five to twelve months. Some individuals, whose malady lasted four or five years, laboured under almost continual diarrhea during that long space of time; in the greater number of cases, diarrhea did not supervene till the principal affection had reached the second half of its course, in some instances not till the closing days of life. The author then regards diarrhea as belonging to the last period of existence, or as of protracted existence, and inquires into the anatomical conditions attending both varieties. Ulcerations of the small or large intestine, or both, with (in four fifths of the subjects) pulpy softening of the mucous membrane of the colon, were discovered in the first class of cases,—all these lesions being evidently of recent origin, and the ulcerations small. Protracted diarrhea was either continuous or remittent.

The subjects affected with the latter form of the symptom, presented lesions not materially differing from those existing in patients whose diarrhea commenced but a few days only before their death; hence the inference, that in the present class of cases, visible lesions were but a small measure of the cause of the diarrhea,—that these lesions have too only made their appearance within the last few days of existence, and that previously to their occurrence, altered secretion was the true source of the intestinal discharge. Lastly, when the diarrhea had been protracted and continuous, and at the same time the stools numerous, vast and numerous ulcerations were discovered in the small and large intestine.

The symptoms of chronic peritonitis, it is observed by the author in a chapter making its first appearance in the present edition, are generally of moderate intensity, few in number, and often pass unperceived; nevertheless they are sufficient to announce with certainty the lesion to which they appertain. The description given of these symptoms may be condensed as follows: At a variable period of the principal affection, sometimes even at its outset, whether it be destined to destroy life in less than two months or in several years, the patient is annoyed with the first symptom of this peritonitis, enlarged size of the abdomen, or slight and sometimes general abdominal pain: both these phenomena may coexist. The pain is increased by pressure and percussion, and independent of diarrhea which is not always present, and when it is, is attended with suffering of a very different character from that of peritonitis. Subsequently fluctuation, or the evidence of gaseous distension may be detected. After having increased for a certain time, the sensation of fluctuation diminishes, and even disappears altogether, while the gaseous distension remains. In certain cases, where the accumulation of the gas occurs at the outset, without appreciable effusion, it diminishes after a time, and the abdomen becomes firmly resisting under pressure, and the outline of masses of intestine distinctly marked on the surface. Nausea and vomiting are rare, unless towards the close of life, as results of superadded *acute* peritonitis. The intensity of the symptoms may be such, that the patient's attention is wholly occupied with them; in other cases, where the organic changes are quite as advanced and extensive, the abdomen continues free from pain, and is simply enlarged and the seat of fluctuation,—the urine not being albuminous, nor the patient exhibiting symptoms of organic disease of the liver. The abdominal symptoms generally persist with a greater or less amount of intensity till the fatal termination; but cases of extremely chronic character are not wanting, in which after more or less protracted abdominal suffering, this ceases altogether, and the chest becomes the sole seat of functional disturbance.

The abdominal effusion may be very rapidly absorbed,—in seven or eight days for example; and the physician is sometimes not a little astonished to find a state of universal adhesion of the intestines through recent false membrane, in subjects who during life, had a few days before presented obvious signs of fluctuation.

In the combination of symptoms now enumerated, the author places the highest confidence, as diagnostic of peritonitis peculiar to subjects labouring under pulmonary tuberculization; through these symptoms he has been led to the diagnosis of the latter affection, in persons "who

did not cough at all or did so but very slightly, and whose chest examined with extreme care, with the aid of percussion and auscultation, presented no obvious sign of disease."

Cancerous peritonitis, however, runs the same chronic course as the tuberculous variety; hence the possibility of error. M. Louis points out the less amount of fever, and the absence of sweating and diarrhea, as circumstances distinguishing the former; cancer will also commonly be found in some other organ, and the age at which this disease is usually observed differs, as is well-known, from that of subjects affected with tubercles.

The symptoms appertaining severally to ulceration of the epiglottis, larynx, and trachea, receive very close investigation at the author's hands. The particular cases related justify the general inference, that fixed pain at the upper part of or immediately above the thyroid cartilage, difficulty of deglutition and escape of drinks through the nose, announces (provided the pharynx and tonsils be perfectly healthy,) the existence of ulceration of the epiglottis; but when limited in amount, this anatomical change was often latent. Moderate pain of limited duration in the region of the larynx, coupled with more or less marked alteration of voice, signifies the existence of superficial ulceration of that organ; whereas severe and continuous pain with persistent aphonia, results from deep ulceration. But in the case of tracheal ulcerations, no matter how numerous these may be, no special symptom is commonly discoverable; in a single instance only, observed by the author, a feeling of slight heat and obstruction behind the upper part of the sternum, was noticed as the effect of tracheal ulceration, — but here the mucous membrane was most extensively destroyed. Simple inflammation of the trachea, characterized by very bright redness, with, in some instances, thickening or softening, produced sometimes more or less sharp pain, with a sensation of heat along the neck.

The statements originally made by M. Louis respecting the course and influence of pneumonia occurring in phthisical subjects, inconsistent as it has been maintained they are with general experience, are repeated with greater emphasis even in the present edition. When pneumonia makes its appearance in phthisical subjects who are still able to pursue their occupations, and whose strength and flesh have not yet diminished materially, the affection presents the ordinary series of symptoms characterizing it in previously healthy persons; but these symptoms are generally not severe, and the disease "almost always terminated by cure, even when there were tuberculous cavities at the apices of the lungs." That is, an affection of the lungs which when those organs have, like the rest of the system, been previously perfectly sound, cuts off from one third to one seventh of those attacked, becomes almost harmless when they are partially destroyed by the worst form of malady prone to affect them. M. Louis adds: "as if the cavities and tubercles, real foreign bodies in respect of those organs, were the principal existing cause of the inflammation, and necessarily, *for this reason*, diminished its peril." This argument does not appear to us either very valid or very clear. The fact has however, as we had occasion to observe in a recent volume, (Brit. and For. Med. Rev., vol. XIII. p. 382.) been corroborated by the result of M. Grisolle's inquiries on the subject. Pneumonia, on

the other hand, "occurring at the close of the disease is almost necessarily fatal;" again, a proposition not very lucidly expressed. When supervening at this advanced period, and when of limited extent, it was not usually announced by any symptom; when implicating a considerable mass of lung, the ordinary symptoms, at least very closely these, are developed.

The experience of M. Louis testifies as strongly to the incurability of pleurisy occurring in the course of tuberculous disease, as to the curability of pneumonia: the former affection he believes an active agent in causing, or at least hastening the fatal event in many cases. But these propositions refer only to pleurisy with effusion,—of course, the dry pleurisy so repeatedly declaring itself during the process of the primary disease, cannot be regarded with such apprehension. Pleurisy with effusion, existing simultaneously on both sides of the chest, the author has never seen except in phthisis and in cases of gangrene of both lungs.

The introduction of a section on the state of the genital organs in both sexes affords M. Louis the opportunity of contradicting the statement, which had once obtained some vogue, that the sexual passion is increased in intensity during the course of phthisis in the male. Careful questioning has proved the very reverse to be the fact; and we have ourselves come to the same conclusion from repeated inquiries.

One female only, of all those observed by M. Louis, continued to menstruate to the last month of life. The period at which suppression occurs is subject to variation; when the disease lasted less than a year, the menstrual discharge ceased to appear on an average at about the middle period of the affection; if from one to three years, the catamenia continued until the last third. When the disease run a rapid course, the cessation of the menses appeared to coincide with the establishment of fever; when the former was of slow type, the author failed in detecting any cause either retarding or accelerating the time of alteration in the catamenia.— Upon the alleged influence of pregnancy in arresting the progress of tuberculous disease, M. Louis is unable to speak with any confidence from his own experience. He points out, however, some easy sources of error in estimating such influence; it is quite possible, for instance, that many of the symptoms of phthisis may be more obscure during pregnancy than when the uterus is empty, although the disease were advancing as rapidly as ever. Nor is it impossible that the progress of the affection might be more rapid after delivery, than at an earlier period of its course. Again, the extraordinary variety which different cases of phthisis present in respect of their progress, points to the necessity of accumulating a vast number of cases, in which the pregnancy has *appeared* to have the alleged influence, before its *reality* be admitted.

We have had occasion more than once to draw the attention of the readers of this Journal to the anatomical and other characters of that peculiar form of meningitis of children, to which the names of granular and tuberculous have been applied by its describers, Drs. Rufz, Gerhard, and P. H. Green. These inquirers appear, however, to have limited their observations to the young subject, and the thesis of M. Lediberder, with the new section introduced into the present edition of M. Louis'

volume, are the only sources from which acquaintance with the affection, as it appears in the adult, may be derived. The malady, occurring at various stages of the primary disease, commences with frontal, intense and continuous cephalagia; the face at the same time becomes alterternately pale and red; the intellect grows blunted; paralytic symptoms are rare at this period; vomiting almost constantly occurs at the very outset, and its association with cephalalgia is regarded by the author as of itself strongly indicative of the presence of tubercles in the meninges. The violent headach persists for from three to twelve days, accompanied with sharp cries from time to time: the face assumes an expression of astonishment, quickly followed by one of stupor. The pupils contracted at first, become subsequently dilated. From the fourth to the sixth day delirium, commonly of a tranquil type, sometimes accompanied with agitation and increase of the general sensibility, appears. Somnolence, and eventually coma, are noticed in the intervals of freedom from delirium. When hemiplegia exists, it generally makes its appearance some days later than the cephalalgia; some considerable part of the face, or one of the eyelids only may be paralysed; and persistent contraction is witnessed instead of paralysis in certain cases.

Meanwhile the circulation and respiration undergo remarkable changes. The respiration becomes less frequent and less deep, dyspnea diminishes or disappears, except during the closing days, when on the contrary it undergoes material increase, proportional in amount to the coexisting somnolence. Even in cases where the lungs are extensively excavated, the fever disappears during the earlier periods of the meningeal affections; but it returns with intensity towards the close; irregularity of the pulse is very rare. The temperature of the skin falls and rises with the alterations of the pulse.

The duration of the disease varies between eight and fifteen days; rarely is it longer or shorter than these periods.

Three cases reported by M. Lediberder are added, confirming the details of the general description now given; the anatomical account of the meningeal disease, as we have already hinted, is not so complete as desirable, but the cases are nevertheless well deserving of deep study. If these cases show that the diagnosis of the affection may often be easily accomplished, and that it may actually be of use, (like the allied state tuberculous peritonitis,) in leading to the detection of the primary and main disease that of the lungs, there are others in which its precise nature may be involved in obscurity. A fourth case, exhibiting the strong similarity which the symptoms, both in respect of nature and catenation, may wear to those of typhoid fever, is added and will attract the attention of readers by the abundance and accuracy of its details.

Impaired power of hearing or actual deafness is occasionally observed in tuberculous subjects; and are traceable, according to M. Ménière, to the development of tubercle in the substance of the membrane of the tympanum, leading to its more or less complete destruction.

Emaciation, one of the most striking accompaniments of phthisis, set in with the commencement of the affection in one half of M. Louis' cases; in a small number of patients it commenced at the same time as diarrhea, or diminished appetite; in the third part of the cases it was

first noticed along with the development of fever. The author justly insists upon the importance of the symptom, and upon its diagnostic utility in all cases, but especially in those of the latent type.

The author next turns to the history of perforation of the lung. Many of our readers are aware that, to the accurate observations of M. Louis, physicians are indebted for the earliest precise account of this occurrence, in regard of its intimate nature, its immediate symptoms, and its eventual effects. But M. Louis and his countrymen fell into the error of imagining that perforation, (with its consequence pneumo hydrothorax) was necessarily mortal, ("mortel de sa nature," Ed. 1ère, p. 488); an inaccuracy exposed by Dr. Houghton, Dr. Stokes, Dr. Barlow, and others, in this country. Indeed cases have been narrated by some of these latter observers, (vid. Brit. and For. Med. Rev., Vol. IX. p. 394,) which seem to warrant the entertainment of the idea, that perforation may be the means of prolonging life, and arresting *pro tempore* the constitutional symptoms of phthisis. For an examination of the grounds upon which this idea may be advocated, we refer to our earlier volume just quoted, and shall only say here, that M. Louis refers in the present edition to cases where life was very considerably prolonged, after the presumedly fatal occurrence, while he makes no allusion to the statements of our countrymen on the subject, with whom all the merit of original observation of the fact exclusively resides.

In illustration of the extreme variableness of the period of the primary disease, at which perforation of the pleura may occur, the author relates with much minuteness the history of a female, aged twenty-three, in whom the first evidences of phthisis had existed for fifteen days only, at the time the serous membrane underwent rupture. This, however, is an extreme instance,—one which stands alone among all the cases ob served by the author in a period of more than twenty years. Nevertheless it is a notorious fact, that although perforation does not occur in the ordinary course of things until an advanced stage of the disease, it does not very unfrequently supervene at a comparatively early period. Pleural perforation appears to be about double as frequent on the left as on the right side; of 50 cases, (by the author 10, M. Reynaud 40,) the rupture occurred in 33 on the left side, in 17 on the right.

The "march" or course of phthisis, is one of the points of its history upon which the author's labours have been most striking. He considers the subject under the three heads of acute phthisis; chronic phthisis terminating by sudden death, explicable or not by the condition of the organs; and latent phthisis. The general description of acute phthisis is founded on thirteen cases. In *three* of these, running a course of twenty, twenty-nine, and thirty-four days, the character of the symptoms was peculiar. In the midst of health these individuals were seized with rigors and more or less violent trembling, renewed on the following days. Heat followed those rigors, of a persistent kind, and as intense as in typhoid fever. From the outset, too, extreme thirst, complete anorexia, frequency of respiration, marked oppression, cough, (except in one instance, where it did not supervene till the tenth day,) and expectoration moderate in quantity were present; and whenever the patients were first observed, they were found in a state of fever.

Amid these formidable symptoms, however, there appears none clearly indicative of disease of the other great cavities beside the chest; nor were the evidences of pneumonia or pleurisy present. Hence the inference that a combination of symptoms such as the above, more especially if after a few days' duration they increase in violence in spite of treatment, and the signs of pneumonia, pleurisy, or intense capillary bronchitis are indiscoverable, indicates with much probability that a case of acute phthisis is before us. In these views M. Louis confirms the justness of the principle of diagnosis laid down by Sir James Clark, (on Pulmonary Consumption.) The author further observes, that if delirium have occurred at the outset, as in one of his cases, the chance of confounding the affection with typhoid fever is rendered possible. However, cough and dyspnea, at least of a notable kind, do not attend typhoid fever from the first, and in this affection the amount of prostration is materially greater, if the febrile action be intense, than in acute phthisis; in the former case too, the alteration of the features, and of the functions of the organs of sense is more deeply marked.

In *eight* other cases, in which death occurred from the fortieth to the eightieth day, the outset of the affection was not marked by any violent symptoms. The febrile action, as far as could be ascertained, presented at that period no great intensity; the cough was however almost always very troublesome, the appetite and strength greatly diminished; the cough nevertheless attained a marked amount of severity in one case only, and once only did hemoptysis occur at the outset. This last fact the author considers to a certain extent subversive of the commonly received notion, that hemoptysis depends on congestion of the lungs; for here are precisely a series of conditions favorable in the highest degree to that congestion, and yet observe the rarity of bloody expectoration. The hemorrhage occurring in the course of tuberculous disease he regards as "dependent upon some state of which the precise nature is undiscoverable, having for its indispensable condition the presence of tubercle." The difficulty of diagnosis in cases of this type is materially greater than in those of the former.

Eight of the author's thirteen patients had pain in the side; yet three only presented the results of recent pleurisy on post-mortem examination. The author thinks it a fair conclusion from these facts, that the pains of phthisical subjects are not always due to pleuritic inflammation; and that rapid development of tubercles may, independently of such inflammation, give rise to those pains. This idea derives support from our inability in *some* cases to detect any physical evidence of pleuritic change in the position they occupy.

The author passes seriatim in review the alterations of the various viscera in these acute cases; and shows, in the words of his conclusion, that notwithstanding the rapid course of the disease the secondary lesions were the same in nature and proportional frequency as in chronic cases, differing in truth only in respect of amount. It is remarkable that in this summary no allusion is made by the author to the aid obtainable by the physical signs in the diagnosis.

It would be doubtless a subject of congratulation could we ascertain with certainty the causes rendering the course of phthisis so rapid, as in

the cases we have been considering; unfortunately the information existing on this point is far from accurate. Has *sex* any particular influence in this direction? M. Louis replies by the subjoined facts:—

	210 Cases.		Mean duration.
Women .	97	...	20 months.
Men . .	113	..	17 ,,

But if from these 210 cases all those which ran a course of more than a hundred months, and respecting the precise period of outset of which doubt may be very legitimately entertained, the proportions change as follows:—

	Cases.		Mean duration.
Women .	94	...	13 m. 28 days.
Men . .	111	...	14 m. 1 day.

Hence it would appear that sex really has no special influence on the progress of the disease; and this idea is confirmed by considering in another point of view the author's facts. In truth, of 113 cases furnished by the male sex, 78 ran a course below the mean duration; and of 91 occurring among females 65 were similarly circumstanced; here the proportions are almost identically the same.

The following figures reply to the question of the influence of *age* on the duration of the disease:—

Age.		No. of cases.		Mean duration.
15—30	...	100	...	12 m. 20 days.
34—45	...	68	...	23 m. 16 days.
45—60	...	26	...	22 m.
61—68	...	11	...	14 m.

The author very justly observes that the number of cases in the last category is too small to inspire confidence in the result; and that therefore, taking the first three, we are authorized to consider youth as one of the conditions tending to give rapidity to the course of phthisis. This derives new probability from the fact that of the 13 acute cases already referred to, 11 were supplied by individuals aged between 15 and 30.

In respect of *constitution* we find these figures:—

Constitution.	No. of [fatal] cases.		Mean duration.
Strong. ...	56	...	14 m. 6 days.
Moderately strong.	87	...	14 m. 11 ,,
More or less weak.	53	...	16 m. 4 ,,

Hence, if this comparatively moderate number of cases is to be trusted to, we must infer that weakness of constitution is rather favorable than otherwise to slow advancement of phthisis; and it seems to be therefore presumable that the same form of constitution cannot be specially effectual in its first development; though, as the author observes, this will not admit of demonstration until the proportion of persons of weak and strong constitutions in the community be ascertained. M. Louis further found that of 144 phthisical subjects, still living, when observed 45 were of strong or very strong, 72 of moderately strong, 27 of weak constitution.

Latent phthisis. Although the present edition contains matter simply confirmatory of the history of latent phthisis given in the first, adding

nothing actually new, we cannot refrain from drawing the reader's attention to its contents. Of 123 cases of phthisis referred to by the author in his former edition, 8 (or 1-15th of the whole) were examples of the latent form of the disease—of the existence of tubercles for a period varying from six months to two years *before* their announcement by cough or any obvious pectoral symptom. In 4 of these cases there were no general symptoms of any importance for a certain period; in the 4 others, although cough, expectoration, &c. were wanting, fever, anorexia, and emaciation, &c., disclosed the existence of some serious affection. And it is truly remarkable that the fever and the general derangement of function were the most violent in three cases where all the organs but the lungs were healthy; so that the only diseased organ was the only one symptomatically free. Facts such as these bear the strongest testimony to the value of physical signs, the sole means given us of avoiding in instances of the kind the very gravest errors of diagnosis, of prognosis, and of treatment. Of the causes impressing this latent character upon pulmonary tubercle, M. Louis is unable to give any account.

The *diagnosis* of tuberculous disease next engages the author's attention. The affection is here considered divisible into two periods,—the one extending from the first deposition of the foreign matter to the establishment of softening; the other, from this latter occurrence to death or recovery, "or at least to the cessation of the principal symptoms and the more or less complete restoration of the patient to health."

During the first of these periods the fact of the cough generally supervening, without obvious cause, in a state of apparently excellent health, and its continuing dry for a variable period, may alone justify suspicion of the real nature of its cause. The sputa are at first and sometimes for a considerable time clear, frothy, mucilaginous-looking, and whitish; this is not the case in simple bronchitis. Wandering pleuritic pains in the sides, behind the clavicles, between the scapulæ, to which we have already made allusion, are in themselves almost characteristic of tuberculization; they could only be confounded with rheumatic pains complicating acute bronchitis, and this combination, especially in the absence (as admitted) of all apparent cause for the disease, is singularly rare. The statements already made respecting hemoptysis would lead the reader to anticipate the author's attaching very material importance to its occurrence as diagnostic of phthisis; its vast significance is indeed demonstrated by the fact, that of "upwards of 2400 patients carefully interrogated upon this point,—patients who had neither had an injury of the chest, catamenial suppression, nor gangrene, nor cancer of the lung,—one only had had hemoptysis of any serious amount." It would have been desirable to know in what proportion gangrene and cancer of the lung were met with as causes of hemoptysis during the period the above 2400 cases were passed in review. We should have wished also that the author had investigated comparatively the characters of tuberculous and cancerous hemoptysis. The rupture of aneurism ought to have been included among the exceptional causes of extensive hemoptysis; such rupture is not necessarily fatal. Acute pulmonary catarrh is generally preceded by coryza; the cough of phthisis sets in without any affection of the nasal mucous membrane.—Evening fever and emaciation will give further support to the inference justified by the previously-stated circumstances.

The author justly states that it belongs to physical diagnosis to decide the question of the existence of tuberculous disease; but we regret to find that percussion and auscultation alone are referred to. The statements respecting the results of percussion present no novelty; nor are the more delicate points of inquiry in this matter made the subject of examination. It is not enough to consider the "sonorousness" of the part percussed, or to speak of diminution of this sonorousness as announcing consolidation; the special character of the tone elicited requires quite as much—often more—close analysis; and signifies more precisely the state of the lung than the mere diminution in clearness or loudness ever can.

In the majority of cases the "character of the respiratory murmur undergoes appreciable changes, even before the sonorousness of the chest is affected;" M. Louis then agrees with close observers in general, that auscultation is available as a means of diagnosis at an earlier period than percussion. He is of opinion that in estimating the value of harsh, bronchial respiration, as a sign of disease at the summit, it is absolutely necessary to take into consideration the side at which it presents itself, as harshness of character and prolongation of respiration are more common under the right than the left clavicle as a natural condition. The author points out, too, the *naturally* more intense resonance of the voice at the right than the left summit as a condition which requires to be carefully borne in mind in estimating the signification of bronchophony.

The author regards "dry and humid crackling" occurring at the summit (and which may set in before even distinct bronchophony is established) as "the indication of a certain quantity of mucus, which may be secreted long before the softening of the tubercles present, nay, while semi-transparent granulations constitute even the whole existing tuberculous change." This is a point of very serious importance.

The author next enumerates, with rapid comment on each, the several secondary lesions to which reference has already been made, and which afford more or less positive indirect evidence of the existence of tuberculization of the lungs. We mean double pleurisy, ulceration of the larynx, chronic peritonitis, and the special form of meningitis above briefly described. Diarrhea of long continuance, accompanied with emaciation, and obstinately resisting all modes of treatment, M. Louis regards as almost exclusively the apanage of phthisis, and capable of leading to the diagnosis of that disease, in cases where neither cough, expectoration, hemoptysis, nor pain in the side existed.

The pages devoted to the diagnosis of the second period of the disease, present little which is not sufficiently well known, to absolve us from the necessity of analysing their contents. In commenting upon the cracked-metal character of the percussion-note furnished by large cavities, the author states that he has, in cases of pleuritic effusion and simple pneumonia of the upper lobe, detected the modification of sound. That the *tubular or amphoric note* (such as is produced by sharply filliping the cheek while the mouth is fully but not very forcibly distended with air) is sometimes to be elicited in the class of cases referred to by M. Louis, is matter of established doctrine in this country; and we believe that it is this peculiar note which is meant by him and not the perfect *cracked-metal character.* At least, we have never encountered this

modification of sound under the circumstances supposed, and its very mechanism in the cases in which it really is producible points to the extreme improbability, if not actual impossibility, of its being generated in cases of simple pleurisy and pneumonia.

Phthisis terminates almost invariably by death, says M. Louis, whether this occurs a few weeks or several years after the outset of the disease. In some rare cases, however, he admits that a favorable issue occurs, and that after having suffered from the severest symptoms the patient is able to return to his ordinary occupations. Three cases are here briefly sketched, in which the disease appears distinctly to have undergone suspension of progress. The author well observes that such cases are in all probability of not very unfrequent occurrence, but that they often pass unnoticed. The statements here given add nothing to our knowledge on the curability of phthisis; it is not the occasional cessation of symptoms of the disease and apparent restoration to health which require to be demonstrated, (all are aware that such fortunate changes do sometimes occur,) but the proportion of cases in which such changes may be expected, and above all the circumstance of age, sex, climate, habitation, constitution, occupation, &c., which may exercise appreciable influence on their accomplishment. Connected, of course, with the latter system of inquiry is the investigation of the therapeutical means best calculated to facilitate the natural processes by which the suspension of the malady is effected.

The researches of the late M. Rogée lead to the inference that anatomical cure of tuberculous disease is a phenomena of much more common occurrence than we should upon any other evidence be justified in believing. Cretaceous and calcareous deposition in the lungs, he, in every instance, regards as the result of transformation of tuberculous matter, and such deposition he discovered in the lungs of fifty-one out of one hundred old women dying at the Salpêtrière, into which institution admittance is not granted until the applicant shall have attained her sixtieth year.

Upon the cases of the three patients just referred to, M. Louis remarks, —"it is worthy of note, that these three persons had passed their fortieth year; that only one lung had been, or appeared to have been, tuberculous; that in two of the cases the cavities had to all appearance retained their primitive dimensions; that the patients belonged to different classes of society; that the outset of the affection was marked by symptoms of much severity in one case only, and that in none of the three could the favorable course of events be ascribed to the influence of treatment." To these observations we may add that in the three cases the *right* lung was the organ affected; and that in the immediate neighbourhood of the single cavity, lined with false membrane existing in one of these cases, were two tubercles of the size of a very small nut in an incipient state of softening: the former fact lends support to the observations already made (p. 427) regarding the questionable accuracy of M. Louis' inference as to the greater frequency of implication of the left lung.

Thirty-six pages are devoted by the author to an examination of the *causes*—predisposing and exciting—of tuberculous disease. The general inference from these is depressing to the last degree; for it is none other than that the most profound ignorance prevails on the subject. We

knew it before we saw these pages; we know it more fully and thoroughly now. Still this section of the volume is far from being without its value, for it furnishes a philosophical exposure of the fallacies into which authors, prone to find *causes* in *antecedents*, necessarily run. A few examples will suffice. *Weakness of constitution* is set down as a cause of phthisis, and the influence of such constitution appears so readily traceable in the development of the disease that it has been universally admitted. But where is the demonstration of the alleged fact? Who has ascertained the proportion of weak and strong constitutions in a given population, and ascertained the ratio in which each class furnishes the victims of phthisis? Yet until this be done, it is obvious that no shadow of importance can be attached to even the most speciously attractive reasoning in favour of the destructive agency of weak constitutions. The more so, as we have seen that M. Louis found the disease run a somewhat more rapid course in subjects of strong than feeble constitution.

The reality of hereditary influence in the production of phthisis is so universally admitted that it would seem a sort of scientific heresy to doubt it. Yet, it may be asked, how has it ever been *proved*? What demonstration has ever been given better than the often reiterated statement (the truth of which is indubitable) that persons often die phthisical whose mothers or fathers were so before them. But obviously, in the instance of a disease so common as phthisis, this affords no sort of demonstration. In order to ascertain the reality and the amount of the influence in question with surety, it would, as the author observes, be necessary to have "tables of mortality, by means of which an equal number of subjects, born of phthisical parents and of persons whose fathers and mothers were not tuberculous, might be compared together." A Parisian hospital physician, M. Briquet, has rather recently published some statistical results respecting phthisis to which undue importance is very likely to be attached; one of these results appertains to the present matter, and we subjoin it:

Phthisical subjects.	Parents healthy or non-tuberculous.	Parents phthisical.	Parent's health unascertainable
67 Males	37	24	6
32 Females	14	12	5*

Hence it would follow that, taking both sexes together, about one third of phthisical subjects spring from parents similarly diseased. But, "if the mortality from phthisis at the Necker Hospital (of which M. Briquet is physician) were during three years $\frac{11}{37}$ of the whole, or a little less than one third, and if this ratio were general through the capital, it would signify that $\frac{11}{37}$ of the population of Paris die phthisical, and that, consequently, whenever engaged in the study of hereditary influence in connexion with any disease, we must expect to find tuberculous parents eleven out of every thirty-seven times; so that if the proportion were found the same in the parents of tuberculous subjects, it would follow that hereditary transmission is really without influence in the case of phthisis." It is to be observed that all these *ifs* are attached to propositions which are in all probability positively ascertainable.

The alleged influence of the use of stays on the development of

* There is some slight error here in the figures in the original.

phthisis is possibly a matter of mere assertion, according to the author. He might have gone further and affirmed it to be actually so. We know of no demonstration in any language on the subject, and are indeed unacquainted with any serious attempt to prove the reality of the baneful influence which it is the fashion to dwell upon. Many of the females submitted to the author's observation had, it is true, laboured under difficulty of breathing for a long period before they became phthisical; but the number of men similarly affected was not less considerable. And besides the majority of the author's patients had been brought up in the country, and not worn stays until their removal to Paris, at an age when they had ceased to grow, and when consequently the action of stays could have little or no influence on the dimensions of the thorax. The question in England is somewhat different, for with us stays are much more constantly worn at all periods of the day,—the desire to produce smallness of waist is also perhaps stronger and more prevalent here than in France, in proportion as the width of pelvis is less among our women than the French; for it is not so much positively small measurement that is coveted, as smallness in comparison with the dimensions of the figure on the level of the hips. But it is obvious that until we have the opportunity of comparing two series of women in all other respects similar, except that the one shall use stays at an early period of life, employ them constantly, and keep them very tightly laced, and the other not use this artificial support, or use it but to a very limited extent, we shall continue without the means of deciding whether and to what amount the alleged influence be real or not.

III. Treatment. We must refer the reader to the work itself for much detail respecting many of the other alleged causes of phthisis; our remaining space we feel called on to devote to the consideration of the author's Third Part, in which the treatment of the disease is made the subject of consideration. A chapter of this part contains the results of M. Louis' inquiries into the real efficacy of the various medicines which have more or less recently been brought into notice as capable of arresting the course of phthisis or curing it altogether.

The *protiodide of iron* stands first on the list. This medicine, the introduction of which into medical practice M. Louis erroneously ascribes to M. Dupasquier of Lyons,* was, we believe, first employed by this person in the treatment of phthisis. The flattering accounts published by him so far back as 1835, appear not to have excited any general attention, until within the last two years, a fact not a little surprising when we consider that these accounts embrace such statements as the following, "the protiodide of iron is borne well by phthisical patients to the amount of from twelve, to thirty or forty drops daily," [how the solution is prepared, and in what proportion it contains the solid salt, we are not told,] "and exercises its influence especially on the lung. Its effects generally become manifest in the space of eight days; out of every *ten patients labouring under the disease in the third stage, at least six or seven derive notable relief from its use.* At the end of a few days prompt diminution, amounting almost to suppression, of expectoration; relief of cough and

* It is sufficiently notorious that Dr. A. T. Thompson used the salt in question medicinally at an earlier period than this.

dyspnea; diminution and eventually suppression of the sweats; diminished rapidity of the pulse; decreased heat and fever; increased strength and appetite are noticed. . . . . In some cases in spite of the improvement of the symptoms, the patient grows weaker and weaker, and gradually perishes; but frequently on the other hand, and in cases where the presence of a cavity had been satisfactorily ascertained, the amelioration becomes from day to day more evident, the patient recovers flesh, the cough and fever disappear, the patient regains his gaiety, and leaves the hospital in a state of cure which it is permitted to hope may sometimes be definitive." M. Louis, encouraged by these most striking announcements, and informed in personal interviews with M. Dupasquier, of the manner of preparing and administering the medicine, employed it in upwards of sixty cases occurring in either his hospital or private practice, and "to his astonishment ...... *in not a single case did he observe any amelioration which could be attributed to the new agent.*" Still, on receiving the positive assertions of M. Dupasquier, and believing it difficult to admit that this physician was mistaken, "at least very frequently" in his diagnosis of phthisis, M. Louis thinks it worth while recommending a further trial of the medicine. We have ourselves used Squire's solution of the salt pretty extensively, but as yet without forming an opinion sufficiently definite, for we have not yet analysed our cases, to justify us in laying it before the reader.

Common salt has of late been forced on the attention of the profession by M. A. Latour, as capable of arresting, nay, of curing advanced phthisis. Here is one of his "cases." "Mademoiselle B. aged 14; phthisical to a very advanced amount, cavern at the apex of the right lung, extreme emaciation, general symptoms extremely severe. This patient was seen by Drs. Scott and Baron. The treatment commenced the 13th April, 1839, was terminated by the end of May, and *followed by complete cure.*" Assuredly statements such as this savour more of the charlatanism of Mr. Morrison, than of science; but knowing, it appears, that M. Latour was a conscientious person, our author gave salt, during five successive months, to all the patients received into his wards at the Hôtel-Dieu. "In no single case, however, did he observe any appreciable effect produced in the state of the functions. Some patients could not go on with the chloride for more than a few days, the greater number took it for a month and upwards."......

Subcarbonate of potass has been recently recommended as a general solvent for all kinds of swellings and engorgements, and as exercising its powers specially in respect of tubercle. But as from his statements, M. Pascal, the author of the recommendation in question, appears like most vaunters of new drugs, to be utterly deficient in the power of distinguishing one disease from another, (judging at least from his printed details,) M. Louis considered himself conscientiously absolved from the necessity of administering subcarbonate of potass.

Dr. Cless, of Stuttgard, derives astonishing advantage (as he says) from the administration of *sal ammoniac* in large doses. The *chloride of lime* is quite as effectual in the hands of M. Hirzog, of Posen, who demands our conviction of his accuracy on the faith of such admirably narrated cases as the following:—"A man, aged 28, presented all the symptoms of phthisis, voided puriform sputa, and left the hospital

cured, after a treatment of fourteen days." The excellent M. Hirzog probably has just as lucid notions about what constitutes phthisis, as it appears he possesses upon the kind of evidence which will now-a-days convince rational beings. But the truth is, a great number of the Germans are in the happiest of all possible conditions for "curing phthisis" readily,—in perfect ignorance of the principles of physical diagnosis, they trust to the local and general symptoms for their guidance, and their acquaintance even with these is superficial and routine-like,—how often chronic bronchitis, simple chronic induration, chronic pleurisy, &c., must be confounded with phthisis under such circumstances is sufficiently obvious. There are of course numerous exceptions to this sweeping statement, but we affirm that in the smaller towns, it will be found to give a fair notion of the proficiency of the medical inhabitants in respect of pulmonary diagnosis.

The cases published some years since by M. Cottereau, in illustration of the curative influence in phthisis of *inhalation of chlorine* are next submitted to analysis by M. Louis. And it clearly appears from this analysis, that not a single one of the cases in question proves the real efficacy of the alleged specific. Some of them are deficient in detail to such a degree, that it is impossible to determine the nature of the disease treated; others are examples of pneumonia or pleurisy occurring in subjects with tuberculous lungs, and it is obvious that with the removal of the pneumonia or pleurisy (which be it observed was in all probability rather retarded than accelerated by the chlorine inhalation,) great improvement in the symptoms must have taken place, and naturally been ascribed to the influence of chlorine on the tuberculous disease, inasmuch as the complications of this had been overlooked. The author, notwithstanding the unfavorable issue of the scrutiny, submitted upwards of fifty phthisical subjects to the action of chlorine, and "without in a single case obtaining a successful result." General experience on this matter supports completely the unfavorable estimate of M. Louis.

The virtues of *digitalis* are next submitted to examination. England having produced the vaunters of this drug, our readers are doubtless acquainted with the wonderful cures ascribed to its agency, and not less familiar with the fact, that no one at the present day has the shadow of confidence in its alleged specific influence on phthisis.

An Italian practitioner, named Fantonetti, has cured *cavities* by the administration of prussic acid *in seventy-two days!* This is quite enough on the subject of Signor Fantonetti and prussic acid.

Creosote has had its supporters as a healer of tuberculous cavities; but no evidence exists in its favour of a kind to claim the consideration of rational persons.

The author closes his list of reputed specifics with iodine, which he does not appear to have tried upon a large scale himself.

Our first inclination was to regret that M. Louis had not experimented with emetics, which have perhaps been more cried up than any system of medication of late years. But perhaps, on considerations of humanity, we have reason to rejoice that some hundred individuals additional were not submitted to the misery of vomiting every second morning for some weeks or months; for we confess that what we have heard or read on the powers of these medicines has left no other impression on our minds than

that of the facility of belief—if it would be discourteous to use the word credulity—of those who administered them. M. Louis might have ascertained the names of several other specifics for phthisis from the works of young gentlemen in this country, laudably desirous of notoriety and practice; but as these names have escaped him we shall allow him to remain in his ignorance,—it would be hardly fair, indeed, to spread these discoveries abroad, and so run the risk of filching from the original observers the reward of their patient zeal and most amiable disinterestedness.

But if the brief survey now given demontrates the utter insignificance of all declared specifics, it by no means justifies us in supine carelessness as to the discovery of a medicine endowed with the special attribute of modifying the course of phthisis. The efforts of those who are placed in a position fitted for the purpose should be unceasing in the search after such a medicine; for nothing can be more unphilosophical than to conclude than it does not exist, because it has not yet been found. It is manifestly not from the ὁι πολλοι of drug-lauders that the discovery of so inestimable a boon is legitimately to be expected,—not from those who seize with avidity upon each new medicine introduced into the market and administer it pell-mell in every form of human ailment,—invariably, according to their own accounts, with the "happiest effects:"—it is from him who, thoroughly versed in the diagnosis of disease, has enough of incredulity in his intellectual composition to doubt the evidence which is not repeated time after time in similar cases,—who has a fund of patience which no labour can exhaust, and a conviction of the grandeur of his task which disappointment, be it repeated ever so often, can never succeed in shaking. And if the general truth, that men of this noble stamp are rare, give us just cause for apprehension that the day of success is yet far distant, we have still fair motive for high hope in the fact that no profession perhaps numbers so many thus endowed in its ranks as the medical.

The author turns to the subject of rational treatment, commencing with a lucid and extremely practical chapter on the means best calculated to ward off the development of the malady in those constitutionally predisposed to it. We shall not extract from this chapter however; because, admirable as it is, the subject has been so completely exhausted by Sir James Clark, that nothing actually important and at the same time novel to the reader of the works of our countryman, could be easily discovered. It is most gratifying to find that the first pathologist of continental Europe has given the weight of his influence to the study of these purely practical matters; especially as it is impossible but that his example will be followed by many of his disciples.

We shall dwell somewhat more at length on the author's views respecting the treatment of the actually formed disease. During the earliest period of the chronic malady, at a time when the physical signs united to the local and general symptoms scarcely suffice to furnish an exact idea of the patient's real state, if there be no particular fever, diarrhea, nor severe thoracic pain, and if the patient be of lymphatic temperament, slightly tonic vegetable infusions should be prescribed; and if the cough be troublesome, some preparation of opium or stramonium in addition. Inhalation of aqueous or narcotic vapours should be had recourse to several times daily, if the cough resist the latter medicines taken by the

mouth, or the former used in enema. The recommendation of tonic drinks at the outset of phthisis must be so novel, to the French reader especially, that we are not surprised to find the author entering into an explanation of the motives which guide him in authorizing it. His arguments may be briefly stated thus: the tuberculous affection is a *general* disease of the system, and hence local remedies must be insufficient to check it, a general treatment being the only one likely to modify the constitution; for persons of the stamp above referred to, tonic medicines are the best fitted, the exhibition of mucilages and the observance of a milk diet are, as far as knowledge goes of the probable causes of tuberculization, means calculated rather to increase than diminish the evil. M. Louis never advises the use of asses' milk, except in those very rare cases of excessive sensibility of the stomach, in which that organ will not bear any other sort of nourishment. In large towns he is strongly opposed to its employment; believing that in these the asses must, as well as the cows, frequently themselves grow tuberculous.

To the vegetable bitter the author would recommend the addition of some ferruginous water at meals (Bussang, Spa, Pougues, &c.,) or some artificial preparation of iron, such as the protiodide. Evening thirst should be relieved by diluents having the same temperature as the apartment, or by a little very very weak chicken-broth.

M. Louis expresses himself most emphatically against the use of issues;—"neither in hospital nor private practice have I in a single instance seen any amelioration produced which could be legitimately ascribed to them."

The importance of avoiding exposure to sudden alterations of temperature, &c., is duly considered; and in connexion with this, the propriety of sending phthisical patients to winter in a warmer climate than their own examined into. Like Sir James Clark, the author counsels removal to warmer latitudes in the very earliest stages of the disease, and on the same grounds as our experienced countryman. But, like him, he admits that a successful result is by no means to be predicated with certainty; and states that he has known females pass two successive winters in warm climates, return to Paris and go through the succeeding winter more favorably there than in their more southern place of sojourn. The author has less confidence in the utility of sea-voyages than many of his predecessors.

Upon the absurdity of submitting these patients to a severe system of diet the author well insists: their food should be succulent, and insuring the greatest amount of nutritive property in the smallest space.

If, contrary to what has so far been supposed to be the case, there be much fever present, until this be removed it is the author's opinion the tonic preparations should be given up and mucilaginous drinks substituted: we believe that in the great majority of cases this will be found correct practice, but are persuaded that instances occur in which ferruginous preparations may be continued with advantage, even during the existence of feverish action of some intensity.

But persistence in the system of treatment now indicated may be rendered impossible, for a time at least, by the occurrence of some one or other of the urgent symptoms to the appearance of which the phthisical subject is almost from the first hour of his illness more or less exposed.

The author considers with much minuteness the various modes of combating these, (hemoptysis, local pain, sweating, &c.;) but we do not discover any suggestions here likely to be new to those acquainted with the writings of English authors. Nor shall we analyze the pages devoted to the consideration of the disease in its second stage; whatever be the wisdom of the therapeutical views here taken—and it is remarkable—we find in them no actual originality, no motive for believing that the author possesses the means of prolonging the life of the consumptive beyond the term attainable by his brethren.

Our task would have been terminated, were it not that some interesting remarks on the probable means of acquiring more deep knowledge of the causes and therapeutical management of the tuberculous affection, present themselves in the preface, and imperatively call for notice. The author is persuaded that success of any consequence in these directions can only be obtained by the united efforts of a large body of physicians placed in various circumstances,—some of them attached to large public establishments, others engaged in private practice only, medical officers in the army and in the navy, &c. But what are the qualifications required on the part of the observer to render him fit to take part in the great work,—what the conditions under which those qualifications should be employed? The first element of capability on the part of the physician is the possession of intimate acquaintance with the natural course of the disease. Infinitely true; but unfortunately this truth has the effect, *in limine*, of cutting down the number of capables most wofully: does one of every hundred practitioners, taken *en masse*, understand the "natural course of the disease;" is he familiar with its diagnosis? We fear not. To medical practitioners thus qualified, however, M. Louis would assign the task of keeping full and accurate histories of all their private patients, comprising details upon the various places of habitation of these persons (rather a difficult task for those practising in these days of railways and steam,) their education and bringing up, their mode of life at all periods of their existence, the affections to which they have been subject or have suffered under, their constitutional state, the mode of progress of the malady, &c. &c. If the individual belong to the working classes, all the circumstances attending the trade he follows, the age at which he commenced to work at it, the number of hours daily devoted to its labours, &c., should be clearly made out. Besides, the health of parents and of all other near relatives should be ascertained, &c. If researches of this kind were continued for a certain number of years by a sufficient number of persons in cities and in country districts, in inland situations and on the sea-shore, in mountainous and in low localities,—applied to soldiers and to seamen, and, in a word, to individuals placed in all possible varieties of hygienic condition;—*if* all this were done, doubtless the world would be much more deeply conversant with the etiology of phthisis than it now is. But when shall we find the enthusiastic *corps* ready to take upon itself the Herculean task which the ardour of the great master has thus traced out? Upon this head we are doomed to conjecture.

Further, M. Louis, in the hope of elucidating the real influence of climate, proposes the institution of a band of travelling physicians, whose duty it should be to study the laws regulating in different latitudes the

progress of the more severe classes of disease. We do not see why the resident physicians may not be deemed, from their long experience, more capable of throwing light on such points than those who (however much superior in respect of pathological acquirement) pass but a few fleeting hours on the spot; more especially, as M. Louis admits, that the wanderer must depend in great measure for his information upon the stationary practitioner. And again, we believe the experience of those countries or universities which do actually supply the funds for travelling observers, deposes in faintest accents indeed as to the utility of such persons. M. Louis, wrapped in his scientific aspirations, forgets the temptations to which the young are exposed in many of those countries to which their mission would invite them,—he dreams not of the enervating influence of those climes where the *dolce far niente* is the summum bonum of existence, and where materialities of all the most intoxicating kinds conspire but too triumphantly to drive philosophy from the field.

But whatever may be the estimate formed of the actual feasibility of M. Louis' scheme, none can fail to be impressed with deep feelings of respect for its projector. Had an individual of less elevated moral attributes laboured so successfully as *he* has done in unravelling the difficult windings of phthisical disease, the successful investigator would have been disposed to exert all his art to prove, if not that the goal was reached, at least that it was unattainable,—that further improvement was an impossibility. M. Louis here exhibits his intellect and his character in a new light; one which must contribute to shed still brighter lustre on a name, than which a more illustrious graces not the Court of Science.

Art. XIV.

Practical Remarks on Gout, Rheumatic Fever, and Chronic Rheumatism of the Joints; being the substance of the Croonian Lectures for the present year, delivered at the College of Physicians. By Robert Bentley Todd, m.d. f.r.s., Physician to the King's College Hospital, and Professor of Physiology in King's College, London.—*London*, 1843. Post 8vo, pp. 216.

We did not expect, when we recently addressed ourselves to the subject of gout and the disorders connected with it, to be so soon called upon to return to the consideration of the numerous and interesting questions it involves; but, desirous as we are to give our humble aid in forwarding any attempt to place pathology upon its true basis, and believing, as we do, that no disease can be so advantageously studied with this object as the one to which we now again invite the attention of our readers, we shall offer no apology for recurring to it. Indeed, the respectability of the authority from which the present treatise emanates, would of itself give to it strong claims on our attention, and on that of our readers. The author's purpose in laying his Croonian lectures before the public shall be stated in his own words.

"The object which the author has had in view, is to place in juxta-position the leading facts in the Natural History of Gout and Rheumatism, in order to direct the attention of practitioners, in a more especial manner than has hitherto been done, to what he conceives to be their true pathology. The work does not profess to give a complete history of these diseases; on the contrary, many details of

symptoms, etiology, and treatment have been purposely omitted, as irrelevant to the argument, the design of which is, by contrasting the phenomena of gout and rheumatism with those of diseases confessedly caused by a morbid state of the fluids, to claim for them a similar origin. The author has availed himself of this opportunity of announcing some facts which he does not recollect to have been noticed by any previous writer on the subject. These will be found in the section "On the paroxysm of gout appearing in low states of the system;" and in that "On the rheumatic diathesis," where it is shown that disease of the heart may come on in that state of constitution, irrespectively of the occurrence of the rheumatic paroxysm, or fever." (Preface, pp. v-vi.)

Now we feel called upon to state, *in limine*, that we think the author has considerably over-rated the amount of novelty which his treatise contains. We should not have thought it necessary, at this time of day, to employ any arguments to establish a *claim* on the part of gout to a humoral origin; since we have always imagined that this is a subject long placed beyond dispute. Gout has been for a long period the strong hold of the humoral pathologist, even in the most exclusive reign of solidism; and from the time that the tide has begun to turn, and the morbific agency of a depraved state of the blood has been more attended to, gout has been continually cited as an unquestioned and apposite example of the influence of such a condition.* Moreover, the chemist has succeeded in detecting that which many pathologists have no hesitation in regarding (either in its simple state, or in combination with soda,) as the real *materies morbi*, lithic acid; and from the time that a strong probability (to say the least) was established in favour of this view by Mr. Murray Forbes, at the close of the last century, the treatment of intelligent practitioners has been directed to the expulsion of it from the system, and the prevention of its reaccumulation.

Dr. Todd attempts to unsettle our notions on this subject, however, by adducing facts and arguments which lead him to the inference that lithic acid is *not* the *materies morbi* of gout; but he does not substitute anything else in its place, his idea of the pathology of this disease being summed up in the following vague and indefinite proposition:

"It appears to me that we must look for the matter of gout as a compound, derived from a product of unhealthy action of the stomach and duodenum, which, being absorbed into the blood, unites there with some elements of the bile, which has been suffered to accumulate through the defective secretory action of the liver." (p. 74.)

As we shall presently join issue with Dr. Todd on this subject, we shall briefly recapitulate the principles which we advanced in our review of Dr. Bence Jones's Treatise, in regard to the causes of an accumulation of lithic acid in the blood.

I. The most frequent cause of an undue production of lithic acid in the system, we believe to be the ingestion of an excess of azotized food. If more is absorbed than can be applied to the replacement of the waste of the body, it must necessarily undergo decomposition, whilst still circulating in the blood; hence, if not drawn off by the excreting organs, it must accumulate in the vessels.

II. Even where there is no excess of production of lithic acid in the system, there may be an accumulation of it in the blood, in consequence of deficient excretion. The reduction of the acid to an insoluble form, by the removal of the

* See, for example, Dr. W. Budd's paper on the Symmetry of Disease, in the Medico-Chirurgical Transactions, vol. xxv.

ammonia with which it is usually combined, through the agency of another acid, is one of the most frequent causes of this deficiency. The presence of an undue amount of lactic acid in the blood, therefore, may operate as effectually in inducing the accumulation of lithic acid in the system, as the excessive production of the latter. The lactic acid may either be generated in undue amount, by disorder of the digestive process, or it may accumulate in consequence of deficient respiration and cutaneous excretion.

III. We recognize as a possible cause, though we cannot admit that any proof of its agency has yet been given, the introduction of an insufficient amount of oxygen into the system; so that the products of the decomposition of the muscular and other azotized tissues are partly converted into lithic acid, instead of into urea.

Now in regard to the constitutional treatment which we should found upon these principles, we pointed out the necessity, in the *first* place, of reducing the azotized portion of the diet to as low an amount as is consistent with affording the supply required to repair the waste of the body; but, *secondly*, we noticed the importance of strict attention to the due digestibility of the diet employed; since, if lactic acid be unduly generated in the stomach, we shall in vain attempt to get rid of lithic acid from the blood; and *thirdly*, we urged attention to those means by which the excreting processes are rendered more active; exercise, diaphoretics, purgatives, &c.,—the first of these serving the additional purpose of drawing off any surplus azotized matter in the blood, and at the same time of introducing an increased supply of oxygen. To these pathological and therapeutic principles, we shall more than once have occasion to refer in our subsequent remarks.

Dr. Todd commences with some judicious observations on the importance of studying the natural history of disease, and on the mode of advancing our knowledge of it. In regard to the particular maladies which form the subject of this treatise, he remarks:

"Upon neither the nature, nor the proper mode of treatment of either of these maladies, but more especially of the latter, can it be said that the views of medical men coincide. We can scarcely ascribe this disagreement of opinion to absolute ignorance of the natural history of these diseases, for they have long been the subjects of close and careful observation; and the descriptions of the older physicians have not been surpassed by the *longioris ævi diligentia*. The true cause of the difference seems to consist in an *imperfect analysis* of their natural history; so that we still stand in need of a careful induction from the facts with which the observations of practical physicians have supplied us, in connexion with the improved state of our knowledge of the phenomena of nutrition in its normal state." (p. 11.)

He then proceeds to advance the claim of gout and rheumatism to be regarded as diseases of the blood, in a manner which, as we have already remarked, would lead a previously ignorant reader to suppose that some considerable novelty was contained in the proposition, and that it was requisite for the author to "guard himself against the imputation of having stated merely speculative notions." Before describing the characters of the true *blood-diseases*, he makes some distinctions which it is important to keep in view.

"Let me, in order to prevent misunderstanding, separate from blood-diseases those which, originating in some local taint of nutrition, are disseminated by the blood to other and even distant parts of the body. The whole class of malignant diseases may be referred to this category; cancer, fungoid disease, &c., originate in a local derangement of nutrition, as in the stomach, the lip, the penis, &c., where the peculiar cancer-cells are first formed. If the local malady be within

reach of the surgeon's knife, and can be removed early, the best chance is afforded to the patient of being rescued from its reappearance in some other place; but if, as is too often the case, the diseased part has been allowed to remain too long, the peculiar cancerous matter makes its way into the blood, and is carried along in the current of the circulation to other organs, where they become the nuclei of new cancerous formations." (p. 13.)

This distinction had been previously drawn by Dr. W. Budd, in his paper on cancer, (Lancet, 1841-2, vol. ii. p. 266;) and in terms so very similar, as evidently we think to have suggested the preceding paragraph. Dr. Todd then proceeds:

"We must also separate those diseases which appear to arise from the deficiency of some element which forms a necessary constituent of the blood, such as purpura, scurvy, and certain states of the body characterized by an anemic condition."

We are not quite sure how far purpura and scurvy can be characterized as diseases of mere *deficiency* of certain elements of the blood; but supposing them to be so, why does not Dr. Todd arrange under the same category diseases which result from an *excess* of certain normal constituents of the blood? Is it not equally certain that in the inflammatory diathesis there is an excessive production of fibrin, and that in the plethoric condition there is an excess of red corpuscles?

"In what I would call the *true blood-diseases*," continues Dr. Todd, "a morbid matter is generated by an abnormal chemical action in the blood itself. The morbid element may be formed primarily in the blood, in consequence of some check to one or more of the ordinary excretions, or from the supply of nutrient material being too great for the rate at which excretion is carried on; or it may have been introduced into the blood as a poison, which speedily deranges the normal changes that are continually going on within it. In this last way, all contagious diseases are propagated, the matter of contagion having been introduced into the blood; malarious diseases are also referrible to this cause. To either or both of the former causes, I hope to be able to show that rheumatism and gout are attributable." (p. 14.)

This is a proposition, we again repeat, which scarcely seems to us to require so laboured a demonstration, more especially after the elaborate and ingenious paper by Dr. W. Budd on Symmetrical Diseases (Medico-Chirurgical Transactions, vol. xxv.), to which in this, and several other parts of his treatise, Dr. Todd seems to us to lie under a degree of obligation, which is but insufficiently acknowledged by a reference to it in regard to the particular subject of the symmetry of chronic rheumatic affections (p. 133). For example, there is continual allusion throughout the work to the proneness of the morbific matters circulating in the blood to fix themselves upon particular parts to the exclusion of others, by a sort of affinity or attraction for the components of those parts. As a vague general hypothesis, this idea might be said to have been long floating in the minds of humoral pathologists; but it was brought forward by Dr. Budd (and by Mr. Paget) in a new and precise form, and supported by novel and very striking evidence; and the identical form, and much of the evidence, is employed by Dr. Todd without reference to the source from which he derived it. We are sure that Dr. Todd is far too honorable a man to have done this from any other cause than inadvertence; but we feel it to be a part of our duty as critics, to keep before the public mind the real sources of improved views which may arise from time to time, either as

to the science or art of medicine; and where an author brings forward views which he has derived from others, in a form that *seems* (even though he may not so intend it) to claim for them his own paternity, we feel it to be an imperative act of justice to direct attention to their real sources. The following quotations, from among many similar parallelisms we could adduce, will serve, we think, as a justification of the tone we have adopted.

"From these facts it appears highly probable that there is a peculiar matter circulating in the blood, which give rise to the phenomena of gout, by occasioning a disturbance of longer or shorter duration, of greater or less intensity, in the nutrition of parts to which it is attracted; the effects of this morbific matter upon any part seem proportionate to the quantity that is drawn to the part; and when strongly attracted to one part, it is not likely to affect others. Hence in first attacks of gout in strong and healthy individuals, a single joint only is generally affected; but if anything impair the force of attraction of the morbid matter to that joint, the poison flies to other parts, which it will fix upon simultaneously or in succession.

"A safe and most important rule of practice may be derived from this fact; namely, *to be very cautious about interfering with the local disturbance which a fit of the gout creates in external parts.* For such local interference, by diminishing the force of attraction of the part first affected, for the gouty matter, may favour its transference to other places." (pp. 55, 56.)

"In the use of purgatives, care should be taken to employ only mild ones. Drastic purgatives should never be given; as, by the excessive irritation of the intestinal canal, which they would create, the gouty matter might be attracted to it from the external parts." (p. 102.)

"These views derive interesting confirmation from a fact to which many a gouty patient can bear testimony, namely, that a part which had previously been injured, is certain to be the seat of the gouty inflammation in subsequent attacks. If a foot or hand have been sprained or otherwise injured, the gout will fix itself there, because the injury has exalted the nutritive process of the part, and there is therefore a greater attraction of the elements of the blood to that part, than in the state of health.* For the essence of nutrition consists in an attraction between the solids and fluids, by which those elements of the latter which correspond in chemical constitution to the former, are drawn into and appropriated by them." (pp. 56, 57.)

We shall now subjoin a few extracts from Dr. Budd's paper; in which, it is to be remembered, the subject of gout as a well known blood-disease is only incidentally noticed, the author chiefly directing attention to the causes why it should not uniformly affect the body in that *symmetrical* manner which he shows to be characteristic of most blood-diseases, and to be one of their most important indications.

"It may be stated, then, as a general proposition, that, in diseases consisting of a number of lesions having a tendency to a symmetrical arrangement, the symmetry will be more perfect, as the course of the affection has been more free from febrile movement or local vascular excitement; in short, has been more chronic in progress, and has resembled more nearly in character the ordinary processes of assimilation; constituting, in this latter relation, a fact of striking import; and giving, in the view which regards these processes as consisting in the separation of matters of definite and identical composition from the blood, by structures of identical nature, the best authority for the particular theory of these diseases, maintained in the foregoing pages.

"Another circumstance of great effect in interfering with the manifestation of

* Cruveilhier states that those joints are most liable to be affected by gout which are most used.

symmetry in disease, consists in the influence which mechanical injury, or any other cause materially affecting the organic condition of a given part, has, in determining morbid matters present in the blood to that part, in preference to others of the same structure. The powerful effect of this influence is sufficiently familiar to practitioners. In gout, especially, this influence of mechanical injury is of such frequent and obvious effect, that M. Cruveilhier has been led to regard the friction and mechanical shocks to which the joints of the feet are especially subject, as the sole cause of the preference of gout for these parts." (Op. cit. p. 127.)

And again, subsequently, Dr. Budd remarks:

"From these views we may also learn the great importance in the treatment of diseases which are liable to shift their seat, of keeping the vital organs as far as possible in a quiescent state, lest, by exposing them to causes of irritation, we thereby favour the metastasis of the morbid matter to the irritated organ, and endanger the life of the patient. The neglect of this important precaution may be, I have several times seen reason to believe, the cause of the transference of gouty matter to the stomach, and the occasion of a fatal event." (p. 131.)

This therapeutic precept evidently corresponds, both in its purpose and its rationale, with that on which Dr. Todd lays so much stress, though couched in different terms.

We shall not again refer to this topic; and gladly quit it, to consider, with Dr. Todd, the origin of gout, and its connexion with the lithic-acid diathesis. As we have already remarked, he does not agree with Dr. Prout and other eminent authorities of the present time, in regarding lithic acid as the *materies morbi*, but his reasons for dissenting from them appear to us far from satisfactory. He seems entirely to lose sight of the fact, that gout results from an accumulation of lithic acid in the blood, whilst a deposit of lithic acid *in the urine* takes place in consequence of its separation from the blood. Hence, as we have already pointed out, we may have an accumulation of lithic acid in the blood, giving rise to gout, without any over-production, in consequence of deficient elimination; and, on the other hand, we may have a copious gravelly deposit of lithic acid in the urine, without the least tendency to accumulation of this substance in the blood. Hence gout and gravel may be vicarious, and it is well known to practitioners that they are frequently so. The symptoms that appeared to forewarn an attack of gout are often observed to pass away, when a copious discharge of lithic acid takes place by the urine, and a regular paroxysm of gout seems frequently to be relieved by a similar critical evacuation. Doubtless, gout and lithic-acid gravel may coexist; for, in consequence of an over-production of lithic acid, there may be more of it in the blood than the kidneys can possibly separate; and we may therefore have gouty symptoms developing themselves in the body, whilst there is at the same time a copious deposit in the urine. But where the deposit takes place, not from excess, but from precipitation, there is no reason whatever why gout should be expected; and the objection urged by Dr. Todd to the idea, that lithic acid is the *materies morbi* of gout—namely, that a copious deposit of this acid in the urine often takes place for weeks or months together, without any gouty symptoms manifesting themselves (p. 66)—is thus totally destitute of force. We are surprised that this view of the case did not occur to him, since he is well aware that an over-production of *lactic* acid in the stomach is one of the commonest causes of an attack of gout (p. 73); and the influence of this agent, in reducing the lithic acid to its insoluble form

by depriving it of its base, ammonia, and thereby rendering it more difficult of excretion, has been long since pointed out by Dr. Prout, and has been generally recognized.

We find little else to remark upon in Dr. Todd's views of gout and its treatment, except the claim which he sets up (p. 42) to being the first to point out "that a low or depressed state of the system is favorable to the development of the gouty paroxysm." We believe the fact to be generally familiar to practitioners, and to have been noticed by writers on gout from the earliest times. Dr. Copland, in his learned Dictionary, when reciting the *predisposing* causes of the disease, includes the depressing passions and venereal excesses as "weakening nervous agency, and the functions of digestion and excretion;" and he assigns to these still greater force as *exciting* causes. "Cold seems to operate, partly by suppressing the excretions, and partly by depressing nervous power." All powerful mental emotions, whether exciting or depressing, will excite a paroxysm; but anger or vexation has this effect in a very remarkable degree. The ancients made anger to be the midwife of gout; and Cadogan considered vexation, in its wide signification, as one of his "three great causes of the disease." Besides these, Dr. Copland enumerates excessive bodily or mental labour; and in general, "whatever disorders the digesting and excretory organs, or suddenly impresses the nervous system." We quite agree with Dr. Copland in the *modus operandi* of these depressing causes; the disorder of the digestive apparatus which they induce having a tendency to generate lactic acid; and that of the deficient action of the excretory organs preventing the evacuation of the insoluble lithic acid thus set free, so as to occasion its accumulation in the blood. Hence the inference which Dr. Todd subsequently draws, (p. 73,) from the appearance of gout in low states of the system,—"that the morbid element of the disease may be present, independently of lithic acid," since, in the cases adduced by him, "this substance either did not abound, or did not exceed the normal quantity by a greater amount than may at any time be caused by a slight general disturbance,"—is entirely baseless. Dr. Todd has not proved that there was no accumulation of lithic acid *in the blood;* and there is a strong presumption that there was;—for in one of the cases mentioned, the individual was affected with Bright's disease of the kidney, and in others there was evident dyspepsia, resulting from an injudicious regimen.

Under the head of treatment we find little to remark upon. In regard to diet, Dr. Todd expresses views very similar to those advanced in our former article;—namely, that the azotized portion should be reduced as low as possible, consistently with the due maintenance of the bodily vigour; and that those kinds of food should be selected which are least liable to produce disorder of the digestive process. He lays considerable stress on the importance of promoting the secretions from the skin; on which point we fully agree with him; and he speaks highly, also, of the effects of copious diluents, not taken at the time of meals, but in the intervals. The following are his observations on the employment of colchicum; from which it will be seen that it differs entirely from those who attribute the greatest power of this remedy to its purgative effects. For ourselves we may say that, generally speaking, we agree with him; but that we have seen cases in which the most obvious benefit was derived

from the more violent effects of the remedy, when its milder influence seemed to have given no decided relief.

"It appears to me that colchicum may act in a twofold manner: first, *chemically*, by producing some change in the urinary and hepatic secretions, both of which it tends to increase in quantity and alter in quality; and, secondly, it acts upon the nervous system, causing more or less depression, and on the mucous membrane of the stomach and bowels, exciting nausea, or vomiting, or purging, either separately or together. If employed in such doses as will produce only its chemical changes, it will, in strong constitutions, most favorably modify the gouty paroxysm, and certainly shorten its duration. If on the other hand, it produce any of its irritant effects, it is likely to do more harm than good; and therefore the dose should be diminished, or the medicine abandoned, if nausea or purging should come on during its administration. I have no doubt that a large share of the bad repute of this medicine is to be attributed to the indiscriminate and careless manner in which it is often prescribed; and I would venture to suggest the following hints for the guidance of the practitioner in its employment.

"1. Colchicum should not be given in the asthenic form of gout. [?]

"2. Colchicum should never be given at the outset of a paroxysm, nor until the bowels have been duly acted on by mild purgatives.

"3. The first doses of the medicine should be very small; they may be gradually increased.

"4. Colchicum should be always administered at first uncombined with any other medicine, until the practitioner has satisfied himself that it is not likely to disagree with his patient. And, indeed, there is always a disadvantage in administering this medicine in combination with others; since it may become difficult, if not impossible, at times, to determine what effects should be ascribed to the colchicum and what to the other ingredients.

"5. It should not be administered so as to excite nausea, vomiting, or purging. These effects should be regarded as indicative of the unfavorable operation of the medicine.

"6. Colchicum may be regarded as acting favorably when, under its use, the urine is increased in quantity, a more abundant bile is discharged; when the fæces, though solid, are surrounded by mucus, and the skin secretes freely.

"7. The effects of colchicum should be carefully watched; as, like digitalis and other medicines, it is apt to accumulate in the system.

"The use of this medicine seems chiefly applicable to the sthenic form of gout, which occurs in robust constitutions, and in the prime of life; but it is almost inadmissible in persons advanced in years, who have had several attacks, and in whom the malady would seem too deeply rooted to be influenced by the temporary administration of this remedy." (pp. 104-6.)

Before quitting the subject of gout, there is a suggestion which we think it desirable to throw out as to the nature of its *materies morbi.* We have hitherto spoken of it as *lithic acid;* but it occurs to us as more probable (though we would not venture to propound the idea dogmatically) that *lithate of soda* is the real morbific agent. This appears to us to be indicated by the fact, that the substance named is that which is separated from the blood in gouty deposits; and still more by the known connexion of gout with biliary as well as with urinary derangements, and by the beneficial result of treatment directed to *both* these excretions. Under the influence of particular substances, as we have seen, *lithic acid* has a tendency to accumulate in the blood; and it seems to us quite possible that, so long as it retains its uncombined form, gout may not result. But if, by deficiency in the secretion of bile, *soda* also be allowed to accumulate, the two will combine and lithate of soda will be formed. We offer this hypothesis for the consideration of our readers.

It accords well with the fact that peculiar advantage, in the prophylactic treatment of gout, is derived from the occasional administration of blue-pill or other mild mercurials.

Dr. Todd next proceeds to consider the natural history of rheumatism; and there are several points in his view of it which strike us as important. To these, therefore, we shall direct the special attention of our readers. He prefers the use of the term rheumatic fever to acute rheumatism; because the amount of febrile disturbance is frequently out of all proportion to the articular affection, and indicates the independent action of some morbid element in the blood; whilst in an ordinary local inflammation the febrile disturbance is constantly proportioned to its extent and intensity. This, we feel assured, is a correct and valuable distinction. He then directs attention to the importance of the fact that a *rheumatic diathesis* may exist without presenting the usual phenomena of rheumatism; and that, in this condition, the heart may become seriously affected, without the constitutional state having previously attracted the notice of the patient or his friends.

"I have now so frequently met with instances of diseased heart in young persons not traceable to an actual paroxysm of rheumatic fever, who nevertheless showed evident marks of a rheumatic diathesis which had existed for a longer or shorter time, that I cannot but regard this state of constitution as a fertile source of those cardiac diseases which are met with in early life. (p. 111.) . . . . I have not met with any allusion to the occurrence of disease of the heart, under these circumstances, in any of the works that I have had an opportunity of consulting. It is evidently a point of great value in the clinical history of the rheumatic diathesis, not only from the obvious bearing it has upon a correct view of the pathology of the disease, but also in a practical point of view, as showing how important it is for the practitioner to be on the watch for the signs of this state of constitution; as no doubt, if it could be removed, many young people might be saved from the terrible consequences of organic lesion of the heart. (p. 115) . . . . . . The occurrence of cardiac affection, as a feature of the rheumatic state of the constitution, must surely be admitted to be completely opposed to and utterly inexplicable by the doctrine of metastasis, which supposes that the cardiac inflammation has been transferred from the limbs to the heart. The truth is that the *cardiac inflammation may be primary;* it frequently exists at the same time with the articular affection, and dates its origin from the same period, as it derives it from the same cause." (p. 117.)

In this last statement, Dr. Todd is supported by the authority of Dr. Graves; and by the experience, we doubt not, of numerous practitioners. After urging other considerations in favour of the constitutional origin of rheumatic disorders, as being clearly traceable to a diseased state of the nutritive processes, (a position which few, we think, would now question,) Dr. Todd inquires whether the affection of the joints is really to be considered as *inflammatory* in its character.

"We have no satisfactory evidence that the articular affections are truly of the nature of ordinary inflammation. The parts are certainly swollen, painful, and there is a considerable flow of blood to them; but they do not suffer even the effusion of coagulable lymph; much less are they the subject of those destructive and disorganizing processes which so often follow in the wake of a true inflammation. Nor is it the nature of a true inflammation to desert quickly one part, leaving it unimpaired, and to fasten upon another; and, after making a short sojourn there, to revisit its old abode, or to fly to some new region. It is true that, in some instances, the joints do not escape unharmed; but a slow derangement of their nutrition is induced,—which may go on for years and years,—which alters

the textures and even allows them to wear away, and which resists those remedies that usually check an inflammatory process. This, although often called inflammation of a chronic kind, is surely more like the slow and insidious working of a canker, which dries up or impairs the matter destined for the nourishment of the tissues.

"I do not wish to be understood as denying that the ordinary effects of inflammation may take place in rheumatic joints. I am bound to admit that some cases are on record which seem to partake of this nature, but such instances are extremely rare, and their occurrence does not invalidate my argument; on the contrary, here *exceptio probat regulam*. In these cases the poison has been more strongly attracted to a particular joint or joints, and others have suffered less; and the remarkable disposition which the rheumatic affection has to shift from joint to joint is absent." (pp. 135-6.)

Dr. Todd admits, of course, that true inflammation exists in the case of the heart; but he thinks that this may be induced by the constant motion of the organ, whilst in the state of irritation induced by the rheumatic affection; and that, if the joints could be freely used during the paroxysm, they would more frequently exhibit marks of destructive inflammation as a consequence of the rheumatic paroxysm. Our views of this subject must depend in part upon the import which we attach to the term *inflammation*. If we abide by the ancient definition, we shall find all its symptoms, heat, pain, redness, and swelling, present in the joint affected with acute rheumatism. But if we adopt the modern view, that it is a process essentially characterized by increase in the quantity of fibrin in the blood circulating through the part, and by the tendency to the effusion of this in the form of coagulable lymph, we may find it difficult to show, by examination of the joint, that such a change takes place in articular rheumatism, however acute its form. But the state of the blood seems to us unequivocally to indicate the existence of true inflammation *somewhere*. We are surprised that, on such an important topic, Dr. Todd should not have referred to the elaborate inquiries of M. Andral, instead of contenting himself with the following vague statement: "It is generally stated that the proportion of fibrin in the blood is much increased. As to the proportion of the red corpuscles, I should say that it must be considerably diminished." (p. 122.) He compares the state of the blood in rheumatism to that of anemic patients; thereby leaving it to be inferred, that the buffy coat is to be attributed to the same cause as that which produces it in *them;* namely, a diminished proportion of red corpuscles, rather than an increase in the fibrin. But what are the results of M. Andral's inquiries? Why that in acute rheumatism (a malady which, he remarks, differs greatly from ordinary inflammations, and yet agrees with them in this important particular,) the proportion of fibrin increases from $2\frac{1}{2}$ or 3, to 4, 5, 6, 7, 8, 9, or even 10 parts in 1000; in the sub-acute form of the disease, it oscillates between 4 and 5; whilst in decidedly chronic rheumatism, it returns to its natural standard. Now we can quite imagine it as *probable* (though we do not see that it is *proved*) that the real *inflammation* is but secondary to the process in which the rheumatic affection essentially consists; just as inflammation may be secondary to tubercular deposit. But we think it an unwarrantable assumption that no inflammation exists in acute articular rheumatism. Moreover, in rheumatic inflammation of the eye—a disease of whose

existence Dr. Todd expresses a doubt (p. 110), but which, from our own experience, we should say is just as evidently connected with the rheumatic diathesis as is affection of the heart, the deposition of coagulable lymph can be seen, and its subsequent absorption watched.

Dr. Todd then goes on to argue that the form of rheumatism termed *capsular* by Dr. Macleod is really to be considered as a modification of gout; and then proceeds to inquire into the nature of the morbid matter, to the presence of which in the blood the rheumatic diathesis is to be attributed. After the objections (invalid as we regard them) which he has adduced against the doctrine, that lithic acid is the *materies morbi* of gout, we are surprised to find him adopting, with little reserve, the idea of Dr. Prout that lactic acid stands in this relation to rheumatism. Without denying that it may be so, we shall only remark that the symptoms are by no means fully explained upon the supposition; and that, in particular, the absence of relief by the copious acid perspirations, which so often spontaneously break out during the attack, seems to indicate that the elimination of lactic acid from the blood does not draw off from it the morbid matter to which the affection is really due. Yet we would not deny the possibility of the accumulation of lactic acid in the blood being so great that not even these copious acid perspirations materially diminish it; and such an idea derives force from the beneficial result of the treatment of the hot-air bath, when so employed as to produce a vastly increased secretion from the skin. To this point we shall presently recur.

An interesting chapter then follows, on the connexion of rheumatism with uterine derangement, in which a considerable amount of evidence is adduced, both from direct observation and from analogy, to prove that the accumulation of rheumatic matter in the blood may be a not unfrequent result of defective uterine action. Drs. Locock and Rigby are cited as having observed rheumatic affections to be not unfrequent accompaniments of certain forms of dysmenorrhea. Additional evidence to this effect is brought forward in the succeeding chapter on chronic rheumatism of the joints, in which that peculiar alteration of the osseous texture and synovial membrane, which has been so well described by Mr. Adams of Dublin, is particularly adverted to. Some doubt exists as to whether this disease can strictly be said to be rheumatic in its character, for it is scarcely questionable that falls upon the great trochanter have given rise to the first symptoms of the disease, as was the case, we believe, with Charles Matthews, whose hip-joint we remember to have seen not long after his death. But, as Dr. Todd justly remarks, "this is by no means improbable; nor is the fact opposed to that view of the disease which assigns to it a rheumatic origin, for doubtless the perversion of nutrition, excited by the violence of the fall, would, as often happens in gout, occasion a greater attraction of the rheumatic matter to the injured joints, than would otherwise have taken place." In these cases, too, that symmetry is for the most part absent, which, as Dr. W. Budd has shown, is more remarkable in the purely constitutional forms of this disease, than in almost every other; the affection often repeating itself in the corresponding joints of the two sides with the most perfect exactness, and the distortion which it produces being identical.

Under the head of Treatment, we have little remark to make upon Dr. Todd's views. Consistently with his idea of the pathology of rheumatism, he trusts more to evacuants, which act by increasing the natural excretions, than to any specific remedies, or to depleting measures; and we believe that, although in particular cases, large bleedings may be indicated by the plethoric state of the system, (which, we may remark, is more frequently the case in the provinces than in the metropolis,) and although some practitioners have dwelt much upon the benefits derived from large doses of opium, Dover's powder, camphor, colchicum, calomel, and opium, &c., the most generally successful plan is that which Dr. Todd recommends, namely, a moderate bleeding at the commencement, serving to unload the vessels and to promote the action of medicines, and a combination of sudorifics, purgatives, and diuretics, especially such as are most efficacious in drawing off fluid from the system; diluents being at the same time freely allowed, in proportion to the patient's desire for them. We are much inclined to believe, from evidence which has recently come before us, that too much attention cannot be given to promoting the action of the skin in both the acute and chronic forms of this disease; and here the results of practice and those of theory coincide to a remarkable extent. For we know that the skin is the principal channel for the elimination of lactic acid from the system, and this especially in the rheumatic diathesis; if, therefore, lactic acid be the element which it is most desirable to remove from the blood, a further increase in the action of the skin would seem to be the obvious mode of effecting this. In so far, therefore, as the hydropathic system of treatment answers this end, we believe that it will be found an efficacious method of treating rheumatism. Several cases have come to our knowledge, in which great benefit has been derived from it, in the most obstinate chronic forms of the disorder; and the modification of it, which is employed by Dr. Freeman at Cheltenham, seems to us more desirable than the original plan of Preissnitz. The copious perspiration is induced, not by wrapping the patient in a wet sheet for five or six hours, but by placing him in a stove, heated up to 160° or 180°. There he remains, drinking as much fluid as he desires, until a copious perspiration (leaving behind three or four pints of fluid in the blanket that closely envelopes the body, and the oilskin on which the patient lies) breaks out; after which the patient takes the cold plunge, *à la Russe*, which we understand to produce the most delightful sensations. Such a plan would seem admirably calculated to draw off lactic acid, or any similar offending substance, from the blood; and of its utility we have had convincing testimony, particularly as regards the chronic forms of the disease, in which the structure of the joints is undergoing alteration. It has the beneficial effect of not only drawing off the *materies morbi*, but of preventing its re-formation, for the plan seems to operate most beneficially upon the digestive system, improving the appetite, strengthening the powers of the stomach, and correcting the secretions.

In closing our notice of Dr. Todd's work, we should be wrong if we were not to add that, although we have felt bound to express our difference from him on several points, and especially in regard to the degree of novelty which some of his views may claim, we can safely recommend his

treatise to the student and junior practitioner, as containing much that they will not meet with elsewhere, and as suggesting to them many topics of inquiry, by following out which they may themselves do something to advance the knowledge of these most important diseases. We fully agree with Dr. Todd in the greater part of his general views. No one can be more alive than we desire to be to everything which can tend to the advance of medical science; and we believe that the reconstruction of the humoral pathology, upon the basis that will be afforded by organic chemistry, and by an improved mode of analysing disease, will be the most certain means of accomplishing this important end.

Art. XV.

1. *Allgemeine Krankheitslehre.* Von Dr. K. F. H. Marx, ordentlichem Professor der Medicin in Göttingen, &c.—*Göttingen,* 1833. 8vo, pp. 273.

General Pathology. By Dr. K. F. H. Marx, Ordinary Professor of Medicine at Göttingen.—*Göttingen,* 1833.

2. *Grundzüge zur Lehre von der Krankheit und Heilung.* Von Dr. K. F. H. Marx, &c.—*Carlsruhe und Baden,* 1838. 8vo, pp. 447.

Elements of Pathology and Therapeutics. By Dr. K. F. H. Marx, &c.—*Carlsruhe and Baden,* 1838.

The medical sects of Germany may be arranged in four or five principal divisions: First, there is the metaphysical, fanciful, and purely theoretical school, peculiar almost to Germany, and giving its hue to all the rest. Second, the physiological or biological. Third, the bio-chemical or material. Fourth, the Hippocratic, empirical, or expectant. To the first division belong the Kantian physicians, the homœopathists, hydriatics, Brownists, and all who deduce details from some general principle or principles, including a host of philosophers whose principles are dreams and visions. Those in the second division, without neglecting chemistry and natural physiology, seek in philosophical anatomy and the laws of transcendental physiology for the true basis of pathology. Schultz, whose work is reviewed in our last Number, is an example of this school. The third is nearly allied to the preceding, but differs in making physiology subservient to organic chemistry and natural physiology. Liebig is the type of this class. The fourth is rather learned than scientific, and is professedly eclectic. The results of the science and observation of past times are not neglected although old, nor are the researches of the moderns despised because they are new. The physicians of this class are, however, rather obstructive than progressive, rather readers than doers; and more inclined to analytical criticism than to experiment. Göttingen may be considered the head-quarters of the fourth class, central Germany of the third, Berlin of the second, and southern Germany, especially the secondary schools, of the first. The Göttingen school approaches the nearest of any to the English, being less outrageously speculative, and more inclined to matter of fact than the others. We speak, of course, generally, for we are quite aware that there are many zealous cultivators of medical science in every part of Germany, who are as practical and as averse to mysticism as ourselves.

The perusal of the works of Professor Marx, and our own knowledge of his intellectual character, would induce us to place him in the fourth of the divisions we have just mentioned. His *Origines Contagii*, and his critical analyses of the doctrines and writings of Herophilus and Paracelsus show very great literary attainments, and an astonishing extent of medical reading; while the works before us exhibit the results of the peculiar line of research into which the intellectual tastes and habits of the author have led him.

The work first mentioned contains a concise and systematic view of the various theories of medicine extant, and comprehensive definitions of the technical terms to which those theories have given origin. It is professedly elementary, being intended for the use of his own class, and as an introduction to the study of medicine. It contains much information in little compass, and is carefully and accurately written. We would strongly recommend its perusal to the more advanced English student who wishes to improve himself in the German language, and at the same time acquire, at little cost of time or money, a clear and exact knowledge of medical theories unencumbered by digressive details,

The Principles of Pathology and Therapeutics is a work of higher pretensions than the preceding, being written for the accomplished physician, and professing to bring the fundamental doctrines of medicine into connexion with the most recent anatomical, physiological, and pathological researches, and with the results of the allied sciences. In doing this, our author professes to avoid all merely controversial topics and to exhibit the art of healing to laymen such as it really is, a thing, not of science and experience merely, but of conscience and earnest conviction. The following paragraph from the introduction exhibits the spirit of eclecticism with which our author is imbued. It will be observed that he professes to steer between the doctrines of the first and third school we have already defined.

"Those who look upon the human body as a mere machine kept in motion by chemical affinities and physical forces, feel themselves compelled to seek for the cause of deranged function and death in disordered affinities, or interrupted processes. On the other hand, they who contemplate the body as being only the outward and visible part of the soul,—as the symbol of spiritual force,—ascribe interrupted function to disorder of the inner life;—to a continuous alternation of relations between the inner exciting force and its external results;—to an impaired action of the soul on the body;—to a yielding of the will, the feelings, and the thoughts to matter; or to a perverted, or a too strong or too weak condition of the nervous system, the medium of all action. Each party seeks to explain the phenomena of disease according to its own favorite views, and feels obliged to develop the guiding ideas in logical coherence with the facts to be explained. This strife is equally natural and praiseworthy. But the fancies of individuals cannot be considered as genuine science, for genuine science is simply the result of ideas clearly demonstrated and communicated. Notions assumed without proof peril its very existence, or at least, subjective opinions must be considered as far inferior in value to objective certainty, so that science necessarily refuses to receive mere probabilities or beliefs within its pale, and justly accepts inductions from facts only." (p. 9.)

In looking over the early pages of our author's volume, we remark less of that severe induction and adherence to facts than might have been expected. Real facts, and especially general facts, are yet *desiderata* in medicine. The plan and scope of his work being an exposition

of principles, may, perhaps, in some degree account for this, but we think it will not excuse the grave exposition of acknowledged axioms. We know well that the intellectual genius of his nation, (and of this, the grammatical construction of its language is a proof,) is diametrically contrary to our own, and that an exposition of first principles, and not an exhibition of particular facts, is the usual commencement of a German treatise. It is not long since we took up a German writer on the Races of Mankind, and found that he considered it necessary to commence his essay with a definition and analysis of the Trinity. Nothing so absurd as this is to be found in Professor Marx's work; but he gravely states principles which would have been considered mere truisms by an English writer, addressing himself to the educated practitioner. In considering prophylaxis, for example, he would not have stated in so many words, that a knowledge of the first beginnings and primary links in the chain of morbid causation is requisite to prevention. And in considering hurtful influences and their removal, it is a matter of course that things may be prejudicial to the close student, which are useful, or at least not injurious to the hard-working, unthinking, unanxious rustic. We do not mean to say that every practitioner bears this difference in mind, for some would deplete the sensitive and highly organized man of genius as freely as the clown; but certainly every judicious practitioner remembers it, and treatment to the contrary is rather the result of indolent routinism, than want of proper knowledge. It is true some English writers have found a mare's nest, and published diatribes against injudicious depletion, on the supposition that all the world had been as tardy as themselves in discovering that routinism was not skill. This circumstance can only be said to prove the ignorance of the authors, and not of the educated part of the profession.

Professor Marx's history of the evolution of disease exhibits much thought; and, though the views are not exactly novel, are worthy the prominence given to them, as they are too little considered by the generality. A disease may really commence with conception, but remain dormant until, in the progressive evolution of the body to perfection, or in its gradual decline, the circumstances occur favorable to the manifestation of the inherent morbid action. This inherent predisposition to disease and its results may be seen in a phthisical family, the members of which are cut off in regular succession as they attain a certain age. Paralysis, insanity, and other diseases may be observed to follow the same law of development in families. The following is a specimen of our author's style and matter:

"The hereditary morbid predisposition is apparent at an early age to the scientific observer; to the ignorant it appears only when the time of its outbreak has come, that is to say, when awakened into activity by injurious agencies, or when the organ in which it had laid dormant comes into full action. Hence the inveteracy and incurable nature of the diseases arising from these causes. A disease of this kind is not so simple as it appears. It is as old as the individual, and sympathises with him in all that he passes through. It is not merely as if a seed lay dormant in an organ, the husk of which need only be burst open to allow it to germinate; the whole organism feels its presence, and is too often in danger, not to be aware that it incloses an unfriendly influence. And this unfriendly influence operates more actively than is usually supposed. The development of the body, nay, even the mental organization, is often determined by it." (p. 30.)

In treating of the susceptibility to disease, it is remarked that there is in this respect, as in others, a sympathy and an antipathy, apparently regulated by certain obscure laws of affinity, intermediate in character between the psychical and the physical. Just as a certain degree of temperature determines whether bodies shall combine mechanically or chemically, so in physiology and pathology there must be similarity and dissimilarity, or more properly a sort of polarization. A person will die of smallpox at an advanced age, in whom all attempts to excite the disease in youth had failed. A woman may be many years married and yet childless, but becoming a widow and remarrying to an individual, very similar to or dissimilar from herself, she speedily becomes pregnant. There are other interesting observations under this head we would gladly have cited if our space would have allowed.

The causes of diseases, and their usual divisions into remote and proximate, predisposing and exciting, external and internal, are noticed; as also the influence of the different periods of life and their physiological condition. The fundamental requisites for the cure of disease, the nature and development of the latter, and the general curative indications, are commented on in succession. A considerable space is occupied with an interesting and detailed notice of the predisposing causes of disease, as they are psychical, structural, or physical. The pathology of the passions, temperaments, and idiosyncrasies; of the solids, of the blood, and the secretion, and of the functions of organs; the pathological relations of kosmic and telluric influences, and of air, light, sleep, food, clothing, baths, poisons, contagions, and infectious matters, are all successively reviewed. Under the head of "Time and space in relation to disease," the effects of climate and soil, and the pathology of endemics, the influences of the diurnal periods and of the seasons, and the pathology of epidemics, the stages and periods of diseases, and the doctrine of critical days—are all noticed.

Professor Marx arranges his therapeutical agents in three principal divisions, as they are strengthening, debilitating, or alterative. In the first division the excitant, irritant, tonic, and astringent classes of remedies are considered. In the second we find anodynes and antispasmodics, and the dietetic or moderately antiphlogistic, and the actively antiphlogistic methods of treatment. The third comprises the evacuants, namely, emetics, purgatives, diuretics, diaphoretics, and expectorants, the derivatives, and a class of remedies which may be termed alterative tonics, comprising metallic tonics and alteratives, the hunger-cure, travelling, and spa-ing.

As all this matter is condensed in fewer than 450 pages, any further condensation is impossible; and as so much of it is valuable, we feel an *embarras des richesse* in attempting a selection. The following exposition of the much misunderstood hematophobia of our German brethren will remove some prejudices:

"Blood is drawn in full stream from a vein, or by leeches or cupping-glass from the skin, according to the nature, extent, and intensity of the disease, and the constitution of the patient. The one is termed general, the other local, bloodletting. General bloodletting is to be practised without reference to the age of the patient, when there is inflammation of any important tissue, and a full and hard pulse; or

even when the pulse is compressed, provided its smallness is unconnected with any cardiac disease, and leaves no doubt that the central organs of the circulation are overflowing with blood. Local bloodletting is advisable when the seat of the disease is easily accessible; when the inflammatory action is limited; when general bleeding has been already practised; when there is a want of power in the system; or when a cachectic state of the system renders general bleeding dangerous; and when local depletion is alone necessary, as in suppressed constitutional hemorrhages. Cupping affords the same immediate relief in external inflammation, as general bleeding affords in internal, or in visceral congestion. The pressure of the cupping-glass has an influence on the capillary circulation, as has also the bleeding after the removal of leeches. The nearer to a suffering organ blood can be drawn, the better. The so-termed derivative bleedings are more effectual than the revulsive. The quantity of blood proper to be drawn in each case cannot be estimated by the eye or a measure. It must be determined by the symptoms, constitution, and age of the patient, and the nature of the disease. In general, from six to eight ounces is a moderate bleeding for an adult; from twelve to sixteen ounces may be considered a copious abstraction of blood. The loss from a leech-bite may be estimated at from three to four drachms for every half-hour the bleeding continues. The loss by cupping depends on the repetition of the scarification, and the size of the glasses. The bad consequences of a copious loss of blood appear either immediately or in a short time. They are fainting, noises in the ears, dimness of sight and even blindness, sudden startings from sleep, delirium, gasping for air, prostration of strength, convulsions, stupor." (p. 364.)

The general rules for determining the quantity of blood to be drawn are thus laid down:

"The extent to which bloodletting should be practised is determined by the urgency of the symptoms, by the importance of the structure affected, by the number of blood-vessels in the inflamed viscus, by the habits of the patient with regard to bleeding, and by the nature of the disease. The greater the inflammation, the more freely must the blood be taken, allowing it to flow until the hardness and fullness of the pulse be abated and the pain relieved.........If other circumstances indicate bloodletting, the presence of the menses, lochia, or hemorrhoidal flux do not contra-indicate the operation. It is of more importance to consider whether the tissue affected be not intolerant of any considerable loss of blood, as for example, the mucous tissue; or whether the heart itself be not affected, and the indications for bleeding consequently appear more urgent than they really are; or whether the blood itself be not in a state of irritation and expansion, as in puerperal or the bilious fever." (p. 366.)

Our author very correctly observes that nothing can be learnt from the buffy coat as to the necessity for further bleeding. He observes that it is altogether unconnected with the nature of the disease. The size of the opening in the vein, the depth, width and polished surface of the basin, and the temperature of the air at the time, all influence the rapidity of the coagulation of the blood, and of course, the thickness of the buffy coat.

There is a degree of conciseness and elegance in Professor Marx's style which is pleasing. The book is more like Gregory's Conspectus, in all respects, than any other work with which we are acquainted.

Art. XVI.

On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the Connexion of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By William Prout, m.d. f.r.s. &c. Fourth Edition, revised.—*London*, 1843. 8vo, pp. 593.

The author's own preface to this new edition of his valuable work, will best explain the views with which he has prepared it:

"The present edition is essentially the same as the last. The chief alteration consists in the arrangement of the introduction, which now constitutes a Third Book. As an introduction, this part of the volume was already too long; and as I could not add a few necessary remarks without rendering it still more unwieldy, I was induced to make the change in question.

"Since the third edition was published, Professor Liebig's treatises on Vegetable and Animal Chemistry have made their appearance, and attracted no little notice. Some of the views advanced by this distinguished chemist in his last work are the same I have long advocated. Others of his views are directly opposed to mine, and seem to me to be neither susceptible of proof, nor even probable. The *practical* nature of this volume, however, precludes all controversy, particularly on matters of no practical utility; and I allude to the subject, chiefly for the opportunity of observing, that having in the following pages stated my own opinions without reference to Professor Liebig, I leave it to the public to decide whether he or I have most nearly approached the truth.

"There is another point also connected with this part of the subject, which requires a few remarks. I have purposely omitted the formulæ, now so much in fashion among chemists, not only because I consider them clumsy and unphilosophical as conventional expedients, but because I am satisfied that very few, if any, of them, represent the true constitution of organized substances. A grand clue to many chemical phenomena will be found among the multiple relations of what are termed the atomic weights of bodies. After nearly thirty years, chemists have reluctantly admitted the existence of such relations among the four constituent elements of organized bodies. Another generation, I have no doubt, will recognize and admit the important consequences to which these relations lead." (pp. vii-viii.)

On the three paragraphs composing this preface, we shall offer a few remarks before proceeding further. We quite agree with Dr. Prout in the desirableness of converting his introduction into a book, on account both of its bulk and its importance; but we should have thought it better to allow it to retain its previous position at the commencement of the volume, instead of transferring it to the end. The student must necessarily acquaint himself with the physiological or normal actions of the body, or of any portion of it, before he can rightly comprehend its pathological or abnormal conditions; and in Dr Prout's treatise this order appears to us to be particularly required, since so large a part of his views on the disordered action of the assimilating and secreting organs, are positively incomprehensible without a previous acquaintance with his ideas of their physiological connexion. We strongly recommend the readers of the present edition, therefore, to reverse the author's arrangement, and to study the last book first.

We also quite agree with Dr. Prout in the undesirableness of introducing controversy into a practical work of this kind; nevertheless, we think that it would not have been amiss to have inserted a few references to Liebig's peculiar opinions, as a guide to the student in the comparison

of them with Dr. Prout's views. A good deal of trouble might have been thereby spared to those who, like ourselves, desire to become fully acquainted with the points at issue between these two distinguished chemists. We trust that Dr. Prout may see the desirableness of publishing, in a separate form, and with more amplification, his opinions on these controverted topics, particularly specifying the evidence on which his own views are founded. Professor Liebig's work is almost entirely of an argumentative kind; the data on which his reasonings are founded are for the most part specified; and thus every reader, possessing a competent knowledge of the subject, can form his own opinion—from his knowledge of the probable truth or error of the data, and from his estimate of the logical precision of the reasoning,—as to the value of the conclusions drawn and set forth by the author. In Dr. Prout's treatise, on the other hand, there is more of assertion, and less of even attempts at proof, the data being, for the most part, locked up in the author's own laboratory; and until Dr. Prout shall see fit to give them to the public, he must be content to have his opinions freely questioned, and the accuracy of his conclusions suspected. We earnestly hope that he may be induced to publish the results of his laborious inquiries, in such a form as may obtain for him that rank amongst organic chemists, to which we feel assured that he is justly entitled.

We are glad to find that Dr. Prout so fully agrees with the opinion we expressed in our last Volume (p. 507), in regard to the fashionable system of *formulæ*. Our remarks were addressed to what we conceived to be their misuse. To their great utility, in a great number of cases, we freely bear testimony. But we cannot place the full confidence in them which some entertain, for the following reasons—1. The atomic weights or combining equivalents of many of the simple or elementary substances are far from being indubitably ascertained. Take that of carbon for example, a correct determination of which is so important in regard to others. Almost every chemist has been in the habit of estimating this at a fraction above *six*. The result obtained by Dr. Turner in 1833, from a series of experiments of which the elaborate accuracy commanded for them the highest regard, seemed to fix it positively at 6·12. Yet Dr. Prout tells us (p. 556, note) that he long ago settled to his own satisfaction, by numerous most careful experiments, that the combining weight of carbon is neither more nor less than 6; this conclusion is the one recently arrived at by Dumas, after a very elaborate series of experiments, and it seems to be gaining ground amongst chemists. We do not offer an opinion as to the correctness of either of these numbers; but we simply say that, until the claim of one of them to adoption is placed beyond all question, no great confidence can be placed in formulæ.—2. Notwithstanding the great improvements which have been made during the last few years, especially by Liebig, in the analysis of the organic proximate principles, there is still a great degree of uncertainty in regard to the exactness of the results obtained. "I know at present," says Dr. Prout, (loc. cit.) "of no apparatus, or means of operating, capable, when azote is concerned, of unequivocally deciding about the presence or absence of *one proportiôn* of hydrogen or even of oxygen in a complicated body. Liebig's analytic apparatus was in effect tried by me twenty years ago; and for rude approximations it answers very well; but

it is not, in my opinion, at all adapted for obtaining very accurate results." Many of those who can reason most acutely upon formulæ, and show to a nicety how every atom of oxygen, hydrogen, carbon, and nitrogen, is disposed of, in the disintegration of albumen and gelatin, are unaware how much room there still is for questioning the results of the analyses on which those formulæ are based.—3. Even supposing that the absolute number of atoms of each of the elements making up an organic compound, were ascertained by a perfect analysis, still the formula must be considered as far from representing the real state of combination. We have no reason to believe that, in any instances, four sets of atoms unite with each other in the manner which we should be thus led to suppose; and we cannot but think that the real constitution of protein is not represented by the formula O. 14 + H. 36 + C. 48 + N. 6, one whit better than that of sulphate of potass would be by the formula S. + K. + O. 4. The inorganic chemist well knows that the constitution of the latter, according to the usual mode of representing it, is (S. + O. 3) + (K. + O.); or according to the present views of the constitution of salts, as consisting of a base directly united to a compound radical (S. + O. 4) + (K.) Each of these formulæ represents a *fact* in regard to the arrangements of the elements of which the compound is made up; the first expresses that it is formed by the union of sulphuric acid and potash, into which it may be again decomposed; the second expresses the doctrine, now generally received amongst chemists, that, when the combination of these two bodies is effected, their elements are newly arranged, so that the base potassium is united with the compound radical sulphatoxygen. Now until we are able to resolve the complex formulæ, by which the constitution of the quaternary organic compounds is now expressed, into other and simpler ones, which shall express, with some appearance at least of probability, their real constitution, we think that Dr. Prout is fully justified in his objection to their use, as expressions of facts, more particularly in consequence of the abuse to which the employment of them is evidently liable. That they may be of great utility as *guides to research*, we freely admit; but, like many similar expedients, their use is temporary only; and we cannot regard deductions from them as satisfactory, until the conversions which they are supposed to indicate have been either effected in the laboratory of the chemist, or have been shown, by pretty clear evidence (physiological and pathological), to take place in the living body.

In our critical review of the former edition of Dr. Prout's treatise, we entered into a pretty full examination of his peculiar views; and the following was our general conclusion :

"We acknowledge and have pride in bearing testimony to the high qualifications of our countryman in the branch of pathological inquiry based upon chemical facts; we recognize the comprehensive sagacity of his speculations, and have respect for the patient zeal with which he has toiled to erect upon these a stable system. But we fear the time for such systematizing has not yet come; and although all speculations on the subject are seductive in themselves, and doubly so when emanating from an individual of Dr. Prout's eminent skill in the department of chemical physiology, it cannot, we think, be denied that in the existing unformed and vacillating state of organic chemistry, they sin essentially in being established on a most unsound basis. Nor can we avoid entertaining some solicitude as to the results of their propagation, which to us appears likely to betray

minds of inferior order into mere extravagances. For these, however, Dr. Prout is not fairly answerable; and should his doctrines—when the frail embryo science on which they are based has reached healthy maturity—be recognized as true, he must almost take rank with those highest intelligences, whose energy has outrun the scientific apprehension of their times. But meanwhile Dr. Prout has neither done his doctrines, himself nor his readers justice in not explicitly stating the foundation for and manner of verifying (so far as he is acquainted with these himself,) his presumed results." (Vol. XI. p. 363.)

We have learned with regret that our criticism, candid and honest as we maintain it to have been, gave pain to the eminent author of the work; and we have reverted to it on this occasion, with the earnest desire to repair, so far as lies in our power, any injury we may have unintentionally inflicted by errors of omission or of commission. When a man of high repute puts forth his opinions *ex cathedrâ* on important practical questions like the present, it behoves those who occupy a position like ours, to exercise (if that be possible) a double measure of their usual critical acumen; in order that they may prevent errors, sanctioned by the authority of a great name, from gaining that prevalence which, when brought forward by those of less note, would have comparatively little injurious influence. It was on this account that, without any desire to depreciate what we imagined to be the universally acknowledged merit of Dr. Prout's labours, we addressed ourselves to the consideration of those points, on which, as we then conceived, and still believe, he had failed in establishing the positions he advanced. But, if disposed to criticise our own criticism, we should now say that, without admitting it to contain any serious error of commission, it ought to have contained a higher tribute than it contains, to what we may regard as the general and distinguishing merit of Dr. Prout's treatise,—the object to which his whole life has been devoted,—namely, the exposition it contains of the important connexion between a large number of disordered states of the urinary secretion (and these the most important) and disordered states of the processes of digestion and assimilation. For the clear and positive idea of this connexion, not only in one or two diseases, but in regard to a large number,—an idea, too, of the most extensive application in regard to the whole physiology and pathology of assimilation and excretion,—we believe that we are mainly indebted to Dr. Prout. And whether the results of future inquiries shall or shall not confirm his *particular* doctrines, we desire to record here our deliberate conviction that the *direction* was first given to those inquiries by Dr. Prout, and that the physiologist, the pathologist, and the practitioner, ought therefore to feel themselves lying under a debt of gratitude to him, which no errors or imperfections in the details of his labours can efface.

With the view of more strongly impressing Dr. Prout's merits, in this respect, on the minds of our readers, we shall offer them a brief summary of his general doctrines, and of their most important applications; pointing out some of the leading differences between his views and those of Liebig.

The conversion of alimentary materials into organized tissue is regarded by Dr. Prout as divisible into two stages, to which he gives the name of *primary* and *secondary* assimilation. Under the term *primary* assimilation he includes all the changes which the food undergoes, from its entrance into the stomach up to its conversion into the materials of

blood,—or in other words, up to the time of its possessing an organizable condition. By *secondary* assimilation he designates the conversion of the organizable materials of the blood into organized tissue; and under the same head,—not we think without some incongruity,—he classes the subsequent *destruction* of these tissues, and their resolution into other compounds, which are destined to be excreted.

Dr. Prout does not regard the digestive process as one of simple *reduction* and *solution;* but believes that an actual *conversion* of one form of alimentary matter into another may be effected by it.

"Two, indeed, of the chief materials from which chyle is formed, namely the albuminous and oleaginous principles, may be considered to be already fitted for the purposes of the animal economy, without undergoing any essential changes in their composition; but the saccharine class of aliments, which form a very large proportion of the food of all animals, except those entirely subsisting on flesh, are by no means adapted for such speedy assimilation. Indeed, one or more essential changes must take place in saccharine aliments, previously to their conversion either into the albuminous or the oleaginous principles......... We cannot trace the conversion of sugar into albumen, because we are ignorant of the relative composition, and of the laws which regulate the composition of these two substances. The origin of the azote in the albumen, is likewise at present unknown to us, though in all ordinary cases it seems to be appropriated from some external source. That the oleaginous principle may be converted into most, if not all, the matters necessary for the existence of animal bodies, seems to be proved by the well-known fact, that the life of an animal may be prolonged by the appropriation of the oleaginous and other matters contained within its body......... Under ordinary circumstances, then, the converting powers of the stomach must essentially consist of the three kinds mentioned, viz. the conversion of saccharine aliments into albuminous and oleaginous principles; and the conversion of oleaginous into albuminous principles." (pp. 470-1.)

Now we consider it to be a question of the very highest moment, to ascertain how far this doctrine of *conversion* is well founded. Our readers are probably well aware that it is admitted by Liebig and his followers only, in regard to the *azotized* and *non-azotized* articles of food considered separately;—that is, they consider albumen as capable of transformation into gelatin, horny matter, or any other azotized compound, and also, by a completely new arrangement of its elements into oleaginous matter; and again, they regard the saccharine principle (including *starch* in all its forms) as capable of transformation into the oleaginous; but they completely deny the possibility of the transformation, under any circumstances whatever, of the saccharine or oleaginous principles into the albuminous. By Dumas and his followers, (who have been recently carrying on a hot controversy with Liebig on this question,) it is denied that even the conversion of saccharine into oleaginous principles ever takes place in the animal body. The saccharine principles are by them regarded as entirely disposed of by respiration, or by conversion into lactic acid; and the oleaginous principles as being carried out of the system by the biliary and other excretions, as well as by respiration, as fast as they are introduced into it,—unless deposited as fat.

Although these doctrines of the continental chemists have been received as valid by many high authorities, we think that much more extended inquiry is necessary, before they are entitled to rank as ascertained facts; and we hold, with Dr. Prout, that although the conversion of sac-

charine or oleaginous matter into albuminous may not be a part of the ordinary process of nutrition, when an animal ingests a proper proportion of the several alimentary principles, it *may* take place *to a limited extent*, when such a conversion is required for the supply of the wants of the system. It seems to us to be the duty of those who deny *in toto* the possibility of this, to explain the following facts, or to show that they are incorrectly stated. 1. However large may be the proportion of saccharine matter in the food, it is not to be detected in the healthy state, either in the chyle or the blood. In what state, then, is it received into the circulation? By Liebig and his followers it will be said that it is converted into oleaginous matter. Granted; but then, 2. By microscopic-chemical examination of the chyle it seems clearly ascertained, that the proportion of oleaginous matter in the chyle, which is usually very large in the peripheral lacteals, gradually diminishes; and that the proportion of fibrin, and probably that of albumen also, gradually increases, until the composition of the fluid approaches more nearly to that of the blood. This seems to us to indicate the continuance of the converting process, which, according to Dr. Prout, commences in the stomach and duodenum. The only positive evidence which he adduces to this effect, however, is contained in a note (p. 504); in which he states that he has "constantly found albumen developed in abundance in the duodenum of animals, whether the food contained azote or not." Now we would earnestly request Dr. Prout to give to the world the precise data on which he founds this statement; and to repeat his experiments, if requisite, in such a form as to leave no doubt of the absence of azote in the food, and the presence of albumen in the duodenum. At present they lie open to the objection, that the articles likely to have been employed, such as starch, sugar, &c., may have contained a small quantity of azote. Yet we think that by far too much importance has been attached to this circumstance by the chemists previously referred to. For the question is *not*, whether rice, potatoes, cassava, and other forms of starchy matter that constitute the staple food of many races of men, contain azote, but whether the azotized principle exists in them in an amount sufficient to supply the wants of the system. For ourselves, we cannot believe that this is the case.

There is another form of the nutritive process, which we consider of great consequence in determining this question, and on which it would not be difficult to make conclusive experiments; we refer to that which presents itself in hive-bees. The case has been already adverted to by Liebig, as proving that the saccharine principle is capable of being converted into the oleaginous; since bees are well-known to be able to produce wax, when shut up in their hives, and fed with pure sugar. But we wonder that it did not occur to Liebig, that, by parity of reasoning, the saccharine principle must be capable of conversion into the albuminous. It is quite true that, in the expressed juice of the sugar-cane, maple, &c., there is present a small quantity of azotized matter, sufficient in amount to become a very active *ferment*; but it is the first object of the manufacturers to get rid of this; and we believe that in the finer kinds of brown sugar, and in all refined sugar, it would be very difficult to find a trace of azote. If bees feed upon this material alone, they will not only make honey and wax, but elaborate from it the materials of

their muscular fibre and other tissues, the question must be regarded as decided that animals can convert saccharine into albuminous matter; although it still remains an open question, to what extent this power may exist in the higher classes. We believe it to be a fundamental error in the physiological chemistry of Liebig, that he has studied animal life under a very few only of its phases; and that he has consequently been led to conclusions which a more extended survey shows to be invalid. We pointed out this formerly, in regard to his doctrine of the connexion between the biliary secretion and the respiratory process, which we showed to be completely inapplicable to insects and mollusca, (vol. XIV, p. 514;) and we have his own authority for stating that this view of the case had never occurred to him. In like manner, we cannot think that he would have advanced this doctrine of the absolute inconvertibility of the saccharine into the albuminous principle, if he had pondered upon the vast number of the insect tribes, and these, too, including the insects most distinguished by muscular activity, and therefore requiring (on Liebig's own theory) the largest amount of azotized nutriment, which derive their whole support from the saccharine juices which they imbibe from flowers. It is quite possible that these juices may contain azote; but it must be shown that they contain enough to replace the *waste* that must take place in the tissues of these active little beings.

Regarding the source of the azote thus added, Dr. Prout's opinion seems to us well worthy of consideration:

"The azote may, in some instances, be derived from the air, or *generated* (?) But my belief is, that, under ordinary circumstances, much of the azote employed in the assimilation of saccharine matter is furnished by a highly-azotized substance secreted from the blood, chiefly into the duodenum; and that the portion of the blood thus deprived of azote is separated from the general mass of the blood, either by the stomach in the form of lactic acid, or by the liver, as one of the non-azotized constituents of the bile; and that the lactic acid and non-azotized substance thus separated are ordinarily excrementitious." (p. 470, note.)

By Liebig it is supposed that one of the great purposes of the saliva is to carry down a quantity of atmospheric air, for the supply of *oxygen* supposed by him to be required in the process of digestion. Dr. Prout points out that *azote* may be thus introduced:

"The atmospheric air involved during the mastication and insalivation of their food is very probably another source of azote to vegetable feeders. Indeed this involution of azote may be considered as *one* of the great objects of mastication, &c., which is almost peculiar to animals chiefly subsisting on saccharine matters." (p. 504, note.)

We have dwelt the longer upon this topic, both on account of its intrinsic importance, and because we desire to uphold what we believe to be the correct views entertained by Dr. Prout, against the untenable assumptions (so at least we at present regard them) of continental chemists.

In abnormal states of the *converting* process, Dr. Prout looks for the cause of some of the most troublesome forms of indigestion, and for the foundation of the excess of sugar in the system, in diabetes. This excess of sugar (which has been proved beyond all doubt to exist in the blood) may be derived in part from the want of power to *convert* saccharine aliments, which are therefore absorbed in their original state. It would

also seem that conversion of albuminous or gelatinous principles into the saccharine may take place in the stomach, for it has been ascertained that sugar is formed in the stomach, even when the food is exclusively animal. Hence it probably is, that not even an exclusively animal diet is successful in preventing the production of sugar; and that, by the ingestion of a small quantity of saccharine matter, which may act as a kind of *ferment*, the quantity thus produced is liable to be very greatly increased. The practical remark of Dr. Prout, "I have known the use of a few saccharine pears undo, in a few hours, all that I had been labouring for months to accomplish," is probably familiar to most of our readers. We regret not to find, under the head of the dietetic treatment of diabetes, any reference to the gluten-bread, which has been introduced, since the publication of the previous edition of Dr. Prout's treatise, by M. Bouchardat—with the view of giving that variety to the diet, which patients long restricted to animal food almost imperatively demand, without doing injury by admitting farinaceous (saccharine) matter into the system. The efficacy of the complete restriction of the diet to azotized matter, which can be thus maintained for any length of time, (a thing extremely difficult of accomplishment with *meat* alone, owing to the absolute *craving* of the patient for something else,) has been spoken of in high terms in Paris; and it has been tried with success in London. As few if any persons have larger opportunities of making trials of this kind than Dr. Prout possesses, we beg to recommend the subject to his attention, and hope to be favoured with the results of his experience.

The power of converting the saccharine principle in the stomach may be *deranged* as well as suspended; and this derangement may give rise to the production of the lactic or oxalic acids in abnormal amount, occasioning their absorption into the blood and the numerous train of symptoms consequent thereon, which we need not now enumerate. We believe that Dr. Prout is perfectly correct in tracing the first of these, at any rate,—and probably the second, in a large number of instances,—to derangement of the *primary* assimilating processes, and especially to that of saccharine conversion.

In regard to the *secondary assimilating* process (strictly so called), our knowledge is necessarily more limited. It is quite possible that, in consequence of an imperfect elaboration of the organizable materials, or an insufficient demand for them, the chief constituents of the blood may be changed into excrementitious substances without even passing through the form of organized tissue; and we have elsewhere endeavoured to show that this *must* take place, in regard to azotized matter, whenever the quantity of it which is absorbed considerably exceeds the quantity that can be organized. But as to the products which are liable to be formed under such circumstances, we have no certain knowledge. It appears to us that Dr. Prout's idea, that the albumen of the blood may, by a derangement of the secondary formative assimilating process, be converted into urea and a saccharine principle,—instead of being normally converted into gelatin,—is rather an assumption than a proved fact. It would seem probable, however, that the results of this decomposition should be similar to those which substances of the same constitution produce by their disintegration, *after* they have passed through the form of organized tissue, in the process termed by Dr. Prout (rather incongru-

ously as it seems to us) *destructive assimilation;* and by Liebig, *waste,* or *metamorphosis.* In his opinion, the metamorphosis of the *albuminous* principles gives origin to lithic acid; and that of the *gelatinous* to urea and a saccharine principle, usually lactic acid. The data upon which this opinion is founded are not given, for the reasons already referred to; but we would again urge upon Dr. Prout the propriety of making them public, if it be only to show that physiological and pathological chemistry has not been neglected in this country to the extent usually supposed, and that Liebig's doctrines are not to be received without the consideration of a much larger number of circumstances than their author seems to have taken into account. For ourselves, we must frankly say that we do not think the distinction which Dr. Prout attempts to establish can be sustained; for this among other reasons,—that, as there is great reason to believe, that the metamorphosis of the albuminous tissues usually takes place (in consequence of muscular exercise) much faster than that of the gelatinous,—we can scarcely imagine the large proportion of urea found in the ordinary urine of the mammalia to be generated from the gelatinous tissues only; nor can we suppose all the uric acid generated in the urine of birds and reptiles to be the produce of their albuminous tissues only. We hope that the researches of chemists, directed to this object, may be successful in throwing light ere long upon this very important question.

Derangements of the destructive assimilation or metamorphosis of tissues are among the most fertile sources of disordered conditions of the excretions; and although Liebig has recently directed attention to these in such a prominent manner, and with such a show of reason, as to have commanded the implicit assent of a large number of followers, yet it would be doing great injustice to Dr. Prout if we did not assert our deliberate conviction that, however much the results obtained by the illustrious professor of Geissen differ from his (and here the question remains open), the path of inquiry which he has pursued is essentially the same with that first opened by our illustrious countryman, and for a long time pursued by him alone.

We shall conclude this article by a notice of some peculiar views of Dr. Prout's, which are frequently adverted to but nowhere distinctly stated, in the volume before us; but which are more clearly laid down in his Bridgewater Treatise. Dr. Prout considers that, after the *death* of the tissues, their *metamorphosis* into excrementitious compounds, resembling those of inorganic matter, does not necessarily take place immediately, but that a portion of them may be again taken into the system and made use of as materials for its nutrition; in other words, that an animal may partly live upon its own tissues. This he supposes to take place by a kind of secondary digestion, occasioned by the liberation of an acid fluid in the capillaries; and he considers that the process of *conversion* may take place *there* as well as in the stomach, so that oleaginous matter (as fat) may be changed into an azotized compound fit for the nutrition of the albuminous tissues. The *mode* in which this kind of change is effected must be admitted to be in a great degree hypothetical; but that such a change does occur we think there is a strong probability. The objection has been advanced that it is absurd and contrary to all sound physiology to suppose that organic matter which has once lost its

vitality can be again taken into the system and enter into the composition of organized tissue; but it seems to have been entirely forgotten by the objector that all the aliment of which we make use is in a dead state or is rendered so during the process of digestion. There is nothing more absurd, therefore, in the idea that a man receives back into his circulation (and therefore partly lives upon) a portion of his own tissues, than that he takes in, recombines, and organizes, similar dead matter from the tissues of another animal. And the hypothesis accords remarkably well with one proposed by Dr. Carpenter, (Human Physiology, § 464-7,) repecting the use of the *lymphatic* system; namely, that it serves to take up, assimilate, and convert into the elements of blood the materials thus set free in the tissues; just as the lacteals take up, assimilate, and convert into the elements of blood the materials prepared by the digestive process. The obvious analogy between the two sets of vessels, their termination in a common trunk, their simultaneous appearance as we pass from the invertebrated to the vertebrated classes, and the very similar characters (both chemical and microscopical) of their usual contents, appear to us strong arguments in favour of this view; and if it be admitted, Dr. Prout's doctrine almost necessarily goes with it.

In taking our leave of Dr. Prout's treatise we have only to repeat our conviction that, in spite of all the faults which we formerly pointed out in it, there is so much of the highest value in its contents that no student or practitioner can be regarded as even tolerably acquainted with the subject who has not read and mastered them.

Art. XVII.

Ueber spontane und congenitale Luxationen, sowie über einen neuen Schenkelhalsbruch-Apparat. Von J. Heine, Doctor der Medezin und Chirurgie, Gründer und Vorsteher der orthopädischen Heilanstalt zu Cannstatt.—*Stuttgart*, 1842. 8vo, pp. 84.

A Treatise on Spontaneous and Congenital Dislocations of the Hip, with a description of a new Apparatus for Fracture of the Neck of the Thigh-bone. By J. Heine, Doctor of Medicine and Surgery, Founder and Superintendent of the Orthopedic Institution at Cannstatt. —*Stuttgard*, 1842.

This essay contains an account of a bed invented by the author for the treatment of the affections of the lower limbs named in the title, and gives the results of the author's experience in relation to the chances of their relief or cure by its means. The style of the work is plain and practical; the contents are evidently the result of considerable experience; and the results arrived at highly creditable to the ingenuity and ability of the author. The applicability of the bed to fractures of the thigh is shown by a case communicated to the author by a friend, whilst its efficacy in affording whatever relief can be given in dislocations of the hip is amply illustrated by its employment in the numerous cases which have come under Dr. Heine's care.

The bed used by Dr. Heine consists of a firm framework, covered with a hard mattress, and furnished with means for the reception of the patient's evacuations. A strong girdle of leather, well padded, is passed round

the pelvis, from the front of which two straps pass over the patient's groin and the perineum to two points on the back part of the girdle opposite their attachment in front. When this girdle is fixed, either to the mattress or by straps to the posts at the head of the bed, the parts above the hip-joints are necessarily fixed, and counter-extension can be conveniently made by force applied to the lower extremity. The lower extremity is laid on a splint, well protected by pads, and furnished with a shoe; to this shoe a screw is fixed, which, by its connexion with a rackwork fixed at the foot of the bed, can be lengthened or shortened, and thus draw the foot further from the pelvis, and thus necessarily lengthen the shortened limb. There are some other ingenious contrivances about the bed, but the general principle on which it is made will be sufficiently understood from this description.

The bed employed for fracture of the neck of the thigh-bone differs but little from that used for the reduction of dislocations; that employed for the former being on the whole the most simple, inasmuch as certain varieties of direction and elevation are occasionally necessary for the treatment of the variable direction which the dislocated limb may assume.

"Case i. John Koch, æt. 17, was exposed to cold, whilst heated with walking a considerable distance, in the autumn of 1838, and soon afterwards began to feel pain in his right hip-joint. The pain and lameness continued more or less till May, 1839, when the right limb was found to be short, for which reason he placed himself under medical care and got somewhat better; but as his ailments again returned, he applied to Dr. Heine, at about eight months from the commencement of his illness. The man appeared to be in moderately good health. There were no signs of inflammation about the joint, and very little pain even on forcible motion of the part. The limb was shortened considerably, and the head of the femur formed a moveable swelling on the dorsum ilii. Under these circumstances the patient was admitted into the hospital, and extension commenced in a few days, on June 25, 1839. The extension was applied at first very gradually and only for a short time, the patient being allowed to walk about during part of the day. On the 22d July, the head of the femur apparently slipped into the socket, but passed out again on relaxing the extension. On July 23d, the bone slipped into the socket, and assumed all its natural conditions of form and relation to the surrounding parts, except that of being three lines short, and having the trochanter major rather prominent. The patient was confined to bed now for three weeks, during which the limb was occasionally rotated on the apparatus. On the 20th of August, the patient was allowed to walk on crutches; the limb was found to be of the natural length; the rotation of the trochanter major more natural. The patient gradually left off his crutches, and left the hospital well, November 19th, wearing a belt round the hips. In December, 1841, Dr. Heine saw the patient, and found him able to walk six or eight hours daily; the two limbs were now the same length, and the stiffness in the joint gradually subsiding.

"Case ii. Philip Fischer, æt. 19, first felt pain in the left hip and knee during the summer of 1839, which continuing more or less till the spring of 1840, was then followed by shortening of the limb. On the 21st March, 1841, the limb presented the following conditions: The left limb was shortened one inch and half, whilst the head of the thigh bone could be felt quite moveable on the dorsum ilii. The patient was free from pain, and all inflammatory affection appeared to have ceased round the joint. Extension was applied gradually from the 6th of April, 1841, till May 8th, when the head of the femur slipped into the socket; the limb, however, remained three lines short and the trochanter somewhat prominent. The patient was treated as the first, and left the hospital, July 22d, 1841, able to walk well but rather clumsily. The leather belt round the hips was also here directed to be used.

Six months afterwards the patient walked with ease a distance requiring four hours to Dr. Heine, who detected no sign of lameness, and found both limbs equally strong.

"Case iii. Catharina Gauger, æt. 16, first felt pain in the right hip-joint in September, 1839, which continuing, was treated as disease of the joint. In January, 1840, the pain became less, but the limb became short. In May, 1841, the limb presented the following appearance: the right limb was small and shortened two inches. The head of the femur could be felt on the dorsum ilii. There was no pain, and all inflammation round the part appeared to have ceased. On May 20th, the extension was commenced. On the 10th of June, the extension had been made rather forcibly during the night, the patient was trying to relieve herself by drawing her body upwards, when something snapped audibly. On examining the limb, the two legs were the same length and the two trochanters on the same level. She was treated as the two previous cases, and left the hospital, walking easily in six months."

[Since this period Dr. Heine has received information that the lameness is hardly perceptible, that the limb continues to gain strength, and that she can walk quite well.]

"Case iv. Christopher Schreiber, æt. 16, first felt pain in the right hip-joint in September, 1841; the pain continued to increase and was attended with apparent elongation of the limb, to which shortening succeeded; increased by an accidental fall out of bed. In March, 1842, the limb presented the following appearances: the limb was shortened two Wirtemburg inches (of ten to the foot), and the head of the bone could be felt on the dorsum ilii, apparently fixed by the muscles and not surrounded by any thickening. The patient was free from pain, and all inflammation appeared to have ceased round the joint. Extension was commenced on the 14th April, 1842, and on the 13th May the head of the bone passed into the socket, the two limbs now being of the same length and the two trochanters on the same level. This patient was treated as the others, and at the usual period was allowed to walk about; the use of the limb gradually becoming more and more complete. This man was seized with symptoms of some obscure affection of the chest, and died on the 23d July, 1842. On examination a large tumour occupied the mediastinum, pressing on the heart and lungs.

"The right hip-joint was very accurately examined. The gluteus maximus and medius muscles were somewhat lax on their lower edges, whilst the gluteus minimus was considerably less firm and tense than in the natural condition of parts. The other muscles and parts round the joint presented nothing unusual, neither could any depressions in the muscles or remains of a false joint be discovered on the dorsum ilii. The capsule of the hip-joint was somewhat lax, but presented on its outer surface no morbid appearance, whilst the head of the femur was accurately fitted into the acetabulum. The head of the bone could with slight force be separated somewhat from the acetabulum, but could not be dislocated in any way. The hip-joint contained about a drachm and a half of a yellow fluid mixed with lymph. The synovial surface of the capsule, on the superior half of its circumference, was of a dark red colour and covered with a membrane of thickened tissue; no remains of any rent or laceration could however be found on any part of it. The ligamentum teres was long, thin, and of a dark colour; the glands of Havers were also dark-coloured and small. The head of the femur was about the natural size, covered with healthy cartilage, but marked with a few grooves. The head of the bone was however somewhat flattened instead of presenting its natural round form." (pp. 9-45.)

In addition to these cases, five others have been treated by Dr. Heine. The ages of these patients were respectively 5, 6, 9, 10, and 12 years; they all had suffered from disease of the joint for not less than two years, but had not suffered from abscesses. On the treatment being first employed, no inflammation of any importance remained; the head

of the bone was drawn up on the dorsum ilii, and the limb was shortened one inch and half. In two cases, the head of the bone was moveable, and two of the patients could walk without crutches. So far as could be ascertained, the original disease appeared to depend on the previous strumous condition of the patient. In these cases extension was carefully but forcibly applied for some months, with some benefit to the shortening of the limb in two of the cases. In the other three, however, such pain was produced by extension, as to suggest the idea that adhesions had formed between the head of the femur and the surrounding parts.

From the result of these cases, Dr. Heine considers that the following rules may be laid down, as regulating the propriety of the employment of extension:

1. The removal of all inflammatory disturbance about the part is necessary before its employment.

2. The less advanced the disease is, the more favorable is the case for the employment of extension.

3. The existence of abscess precludes any attempts to return the bone to the joint.

4. The prominence and mobility of the great trochanter are favorable signs, as they indicate that the head of the bone is not adherent.

5. The prognosis is more favorable in affections of the hip from mechanical and rheumatic causes than from affections of the general health and scrofulous disease.

6. When extension is applied at an early period after the occurrence of the dislocation, it affords more chance of success than when it is applied at a later period.

The congenital dislocation of the hip has been observed by Dr. Heine in nine female and two male children, six of whom were affected in one hip and five in both. No decided benefit resulted in any of these cases from the employment of extension, as the bones returned to their previous position on the extension being omitted.

The account of a dissection of a congenital dislocation of the hip is recorded, and appears to be of sufficient interest to insert here.

"Frederic Schwarz, æt. 40, died of acute disease, having suffered all his life from lameness and deformity of the left hip. The trochanter major was situated one inch and a half higher than that of the opposite limb, the thigh and leg were both short, the patella situated more on the inner side of the joint than natural, and the foot turned slightly inwards. On dissection the patella was found to be united by fibrous substance to the inner condyle, whilst the muscles passing to the upper part of the tibia formed long fibrous threads. The muscles round the hip were extremely weak and small; the trochanter major was of its usual form, and the capsule firm and thick, with a smooth membrane lining it. The head of the femur was covered well with cartilage, somewhat flattened, and only half its natural size. The ligamentum teres was wanting, and its place of insertion into the femur was covered with cartilage. A smooth surface, situated above the natural position of the acetabulum, formed the articulating surface for the head of the femur, which was fixed to it by the firm capsule. Below was situated the natural acetabulum, of a narrow oval form, two lines in depth, and forming a cavity for the flattened trochanter minor." (p. 69.)

Art. XVIII.

Pulmonary Consumption, successfully treated with Naphtha. By John Hastings, m.d., Senior Physician to the Blenheim Street Free Dispensary.—*London*, 1843. 8vo, pp. 120.

We have risen from the perusal of this book with varied feelings of a most unpleasant kind; and if that fertile source of inspiration acknowledged by the poet in the well-known verse,

"Si natura negat facit indignatio versum,"

were permitted to the scientific critic, assuredly the present article would be both copious and severe. But reason, not passion, must guide the pen of the reviewer, whose office ought ever to be that of a judge and never that of a satirist; we will, therefore, repress all emotional feelings, and sit down calmly to dispense justice.

We say justice; according to our solemn conviction of what is right and true, after complete investigation of the premises, and without affection or favour, on the one hand, and without prejudice or malice on the other. And we are proud to say that this is a merit which this Journal may boldly claim. In witness of its right so to do, we refer fearlessly to every Number of our now-long series, and to every article of every Number; and we will add, that it is a merit that it shall not forfeit so long as the hand that has hitherto directed its course shall continue to rule and guide it. In making this statement, we do not pretend to maintain that all our opinions have been correct and all our judgments just; to do so would be to assert a claim incompatible with the imperfection of human things; but we do maintain that where we have erred we have never done so wilfully, nor yet through carelessness or a laxity of principle almost as criminal as conscious corruption. Would that the past and present state of medical criticism in this country had made the character now claimed for this Journal in no respect singular or peculiar! But, alas, when we see published, week after week, quarter after quarter, accounts of medical books so utterly at variance with their real character, we feel ourselves warranted in the adoption of a tone which, in other circumstances, would be presumptuous. In charging some of our contemporaries with this delinquency, we entirely acquit the editors of voluntary and conscious injustice; we merely accuse them of a culpable carelessness, and of indulging in the silly and vain hope of reaping where they have not sowed. So long as it can be said of any journal that it will admit into its pages an author's criticism of his own book, or a friend's criticism of his friend's book, or a publisher's puff of *his* book, or, to speak in general terms, so long as gratuitous contributions from suspicious or incompetent hands are permitted to figure as literary adjudications, so long shall we continue to denounce much of the medical criticism of our time; so long shall we look upon our own labours with conscious satisfaction, nor feel the

—Sume superbiam
Quæsitam meritis———

of the poet, to be a mandate suggestive of reproach instead of honour.

These reflections have arisen in our mind while meditating on the character of the judgment which justice compels us to pronounce on the book now before us, and anticipating, in imagination, the very opposite verdict that will probably be returned in other courts of criticism, where perchance laxer and more good-natured judges may preside over more facile juries. Be this as it may, we, at least, shall not shrink from the discharge of our duty, however painful, nor wittingly incur the penalty implied in the once obscure but now immortal apophthegm,

"Judex damnatur cum nocens absolvitur."

It is doubly painful to us, both from national and personal considerations, to be called on to notice a book like that of Dr. John Hastings, (we earnestly entreat the reader not to confound the author of this book with the honorable and honoured founder of the Provincial Medical Association, Dr. Charles Hastings of Worcester,) just after rising from the task of reviewing the immortal work of M. Louis; and the more so when we reflect that we have had to do like justice on so many kindred productions of our countrymen, on the same subject, during the last few years. It was only in our Number for April last, that we found ourselves called on, while noticing *two* new books on consumption, to make use of the following somewhat severe expressions:

"But this matter grows too serious to be treated triflingly. For, though we are unwilling to acquiesce in the common notion that the learned writers produce these books with the simple view of advertising themselves, and for our own parts feel contented to regard them as the offspring solely of a somewhat impertinent vanity, yet productions of the sort are not as harmless as they might appear. They in truth lend their little aid in supporting and giving vigour to the monstrous system of fraud practised by the 'consumption-curing' doctors on the too gullible multitude. Viewed in this aspect they are mischievous in the extreme; though redolent of the very refuse of Paternoster Row, their influence upon the sufferers from consumption will not be the less active. Though utterly destitute of logic, as of evidence of even moderate acquaintance on the part of the writers with the principles of precise medicine, they will not fail, because they hold out hopes of cure, to lure many a victim to the toils set for all whose pockets are sufficiently well lined to make them worth the catching." (Vol. XV. p. 540.)

And we are bound to add, in justice to the gentlemen here inculpated, that the volume of Dr. John Hastings (" in the lowest deep a lower deep") is, in many respects, more deserving of reprobation than theirs. In the first place, it is written in a much worse style; secondly, it betrays still greater ignorance of precise medicine and all the rules of logic; thirdly, it is much more calculated to impose upon the public and the more ignorant and inexperienced members of the profession, by the boldness of its tone, the long array of alleged cures which it displays, and, above all, by the specious manner in which it has, for the first time, enlisted auscultation as a false witness in the cause of empiricism. It is this last enormity which chiefly roused our indignation in perusing the pages of Dr. John Hastings. We felt, while reading his cases, as if the staff on which we were accustomed to lean in all our difficulties, had not only snapt asunder beneath our weight, but had wounded us as we fell; our most cherished medicines seemed converted into poisons; we started as if the benevolent genius which had for so many years been leading us on to

truth, had suddenly been transformed into a malignant demon to lure us to destruction; it was like seething a kid in its mother's milk.*

The work of Dr. John Hastings, although professedly written to magnify the virtues of naphtha in consumption, contains, of course, all the usual display of materials which the public could desiderate in a treatise on so popular a malady; accordingly, we have separate chapters on the causes, symptoms, complication, pathology, diagnosis, and treatment of consumption. Of these we shall only say, in the mass, that with the exception of a single observation of a few words on the subject of "a globule" in tubercle "hitherto unnoticed" (p. 35)—and which observation is obviously, like the rest of the book, a mistake—they do not contain one particle of information which is not to be found in the works in everybody's hands, while they are disfigured by innumerable omissions, oversights, and mistakes, and exhibit a style of composition discreditable to any man who has received even an ordinary English education, and disgraceful to one who boasts the highest honours of our learned profession. Some few of these philological delinquencies we give in a note below;†

* In reading that part of Dr. John Hastings's book which relates to diagnosis, we are forcibly reminded of a caution addressed to young auscultators by one of the earlier labourers in this field, in a very imperfect book, but one, it is believed, of much use at the time of its publication; and as the caution, even at the present day, when physical diagnosis has made such wonderful progress, seems still needed, we shall offer no apology for here transcribing a part of it:

"It is highly proper and necessary that it should be generally understood, that the practice of auscultation, although easily acquired by care and attention, is not to be acquired (in such a degree of perfection as to be useful) without these, and without considerable experience. And I would advise every one to be extremely cautious in drawing from his explorations any conclusions which are to influence the treatment of diseases, until he has been able to convince himself thoroughly of the correctness of these by a good deal of experience, and, if possible, by the unerring testimony of dissection.

"The reason of this caution will be obvious on considering the relative characters of the physical, and the common or sympathetic diagnostics. In general we depend so little on any *one* symptom of a disease, that we seldom risk any momentous treatment, or hazard a decided prognosis on it; and, therefore, it is not a matter of very great consequence, practically, whether we are right or wrong respecting its supposed import in any individual case. It is very different, however, with many of the indications furnished by mediate auscultation or percussion. To such persons as admit their authority, these carry with them the conviction of almost physical demonstration; and it is impossible not to yield to opinions founded on such a basis, assent of a very different kind from that which follows the contemplation of a mere sympathetic symptom. We may deem lightly of a quick pulse, or a hurried respiration, or an acute pain, because we know that all these may accompany an affection of the most temporary kind, and of no danger; but when we know that the fleshy sound, or absence of respiration, over one side of the chest, can only arise from a great organic change, it is impossible that we can regard such a sign but as one of the highest consequence, and as worthy to determine our prognosis and direct our practice." (Original Cases, &c. by John Forbes, M.D.—London, 1824. Preface, pp. xxiii-iv.)

† It would be too serious a task to expose all the peculiarities of style contained in the pages of Dr. John Hastings; to do so we would need to reprint his book, and our philological zeal does not go so far as this; but the following short list of specimens, taken at random from the volume, will sufficiently enlighten our readers as to the precision and clearness of the style, and the grammatical and classical knowledge of the "Senior Physician to the Blenheim Street Free Dispensary:"

"...... at no period of life *are* either sex exempt." (p. 3.)

"*Preventitive* remedy." (*Ib.*)

"Frightful as are the results, they fall far short in amount of misery *with* those of consumption." (*Ib.*)

"The most common of the various *incitements to the propagation of tubercle* is the hereditary cause." (p. 10.)

"Some individuals........blind themselves to their own condition, *by refusing their*

but we have not time to dwell on them: our business, at present, is with the matter not the form of the book—with the facts or alleged facts, which it presents to us in proof of the power of naphtha to cure consumption.

We have no right, *à priori*, to conclude that naphtha or any other unknown substance may not possess the happy distinction of being remedial of phthisis; although from the failure of tar, creosote, and other analogous substances, we might feel disposed, at first, to doubt the probability of this. And we give Dr. John Hastings all due credit for having been the means of introducing this medicament to our notice in relation to consumption. From some of the statements made by him in detailing the cases, we are disposed to hope that it may be found useful in catarrhal and other chronic affections of the air-passages; and we, further, recommend its being tried in these and also in cases of true phthisis, both in the early and later stages, in the manner prescribed by him. It is *possible*, at least, that naphtha may be a cure for consumption, though Dr. John Hastings may have failed to show that it is so.

The original part of this volume consists in the details of thirty-seven cases of pectoral affections, all regarded by the author as examples of genuine phthisis, all treated by naphtha, and *all*, or nearly all, CURED by it, in a space of time varying (to speak in round numbers) from one month to four months!

"From the very *first moment*" [how marvellously rapid is the operation of this agent and how quick the perceptions of the author!] "From the very first moment I employed naphtha in pulmonary consumption up to the present time, it has been so successful in my hands, that I have no doubt it will be found, upon careful

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consent to the well-marked symptoms of the disease; while others in complaining are subjects of ridicule from their blooming cheeks............and thus *the disease progresses* to an advanced stage *before seeking* medical aid." (p. 16.)

"Flatulence and pain *exists* at the pit of the stomach after meals, which in females *is* accompanied," &c. (p. 17.)

"...... after the carbonic acid and water *has* been given off ..." (p. 27.)

"The tubercles .... begin to soften in their *centre, which gradually extends* to the circumference." (p. 28.)

"The cough and difficulty of breathing *is*," &c. (p. 34.)

"Cheerfulness of mind and *calmness of the nervous organisation* are indispensable." (p. 44.)

"...... the cough, expectoration, and difficulty of breathing *was* very much improved." (p. 67.)

"The breakfast *hour* should be nine o'clock, and *consist* of milk," &c. (p. 43.)

"...... an evident *change* for the worse ensued, which was quickly *removed*," &c. (p. 100.)

"The *catamenia was* correct as to time." (p. 62.)

"The *catamenia was* regular." (p. 69.)

"The *catamenia was* irregular." (p. 74.)

"The *catamenia was* regular." (p. 79.)

"The *catamenia was* healthy." (p. 81.)

"The *catamenia was* irregular." (p. 82.)

"The *catamenia was* natural." (p. 86.)

"The *catamenia was* healthy." (p. 91.)

We have made these numerous citations to show that this *singular* reading of *catamenia* is not a mere typographical error. We fear some of the following come within the same category; but we hope the printer is the sole delinquent:

"And *sub-crepetant* râles, which the French call the *tempest sound*." (p. 34.)

Stethoscope is everywhere spelt *stethescope*, except in two or three instances.

Prostrate gland. (p. 24.)

Vis medicatrix *natura*. (p 29.)

Sub-crepetant. Passim.

and judicious use, to be *little less than a specific* in the earlier stages of the disease." (p. 8.)

"Single-handed, if I may be allowed the use of the expression, *it has cured pulmonary consumption in almost every case in which it has hitherto been used,* when the disease has been treated *in an early stage.* And from what I have more recently observed, although I do not consider myself justified at present to publish *it,* [what?] I am most sanguine that *even in the latter stages* of the disease *a restoration of health may generally be calculated upon.*" (p. 120.)

Such general statements as these, however remarkable, coming from a quarter hitherto unknown and consequently of no authority, would make but little impression on the minds of those who know phthisis by long and melancholy experience, or who have overlived the kindred marvels once worked in utopian medicine by digitalis, hydrocyanic acid, &c. &c. Unsupported by facts, or alleged facts, they would be utterly disregarded by men of experience, as merely yet another brood of those vain visions which, in all times, have beset the brains of incipient doctors. But when we have these statements ostensibly *borne out* by the details of the cases of thirty-seven men and women, with their names and addresses blazoned in a book,—men and women, too, all alive and well, with lungs now as sound and sonorous as a bell, although, but a few weeks or months ago, with chests as dull as a board from tubercles,—the thing becomes of much more serious import and demands close investigation.

What, then, are the "facts" which Dr. John Hastings has recorded in his book—and with which, if all the men and women of England are not soon made acquainted, it is assuredly their own fault for shutting their eyes to the gentle intimations given of its existence and the nature of its cheering contents, in every journal of the day? The "facts" are these:

Thirty-two women and four men* came to Dr. John Hastings, labouring under pectoral affections of various duration, from a few weeks to some years, and presenting, with scarcely an exception and with remarkable uniformity, the following very serious combination of *symptoms:* cough, dyspnea, copious expectoration, profuse night sweats, emaciation. Four had had hemoptysis. The nature of the expectoration is scarcely ever mentioned, and the state of the pulse only three or four times. A large proportion of the patients are stated to be of consumptive families. *Every one* of these patients, in addition to these symptoms, afforded to Dr. John Hastings auscultatory *signs* of the most formidable import: viz. *dulness over a part or the whole upper regions of the chest*, absence of the respiratory murmur in the dull spaces, or change of its natural character to that of feeble, harsh, &c. &c. In no less a number, we think, than twenty-seven out of the thirty-seven cases, the dulness is represented as existing not only *on both sides but over the upper portion of the chest generally;* in ten cases, or thereabouts, the dulness is confined to one side. In about one half of the cases the sounds of the heart are stated to be abnormally audible over the dull parts; and the respiration is recorded as *puerile* on the sound side in almost all the cases where the dulness existed only on one side. Supra-clavicular or sub-clavicular depressions are stated to exist in several cases.

It is evident that if we admit the validity of the evidence of disease here afforded to us, and implied by these symptoms and physical signs, we must concur in opinion with Dr. John Hastings, that he had to deal

* We exclude, for the present, Case Twenty-three, that of a patient in the last stage.

with serious structural disease of the lungs; in short, with pulmonary consumption in an advanced stage of its progress. And yet what is the result of his treatment of these very cases?

Under the use of naphtha (in doses varying from ten to twenty, thirty, or forty drops, thrice a day) and of naphtha alone (for in general no other means seem to have been used), ALL *are cured;* the cough, expectoration, sweats, &c., disappearing for the most part within a few weeks, and the physical signs indicating structural change, vanishing shortly after!!

That we may avoid all possibility of exaggeration in an affair of so much importance, we will here put down the precise periods within which the complete cure was effected, as evinced by the disappearance of all the physical signs in addition to the common pectoral symptoms which had, in general, taken their departure long before: and, although we might be justified by the statements and obvious inferences of the author, in classing *all* the cases as examples of *perfect cure*; still, as in a certain proportion of them (eight out of the thirty-six) it is not positively said that there were no symptoms or signs left at the period of closing the report, we shall divide the cases into two classes; viz. (1) cases in which all symptoms and physical signs had disappeared, and the cure was therefore perfect; and (2) cases in which, although the patient was regarded as cured, some trifling relics of functional or structural change still remained.

Of the first class, then, or *positively perfect cures*, we have 4 occurring within the thirtieth day; 4 between the thirtieth and fortieth; 7 between the fortieth and fiftieth; 2 between the fiftieth and sixtieth; 5 between the sixtieth and eightieth; 5 between the eightieth and the one hundred and tenth: and 1—such, alas, is the occasional tardiness of cure—not sooner than the one hundred and seventy-first. Of the second class, or *virtually perfect* cures, 5 occur between the tenth and thirtieth day; 3 between the thirtieth and seventy-seventh day: total 8. Grand total, 36 cures.

The case not included in either class is case twenty-three, the only one in which it is positively stated that the disease was in the last stage, as evinced by "dulness over all the superior regions of the chest," and "a cavernous *râle*, accompanied with perfect pectoriloquy, below the centre of the left clavicle." This case, however, although *not stated in words* to be cured, tends by no means to damage the glory of Dr. John Hastings and his matchless specific, as the following short extract will show:

"After a lapse of *sixteen days*, the cough, expectoration, and difficulty of breathing had diminished, the night-perspirations and other signs of hectic fever *had disappeared for rather more than a week*, the appetite was natural [it had previously been bad], and a dry blowing respiration was well marked over the space occupied by the cavern. The dose of naphtha was now increased to twenty drops. On the twentieth day he had a return of all his former symptoms, owing to his getting wet and allowing his clothes to dry on him. *The cavern, nevertheless, below the clavicle could hardly be detected.* . . . By the thirty-fourth day, a further amendment had evidently ensued, *for percussion yielded a much clearer sound*, and the respiratory murmur was improved. The cavernous *râle* was less distinct and *pectoriloquy* could not be detected. *Percussion elicited a good sound throughout the chest.*"!! (p. 89.)

In justice to Dr. John Hastings and ourselves, we will extract two or three of his perfectly-cured cases, selecting those which will occupy least space. We shall give them *verbatim* and complete.

"CASE XI. Thomas Howell, a married man, aged twenty-two years, residing at 16, Foley street, Foley place, of a consumptive family, his father, brother, and five other members of his family having died during the last twelve months from pulmonary consumption, and a sister being now confined to her bed from the same cause, consulted me for the first time, on the twenty-fourth of March, 1843. He was following the trade of a shoemaker. About two months ago he caught cold while riding at night outside a coach from Cheltenham to London. This was followed by cough, which ceased in the course of a week; leaving, however, a little shortness of breath. The cough returned a fortnight ago, without any apparent cause, which had daily grown worse, and a rapid loss of flesh and considerable debility was the direct consequence. His general health, which had always been excellent, remained unimpaired, and the only other complaint he had to make, was a little dimness of vision which he had suffered from more or less since childhood. The chest was well formed, although its motions were somewhat restricted, and the clavicles were unusually bent outwards. A dull sound was the result of percussion over all the superior regions of the chest, and the respiratory murmur was generally feeble and harsh over the same spaces. The sounds of the heart were very distinct below the right clavicle. After taking naphtha for seven days, the cough had ceased to trouble him, and the sounds of the heart were scarcely audible below the right clavicle. At the end of fourteen days he had no complaint to make, and was growing stouter and stronger; percussion yielded a clear sound over the whole of the upper part of the chest, the respiratory murmur was much improved, and the sounds of the heart were inaudible below the clavicle. On the twenty-seventh of April, being the thirtieth day of the naphtha treatment, he enjoyed excellent health. Percussion continued to yield a natural sound, and the respiratory murmur was slightly harsh below the left clavicle." (pp. 71-3.)

"CASE XVI. Eliza Shepherd, a married woman, aged forty-one, residing at 39, Carnaby street, Regent street, consulted me on the eleventh of May, 1843. Her father and several branches of his family had died through consumption. For some time, she had been troubled with cough and expectoration attended with difficulty of breathing and nocturnal perspirations, and during the last six weeks had greatly fallen away. Her pulse was eighty and weak, her bowels constipated; there was great loss of appetite, and a sensation of pain at the pit of the stomach after meals. The *catamenia* was regular. The walls of the chest were very much attenuated, there existed considerable depressions in the neighbourhood of both clavicles, and on the left side the movement of expansion was hardly perceptible. There was a dulness of sound over all the upper regions of the chest, and the respiratory murmur was generally feeble and harsh, the sounds of the heart being very distinct on the right side. In order to correct the bowels two aperient pills occasionally at bed time were prescribed, and the naphtha treatment in fifteen drop doses commenced. The night perspirations soon ceased. At the end of seven days the cough and expectoration had diminished, and the appetite had improved, but she complained of pain between her shoulders. The drops were now increased to twenty, and a mustard poultice applied between the shoulders. At the close of the fourteenth day she had greatly benefitted in every respect and possessed a good appetite. On the twenty-eighth day the cough, expectoration, and difficulty of breathing had entirely left her, and the sounds both from percussion and auscultation were of a healthy character." (pp. 79-80.)

"CASE XIX. Margaret Lee, a married woman, aged twenty-seven years, of 47, Stanhope street, Clare Market, informed me, on her admission as a patient on the fourth of May, 1843, that as far as her knowledge of her family history extended, the only member of her family who had died from consumption was her brother. For the whole of the past winter she had cough, expectoration, difficulty of breathing, and nocturnal perspirations, and for the last month had rapidly wasted and lost strength. The *catamenia* and the bowels were natural, the appetite was deficient, and some pain at the pit of the stomach was experienced after meals, attended with flatulence and palpitation. A dull sound was generally the result of percussion over the superior regions of the chest, and the respiratory murmur was

then feeble and harsh, while below the right clavicle a dry crackling *râle* was distinct. The heart sounds were loud over the right side. The fourteenth day having arrived, after taking fifteen drops of naphtha in the usual quantity of water, the cough and perspirations had subsided, the difficulty of breathing had diminished, the appetite had improved, and the signs from percussion and auscultation were now satisfactory. The dose of naphtha was increased to twenty drops. In the course of the thirtieth day after the employment of the naphtha, the symptoms enumerated were removed, and the chest sounds and respiratory murmur were of a healthy nature." (pp. 83-4.)

Now is not this—and all this—most wonderful? Are not the results here blazoned almost unexampled in the annals of medicine, even when we include therein those copious but disgraceful and degrading chronicles of which our newspapers are the vehicles and of which the vendors of secret pills and balsams and other *universal* remedies, are the authors? We think they are. Other wonder-workers in the therapeutics of desperate maladies have for the most part toiled in the dark. In regard to the dreadful diseases they boasted to cure, honest men had the consolation to think, amid their own failures, that the diseases either did not exist at all, or that the so-called cures were merely temporary alleviations, to which almost all diseases are naturally subject. No evidence but the assertions of the parties was adduced; and, of course, these went for nothing among men of sense and honour. But the deeds of Dr. John Hastings are of quite another stamp. Wisely judging, no doubt, that the mere assertion of a doctor of medicine, even although that doctor should be the senior physician of a free dispensary, would, on such a subject, go for little, even among the well-informed men out of the profession,—now, when the knowledge of auscultation has become so general,—he has called to his aid *the positive evidence of physical signs*, leaving no loophole whereby the most sceptical (admitting the truth of the statements) can escape from the conviction that he has actually cured, in succession,* at least six and thirty cases of confirmed phthisis pulmonalis (and God knows how many more,) with consolidation of the whole or greater portion of the upper lobes, in one, two, or three months,—and all by some ten or twenty drops of naphtha taken thrice daily in a little water!! This is a feat assuredly "beyond all Greek, beyond all Roman fame;" beyond all that has been claimed for their panaceas by the quacks of past or present times; beyond all that has been observed by faithful and experienced physicians since the days of Hippocrates; beyond all that the most enthusiastic believers in the remedial powers of art have ever ventured to expect; beyond all that the fondest lovers of their kind have dared to hope or almost to pray for!

And can we, then, sitting here as capable, honest, and unprejudiced

* Of the thirty-seven cases, all, except four, occurred during the first six months of the present year—viz. four in January, four in February, six in March, eight in April, seven in May, and four in June. We should like to know if any other physician to a *general* dispensary, or even to a dispensary devoted exclusively to pectoral complaints, ever before met with so many cases of *chests dull all over the upper regions*, in the earlier stages of phthisis, in so short a period? We wish Dr. Roe or Dr. Walshe would inform us how many cases of this kind they have seen during the present year at the Hospital for Consumption. We wish still more earnestly that they and their colleague Dr. Williams would invite Dr. John Hastings to exhibit the Herculean heroism of his naphtha before them at the hospital. We dare say they could find, at least, a few specimens of disease for him, answering—or almost answering—to his stereotyped nosological formula:—"All the superior regions of the chest yielded a dull sound on percussion; the respiratory murmur was scarcely audible or was *feeble and harsh*, &c."

judges, lay this precious and flattering unction to our souls, and believe that all this has verily been done; and that we may now, in consequence, confidently expect, with Dr. John Hastings, to cure, by means of this glorious naphtha, every case or almost every case of pulmonary consumption, and thus, for the future, save from a premature death one fifth of the human race? Would that it were so; but alas, alas, it is not so! All our knowledge of health and disease, all our experience of medical treatment, all our reasoning on the facts of science, all our reflection on the fallaciousness of human testimony, on the imperfection of medical observation, on the sins of partiality and prejudice that so easily beset us, and on the seductions of vanity and self-interest, would lead us, *à priori*, to doubt or disbelieve such a proposition, if stated to us in general terms; and the careful perusal of the work before us has left on our minds the painful certainty that, in the individual case of Dr. John Hastings, we must utterly deny its truth!

The unexpected length to which this article has already run must prevent us from giving, in detail, all our reasons for coming to this distressing conclusion; and we must request such of our readers as have confidence in our judgment and good faith, to take its accuracy upon trust, until such time as they themselves have read the book *or tried the remedy*. We may, however, here put down, as briefly as possible, a few of the grounds on which our conclusion is based:

1. The extreme improbability that such a statement should be true, for reasons already stated as well as others.

2. The much greater likelihood that *any man* should be mistaken, than that events of such a marvellous character should have really taken place.

3. The infinitely greater likelihood that a man of the intellectual caliber and degree of education of Dr. John Hastings (as shown by his book) should be mistaken, than that the alleged facts should be really facts.

4. The general character of the cases as detailed in the book render it extremely probable that such a mistake has actually taken place. Indeed, we are certain that no experienced auscultator will read these cases consecutively, and not come to the conclusion that, in their construction, the modesty of nature has been overstepped—that they are, in fact, not true transcripts of actual disease. They are so very perfect specimens; the physical signs are so very strong, so very precise, so charmingly and facilely responding to the presumed organic lesion, and so exactly alike in every case; they are so uniformly terrible in their aspect and yet so uniformly amenable to the remedy, so ostentatiously consistent and yet every here and there betraying so many little incongruities, that the old observer of old nature feels, when reading them, that he is not on his ancient ground, or rather that his ancient ground has been metamorphosed into something new,

"And starts—for life is wanting there."

Do we then (it will be asked) presume to state or to insinuate that the cases here narrated were not actually seen by Dr. John Hastings, or that he has, in any respect, promulgated what he does not believe to be true? By no means: on the contrary, we have not the slightest doubt that every case here related is a *bona fide* case, and that every patient is to be found or heard of at the localities so carefully indicated: we believe that he imagined, and still imagines, that he heard and saw everything he de-

scribes, while examining his patients; and that, in drawing up the details of their cases, he intended to delineate, and thought he delineated, only what his senses communicated to him. We think it right, however, that he should be made aware that many of his medical brethren do not put so liberal a construction on his conduct and motives in composing and publishing his book. The quality of its materials, its tone and manner, its "taking" title, and the way in which it has been blazoned in the newspapers, to an extent far beyond the ordinary range of honest medical advertisement for the mere sale of books—all too forcibly remind them of the publication of works, the subsequent conduct of whose authors left no room for indulging the charitable belief that their productions were merely the offspring of ignorance and folly. The speedy lapse of these men into the practice of open and unblushing quackery, made it evident that they had calculated beforehand the effect that would be made on the public mind by their bold announcement—that a most destructive malady, usually deemed irremediable, was really "curable," and not only so, but that it had been "successfully treated" by themselves. Wherefore it is that honorable men doubt of Dr. John Hastings, and that we deem it our duty to point out, at least, the danger that he runs.

As we have said, we acquit Dr. John Hastings of all intention to deceive others; we believe he was himself deceived. He thinks he has been stating the truth; we believe that he has been egregiously mistaken. And all this is readily explicable, and indeed is easily explained. Grant that Dr. John Hastings possesses a sanguine temperament and a lively fancy; that he has never been taught the difficult art of observation; that his mind is by nature ill qualified for, or has been ill disciplined in the processes of logical reasoning, and ill instructed in the laws of evidence; that he is an indifferent pathologist and an inexperienced auscultator,—and we need have no difficulty in accounting for and explaining all the marvels of his book, and reconciling them all with our conclusions.

Such is our conscientious judgment of the book of Dr. John Hastings, and such our theory of the marvels it unfolds. We believe the one to be just and accurate, and we think the other sufficient. Both, however, *may* be wrong. It is certainly *possible* that, in the cases narrated, the physical signs were accurately perceived and correctly delineated, and that they were, as in other instances, faithful interpreters of the presence of organic changes in the lungs; it is equally *possible* that, in the same cases, the naphtha taken by the patients may have, in the wonderfully brief space of a few weeks, thawed and resolved into a dew those vast and solid masses of tubercular deposit, as the sun in April melts the snow-wreaths of winter. It becomes us, therefore, to add—and in doing so we shall conclude this article, already too long—that it is our intention to give a fair and full trial of naphtha, in our own practice, and to urge our friends, as we now urge our readers at large, to do the same; and we hereby promise that if we shall ourselves observe even an approach to the success recorded in this book as flowing from it; or if we shall receive from competent and honest judges the assurance that they have been so fortunate, we shall not merely make public the results, but shall, as openly as we have accused Dr. John Hastings, avow our own mistakes and false judgment, and claim his forgiveness.

Art. XIX.

Brighton, and its three Climates; Remarks on its Medical Topography, and Advice and Warnings to Invalids and Visitors. By A. L. Wigan, m.d. Surgeon, formerly practising in that Town. — *London*, 1842. 8vo, pp. 72.

This is a pretty little pamphlet, neatly and smartly written, and will be found very useful to the thousands who visit Brighton for the purpose of deriving benefit from its pure and bracing climate. It briefly points out a good many little things which invalids ought to seek and shun. It, however, hardly fulfils its promise, inasmuch as it gives us extremely little information as to the *climates* so ostentatiously set forth in the title-page. The following extract contains nearly every word said on this subject, and though the statements are accurate, as far as they go, they scarcely announce such differences as suffice to constitute variety of climate. We believe the three divisions of the town really to possess, respectively, qualities capable of influencing the health of delicate persons; and in recommending our patients to Brighton, in common with other physicians, we have been in the habit of indicating their residence accordingly. But we have to complain that Dr. Wigan rather indicates the localities that describes the climates.

"The town of Brighton may be conveniently divided into three parts: the centre portion, extending from Cannon Place on the west, to the New Steyne, or even to Rock Gardens, on the east; and the two extremities, to Adelaide Terrace on the west, and to Arundel Terrace, Kemp Town, on the east, each inclusive.

"Although this division is arbitrary, and the characteristics of each portion fade off insensibly into each other, it is sufficiently distinct for my purpose, and I shall use the terms *Eastern*, *Western*, and *Centre* portion when speaking of the influence of locality on disease, and recommend or forbid them accordingly.

"1. The Centre portion, being occupied by almost all the permanent inhabitants of the town, is necessarily the most frequented; and as all these persons have connexions in London, is, of course, most frequently recommended to strangers. It differs, however, very little from any inland town placed in a low situation, and it possesses none of the quality commonly called 'bracing.' Much annoyance is occasioned by the numerous steam-engines belonging to the baths and breweries, and the chimneys of private houses in the Western and Eastern portions of the town, according to the direction of the wind, which blows from those quarters respectively about eight months and two months throughout the year—north and south for the remainder.

"When the wind is due north the atmosphere at all seasons is beautifully transparent, and the whole town as brilliant as Genoa. In ordinary weather, although each extremity of the town is extremely clear, the middle portion has a mist (at least) which prevents the free ascent of the smoke, and the inhabitants of one end of the town can scarcely ever see the other extremity—each generally supposing the other end to be enveloped in smoke, when, in fact, it is as clear as his own portion.

"The Centre of the town has been so effectually drained, and there is so much care employed to remove nuisances, that it is not objectionable to those in health, nor, indeed, to more than one class of invalids, and it presents the advantage of baths, libraries, shops, riding-schools, fencing-rooms, billiards, and other sources of health or recreation.

"To individuals without families, and who are unable to have a permanent conveyance, and to those who resort to the baths for maladies requiring surgical aid only, or who merely come to Brighton to escape the turmoil of business, or the

thick atmosphere of London, this part of the town is well adapted; but such as have their families with them, and can afford the larger and better-built houses of the East and West, should not subject themselves to the disadvantages and annoyances inseparable from a low situation in the centre of a large town.

"II. The Eastern portion of Brighton is elevated considerably above the sea, and, except when the wind is violent, escapes in a great degree the admixture of saline particles with the atmosphere; not chemically, but mechanically suspended therein. It has a chalky soil, through which the rain immediately percolates, and which permits no moisture to remain on the surface. The houses, too, are of modern erection, and possess many of the comforts and conveniences which have only lately been brought into general use; and the air is decidedly 'bracing.'

"III. The Western portion, again, (at least that part of it which abuts on the sea,) has a clayey soil, and a mild and soft climate. The clay has, however, been so largely excavated for building and for bricks, and the immense lawn in front of the sea is so entirely composed of artificial soil, covering a bed of rounded pebbles, and the country further to the west in the direction of the prevalent winds is so entirely gravel, that it escapes in a great degree the annoyances which the original composition of the soil would have seemed to indicate. I consider that portion of the Western division of the town which is north (and, perhaps, inclusive) of the Western Road; the upper part of Montpellier Road; extending to the New Vicarage, and the house called the Temple, and round to the Poor House, to be by far the most salubrious portion of the whole town There are indeed very few, if any, maladies for which the air of Brighton is advisable that will not be more favorably placed here than elsewhere." (pp 25-9.)

As Dr. Wigan has resided many years at Brighton, and is evidently a shrewd and a good observer; and as no suspicion can attach to his testimony on the score of an interested partiality, (he having left the place,) we regard the few following remarks, gleaned from various parts of his pamphlet, as well meriting the attention of all who send or are sent to Brighton.

"It is of the very essence of atmospheric influence, that if it benefit one class of diseases, it must necessarily aggravate those of an opposite character, and the air of Brighton, as far as his experience extends, is never neutral." (p. 14.)

"There is no proposition of which I am more firmly convinced, than that, in all cases whatever, the sea-air is more salubrious at the distance of five or six hundred yards from the edge of the water than close to it." (p. 30.)

"From the middle of March to the middle of May, unless in compliance with deliberate medical advice by one who thoroughly understands the subject, and to whom you have given ample opportunities of knowing your disease, stay away altogether. I know no malady whatever, not even glandular disease, which is benefited by sea-air thus early in the season." (pp. 32-3.)

"From the middle of July to the middle of October, invalids may locate themselves as close to the water as they please, or on it. At other seasons let them keep at a short distance, and if they have the power to choose, select a house with an eastern aspect. This, by the by, from the structure of the town, will generally be south-eastern; take it altogether, the best 'exposition' of all." (p. 32.)

"The month of June, when (as the phrase is) there is 'not a soul in Brighton,' which, as in London, means souls and bodies of a certain class; this month and the next are not merely the most delightful period of the year, but that in which the air of the sea at Brighton has the most energetic influence in the cure of disease. The sea-fogs are over; the air is become of an uniform temperature, not too warm to admit of abundant exercise; the pathways and roads through the corn-fields afford delightful walks and rides; the Downs are in perfection; open carriages may be had on exceedingly moderate terms." (pp. 34-5.)

"The Downs, which surround the town, furnish the purest and most exhilarating air in the world. Never on the Alps, the Appenines, or the Jura have I felt so intensely, so exultingly, the abstract pleasure of mere animal existence, as on the Downs in the neighbourhood of Brighton. No manure, no decomposition on the surface, because no humidity will remain there; at such a distance, or such an elevation above the sea, that all which is insalubrious in the air has been deposited before it reaches them. A canter over the Downs in a fine day produces the feelings of the Arab in the desert: the breathing deep and complete, and every air-cell of the lungs fully opened and performing its duty." (pp. 39-40.)

"Speaking generally, and with exceedingly rare exceptions, (if any,) all congestive diseases are aggravated by a residence at Brighton. By 'congestive,' I mean such diseases as are caused or accompanied by local accumulation of blood, principally venous, circulating languidly in any of the organs of the body, and unattended with inflammation." (pp. 40-1.)

"Inflammatory dyspepsia is another of the ailments for which Brighton is objectionable; and, generally, all the diseases of the kidneys and bladder which are not attended with a considerable secretion of albumen, the latter being apparently benefited by the atmosphere which aggravates the others. Such at least is the result of my own experience in these obscure diseases." (p. 42.)

"Many invalids, whose ailments were stationary or retrograde while residing in face of the sea, or very near to it, have recovered immediately when I have sent them to the top of Montpellier Road, Montpellier Terrace, to the New North Road; to the row of cottages near the Poor House called Vine Place, or even to the Western Road, especially the western part of it; but these places afford few good houses." (pp. 43-4.)

"That delightful promenade, the Chain Pier, is so frequent a cause of indisposition in the early part of the year, that I have been accustomed to say that it ought to be maintained at the expense of the medical men of the town, from the practice it brings them. The sheltered walk under the cliff (which leads to it) affords a delightful resource, when a bitter north-eastern makes other places disagreeable. The perfect defence from the wind, with the benefit of a winter or spring sun reflected from the Cliffs, gives quite the feeling of summer, and this degree of shelter and warmth will extend perhaps to half the length of the Pier itself. At a certain point the protection of the Cliff ceases; you pass from a calm air (under the Cliff) at fifty-five or sixty degrees, to a keen wind at thirty-five [?] or forty, which from its rapidity produces the effect of a frost. The bright day has perhaps induced some change in the clothing, and with women and children especially, the mischief is often done in a few minutes." (pp. 45-6.)

"A common objection to Brighton on the part of Londoners is, that it is injurious to the eyes. This opinion, no doubt, takes its rise from the pain which often accompanies the comparatively permanent contraction of the pupil on changing the moderate light of a town for the vivid light of the south coast. It is a pain which ceases at the end of a few days, and, as far as I recollect, is always followed by an improvement of the vision. I know no exception. Brighton has the most extraordinary exemption from disease of the eyes of any place in Europe that is known to me." (p. 47.)

We wish Dr. Wigan would write a completer work on Brighton, pointing out the precise nature and peculiarities of its climate or climates, and its or their effects on different constitutions, temperaments, and diseases. This little work proves him to be well qualified for the task.

ART. XX.

Physiologie der Entzündung und Regeneration in den Organischen Geweben. Von Dr. HERMANN KLENCKE.—*Leipzig*, 1842. 8vo, pp. 230.

Physiology of Inflammation and Regeneration in the Organic Tissues. By Dr. HERMANN KLENCKE.—*Leipzig*, 1842.

WE have already, more than once, had occasion to speak briefly of some of the many works of Dr. Klencke. They have all been distinguished by the same character of vague idealism as is predominant in this; but, unlike his former publications, this contains a few new facts and experiments, and therefore merits a more lengthened notice than they did. Passing over the first 120 pages, we shall come at once to the *facts*, of which our readers may perhaps make a better use than the doctor himself.

1. *On the regeneration of nerves.* In several experiments, of which four are detailed, Dr. Klencke proved, both by the restoration of function, and by microscopic examination, the reproduction of nervous filaments in the frog; and he says he traced their gradual development through the same series of forms as those through which they pass in original construction. He found also that after division of the pneumogastric nerves of kittens, sensation gradually returned in the parts supplied by them; in these, moreover, he traced the gradual development of the reproduced nervous filaments, and, contrary to the statement of Haighton who assigned six weeks as the period sufficient for restoration, found that reunion of the divided vagus-nerve required sixteen weeks for its completion. In one case he divided the spinal marrow of a dog, and fourteen weeks afterwards the animal could move its hind-legs a little; it twitched its head when they were pricked, and licked its hind feet as if it had sensation in them. After death there appeared to be some restoration of the divided cord; there was a distinctly fibrous bond of union between its two portions, which were chiefly connected by a substance containing strongly developed cells, forming a swelling like a ganglion. In another case, after division of the ischiatic nerve of a kitten, he believes that a genuine ganglion was formed upon the divided part; for, four months after the operation, the limb twitched whenever it was pricked or otherwise stimulated, though the animal had no voluntary power over it; and this twitching continued after all the lumbar nerves had been divided. On examination immediately after death, a round swelling four lines in diameter was found at the part where the nerve had been divided.

"It was covered by a vascular neurilema, which gave the swelling a uniformly glistening, white and not striated appearance. It was now divided longitudinally and examined with a 250 times magnifying power. We found genuine cells such as are found in ganglia, and which here invested fibres which passed without interruption through the swelling near its axis. These fibres exhibited sharp curves, and even knot-like elevations which might be regarded as the cicatrices of the primitive fibres....We discerned a white and a gray substance; the latter consisting of genuine cell matter, the former of fibres. A very compact cellular tissue

sent processes from the outer coat into the tumour, and separated in many situations distinct bundles of fibres." (p. 139.)

The author has no doubt this was a genuine ganglion. The observation needs to be repeated by one of a less ideal disposition of mind: though the observations of Arnemann on the restoration of the brain, and two excellent cases in which the author found a complete restoration of both cortical and medullary substance in two soldiers who had lived long after *hernia cerebri*, with great loss of substance, prove that such an event as the production of a ganglion after an injury is far from impossible.

II. *Regeneration of bones, cartilages, and fibrous tissues.* In proof that losses of the wall of a bone may be repaired by growth from the medullary tissue, Klencke describes experiments in which he cut out wedges from the walls of bones and found, after a time, that the spaces were filled up more or less completely by growths of bone from the medullary tissue which was exposed by the removal of the wedges. He describes also two cases of more extensive reproduction after necrosis of part of the wall of a long bone, but they are not such convincing cases as he imagines, for it is not possible to say how deeply the necrosis extended in either case, nor therefore, whether the new wall grew from the surface of the medullary tissue, or from the remaining internal layers of the old wall.

In *internal necrosis* he concludes justly that "the outer layer of the cortical substance which remains healthy, can almost always become the basis for the reproduction of the new bone; and this, not by swelling, expansion, and deposition, but "by actual *catamorphosis* and new formation;" and he adduces several experiments in proof of this, some of which are satisfactory. At the same time he admits that this layer of bone is not essential to the reproduction; and he adds a few to the facts already known, which prove that the periosteum or the surrounding soft parts are capable of forming a new though imperfect bone, where an old one or its shaft, has been wholly destroyed.

Some cases are next adduced in which, after the union of fractures of long bones, the medullary tissue was completely restored; and some in which portions of skull having been removed by the trephine or after necrosis, the holes were nearly filled up by bone. A case is also related in which, in a scrofulous girl, nearly the whole of a scapula was reproduced. The sequestrum comprised all the bone except the acromion, and the surfaces of the edges of the costæ; and it was completely inclosed in new bone presenting, though somewhat roughly, the ordinary form of a scapula.

In the part on *reproduction of cartilage*, there is a probable case of reproduction of part of a costal cartilage in a man wounded five years before death; but on the whole, little addition is made to the few facts which render an occurrence of this kind at all probable. So also, in what is said on the reproduction of muscular and fibrous tissues, and of serous and mucous membranes we find nothing satisfactory. The facts which we have quoted give the work a small degree of interest, but our readers may rest contented that with less labour they have learnt as much from it as we have.

PART SECOND.

Bibliographical Notices.

ART. I.—*Some Account of the African Remittent Fever which occurred on board Her Majesty's Steam-ship Wilberforce, in the River Niger, and whilst engaged on Service on the Western Coast of Africa; comprising an Inquiry into the Causes of Disease in Tropical Climates.* By MORRIS PRITCHETT, M.D. F.R.G.S., Member of the Royal College of Physicians of London, Fellow of the Royal Medical and Chirurgical Society, late Surgeon of H.M. Ship Wilberforce, &c.—*London*, 1843. 8vo, pp. 216.

THIS book reached us at too late a period, last quarter, to allow us to notice it at the same time with Dr. M'William's work on the same subject. Although badly arranged and exhibiting a good deal of loose speculation and hypothesis, Dr. Pritchett's volume is very creditable to himself and to the service to which he belongs. It contains full, and no doubt accurate details of everything relating to the fever as it occurred in the Wilberforce, and, together with Dr. M'William's book, must be consulted by all future explorers of the African rivers. The valuable matter contained in Dr. Pritchett's volume would be much more useful to the reader, if the author had taken the trouble to put it in more formal book-shape, by dividing it into chapters and sections, and giving us a table of contents. As it is, we have no guide to help us through its mazes.

Out of the 184 pages of text, of which Dr. Pritchett's volume consists, nearly 100 pages are given to a detail of the cases (43 in number,) which occurred under the author's care. These give a sufficiently good view of the disease, but present no feature of novelty. The disease was evidently the same as that described by Dr. M'William, the ordinary remittent of warm climates. From Dr. Pritchett's disregard of statistics, it is not always easy to come at results, but it would appear that the cases of fever occurring in the Wilberforce were sixty of remittent and twelve of intermittent, in all 72; and that of this number, it appears from the table in Dr. M'William's work, that eleven died. We find no novelties in the post-mortem appearances; and the following is the only novelty in the *treatment*, which we recommend to the consideration of our readers:

"The best means of lessening the sense of burning heat which was so commonly complained of, I found to consist in encouraging the natural process of perspiration by keeping the body somewhat warmly wrapped up, while the patient was inhaling the open air, as cool as it could be had; the sick in the Wilberforce were placed under the awning on the upper deck. So profuse was the cuticular discharge under these circumstances, that the bedding commonly soon became saturated with the excretion. This plan of treatment was adopted from observing that, when an attempt was made to keep the surface cool by constant sponging with cold water, or vinegar and water lotions, and removing the usual covering, the skin always became dry and parched, and the patient obviously uneasy and nervous. Those, on the contrary, who were treated in the way indicated above,

lay quiet and bathed in perspiration, which evidently had a soothing effect on the whole system. On several occasions the warm bath was had recourse to; but any benefit resulting from it was by no means generally apparent." (pp. 167-8.)

Dr. Pritchett found bloodletting injurious; mercurialization at least useless; and quinine "not attended by that success which was anticipated." Some of our readers will smile at the following practical commentary on the author's statements, that "anything like activity, whether depletory or in the way of stimulation, was equally attended by an aggravation of the malady;" and that "the practice about to be recommended will probably be viewed by the advocates of either of these remedial plans, as *weak and wanting in energy:*"

"Where derangement of the chylopoetic organs is obviously present, the stomach ought to be emptied of its contents, and spasm of the hepatic ducts, if such exist, removed by the exhibition of an emetic; patients generally expressed themselves greatly relieved by its operation. A scruple of calomel, followed by a dose of castor oil, is next to be administered, with a view to clearing out the bowels. Should this not have proved efficacious, recourse may then be had to more active cathartics, such as croton oil, till the evacuations are sufficiently copious. These measures were followed up in the earlier period of the disease with doses of from five to ten grains of calomel, combined with three or four of the pulvis Jacobi veri, every two, three, or four hours, according to the urgency of the symptoms, till the mouth became slightly affected, when the medicine was discontinued." (pp.165-6.)

A considerable portion of the work is occupied by an exposition of the author's views as to the causes and nature of the African fever. He strongly combats the common notion of the cause being *malaria*, or an unknown extraneous poison originating in particular localities; and endeavours to show that the cause is to be found in the ordinary atmospheric influences of hot climates. He seems to regard the *moisture* of a warm atmosphere, by inducing important changes in the system, as the great predisposing cause, and direct exposure to the solar rays as the great exciting cause of the African fever, both acting more or less through the medium of electrical changes produced in the system. We think, however, that Dr. Pritchett has altogether failed to make good any of these positions; and although the theory of the miasmatic origin of fever has received a severe blow from some facts recently disclosed by Dr. Wilson and Major Tulloch, this cannot be said to be strengthened by anything advanced in the present work.

Art. II.—*Thèse sur le Delirium Tremens, ou Folie des Ivrognes, &c.* Par le Docteur Bougard.—*Bruxelles*, 1843. 8vo, pp. 163.

Thesis on Delirium Tremens, or the Madness of Drunkards, &c. By Dr. Bougard.—*Brussels*, 1843.

This thesis was composed on the occasion of the author's obtaining the title of "docteur aggrégé," from the faculty of medicine in the university of Brussels; and like many similar productions has a formal, syllogistic air; and contains not a few needless distinctions and divisions. At the same time, there is to be found in it a considerable amount of practical information and observation. Although we cannot perceive anything essentially novel, in regard either to doctrine or treatment, we can yet, with confidence, recommend the volume as a rather valuable and useful monograph.

ART. III.—*Ueber das Verhältniss der Medicin zur Chirurgie, und die Dreiheit im heilenden Staate, &c.* Von Dr. C. H. BISCHOFF, &c. — *Bonn*, 1842. 8vo, pp. 102.

On the Relation of Medicine to Surgery, and on the threefold Division of the Medical Profession. By Dr. C. H. BISCHOFF, Royal Prussian State Councillor, Professor of Materia Medica, and of Military and Civil Medical Police, &c. in the University of Berlin.—*Bonn*, 1842.

THE excitement now prevalent in Germany respecting the constitution and internal organization of the medical profession is the more worthy of notice as being contemporary with a similar movement in this country.

We have already noticed the state of the profession in Germany, in our review of Professor V. Walther's pamphlet, (Br. and For. Med. Review, vol. XIV. p. 402,) and we have little to add to the remarks we then made. Dr. Bischoff advocates a ternary division, namely, into the two classes of physicians and surgeons, equally educated in every respect, but distinct in practice; and a third class, comprising the assistants to the superior grades. Dr. Bischoff's arguments are mainly directed against the practice, now prevalent in Germany, of educating medico-chirurgical or general practitioners of an inferior grade of attainments, and have consequently a polemical tone neither pleasing nor convincing to the unprejudiced reader. It appears that the Medico-Chirurgs are poaching sadly upon the domains of the regularly-educated and titled doctors. Dr. Bischoff says that they argue thus with the public : "Herr A. is an excellent physician ; Herr B. an excellent surgeon ; and Herr C. is both : *ergo, fiat applicatio.*" And this argument is, and ever will be, valid and weighty.

The public wants men who are both, as we have previously shown, and all the rules and regulations that can be devised will never prevent that want of the public being supplied. Medicine and surgery are one and indivisible in teaching or in practice. The greatest difficulty reformers have to encounter is, as to who shall dispense medicines and perform the less honorable and more mechanical duties of the surgeon. If they be left in the hands of the druggist, that useful individual becomes *ipso facto* a surgical physician of an inferior grade, and we are where we were, with a tribe of ignorant *sage femmes en culottes* into the bargain. Probably the wisest plan would be to intrust the inferior and less responsible duties to junior practitioners; making them, in fact, serve a sort of medical curacy for a title. A service of this kind, if compulsory, would come to be considered no more degrading than the *status pupillaris* of the medical student is now.

ART. IV.—*Facts and Observations relative to the Influence of Manufactures upon Health and Life.* By DANIEL NOBLE, Surgeon.—*London*, 1843. 8vo, pp. 88.

THE leading article in our Journal for April last, written at our request by the author, "constitutes (as he states in his preface) the basis of the present publication." Had we not thought the original essay valuable, it would not have found a place in our pages ; and we will only say of the work before us, that the great mass of new matter contained in it is of the same important character as that we had the honour of printing. The pamphlet deserves the notice of statisticians and of all inquirers into the etiology of diseases.

Art. V. — *Darstellung der Aequilibrial-Methode zur sicheren Heilung der Oberschenkelbrüche ohne Verkürzung.* Von Georg Mojsisovics, Doctor der Medicin und Chirurgie, Primarchirurgen im k. k. allgemeinen Krankenhause.—Wien, 1842. 8vo, pp. 114.

An account of the Æquilibrial Treatment employed for the more successful union of Fractures of the Thigh-Bone, and the Prevention of any Shortening of the Limb. By George Mojsisovics, Doctor of Medicine and Surgery, Senior Surgeon of the Imperial Hospital at Vienna, &c.— *Vienna*, 1842.

This essay is of a purely practical nature, and contains the experience of the writer on the relative advantages and faults of the various and common modes of treating fractures of the thigh-bone, with an examination of the anatomical reasons of the various forms of displacement from the direction of the fracture, the action of the muscles, and the weight of the limb. A short outline is also added of the influence of accidental causes, as varieties of health, alterations of temperature and concomitant diseases, in retarding the union of the broken limb.

This list of contents necessarily embraces a large field of inquiry, which might perhaps seem rather cramped for space in a pamphlet; but the short practical style and complete absence of all controversial writing, have enabled the author to compress his observations into a small compass.

The peculiarity of the author's views consists in the peculiar apparatus which he has introduced and found exceedingly successful in the treatment of broken thighs. The other part of the work, though valuable for its sound sense and practical value, does not contain *novelties*, but rather a condensed view of the present *knowledge* of this subject, corrected by the writer's experience. To the novel part of this pamphlet we therefore now proceed:

The long splint of Desault and Mr. Liston, the double inclined planes of Sir Charles Bell and others, or the fracture bed of Mr. Earle, are all used in this country for fractures of the thigh. In the great mass of cases union is effected in a complete manner, and the patient recovers with a useful and well-shaped limb. When the fracture occurs below the upper third of the bone, the success is however greater than when the fracture is higher up. And it certainly must be confessed that the broken thigh does not always exactly assume its old form after union from fracture.

The limb may be short, or the lower portion inside the upper in fractures of the upper third of the thigh, the lower extremity of the upper portion is especially liable to jut forwards, whilst the upper extremity of the lower portion passes behind it; the lower portion is also not unfrequently rotated unduly outwards. Such, in short, are the most important faults after fracture of the thigh, producing shortening of the limb, more or less eversion of the foot, as well as alteration on the lateral and antero-posterior axes of the limb.

Whether the fracture be oblique or transverse, Dr. Mojsisovics' object is to obtain such accurate union that the femur shall retain the exact form of health. The form and situation of the fracture, also the number of fractures, if more than one, exercise a certain influence on the position of the part, but the muscles passing from the pelvis to the lower extremity form the

chief obstacle to a proper adjustment of the broken parts, and have chiefly attracted the notice of our author.

The muscles passing from the pelvis, as well as from the loins to the thigh-bone and knee, form, according to the writer, a cone, the base of which is above and the apex below. These muscles are but little under the control of bandages, and are liable to draw the lower fragment away from the upper, as well as to bend the thigh-bone upon the pelvis. Such, in short, are the general results of the action of the muscles which form the obstacle to accurate union, and which impediment the writer removes by his new apparatus. We need not give a minute detail of this, as its nature will be well enough understood by a general description.

The patient is placed on a firm bed, over which a framework is laid; this frame consisting of four posts connected by a horizontal framework. In short, let the reader picture to himself a common four-post bed, the posts and connecting transverse upper beams of which are very strong.

The patient is laid flat on his back, whilst a strong band, passed from one side of the bed to the other, fixes his pelvis on the bed firmly. The thighs are then bent at right angles to the body and placed in a vertical position, whilst the legs are bent at the knee, and consequently placed in a horizontal position at right angles to the thighs. Two broad folds of linen, passed from the upper beams of the frame beneath the calves of the legs, support them; pasteboard splints, fitting accurately to the thighs and buttocks, keep them in position; whilst a weight, passed over a pulley and fastened to the foot of the broken limb, extends the limb to the level of the sound limb placed by its side. The patient, in short, lies on his back in bed with his legs in the same relation to his body that they are when he is sitting in a chair, but now necessarily bent up in the air.

This position at first seems strange, but, on reflection, it will be found to deserve most serious attention: we think the plan reflects credit on the inventor.

Under the care of Dr. Mojsisovics all fractures of the thigh are thus successfully treated. Such a general use of this plan cannot be expected in this country, but in the treatment of fractures of the upper third of the thigh it deserves careful trial. Great difficulty is often experienced in bringing down the prominent end of the upper portion, but this plan of bringing up the lower portion to the upper certainly appears to remove the difficulty, and is likely to place the upper and lower portions in the same line, whilst their overlapping is prevented by the extension at the foot.

This mode of treatment presents nothing to render it dangerous or questionable, but unites a means of accurate extension with complete rest and support to the broken limb; superior, according to the writer's experience, to the common modes of treatment; and certainly, to all appearance, equally successful with them in the majority of cases of fracture of the thigh, and superior to many in the treatment of fractures of the upper third of that bone.

ART. VI.—*A Diagram to define the Lives of the Patriarchs, and the early History of the Seed of the Serpent and the Seed of the Woman, particularly in reference to the origin of disease and the danger of unsanctified knowledge.* By H. L. SMITH, M.R.C.S. &c.—*Cheltenham*, 1842.

RELIGIOUS physic seems to be fashionable. Our only wish, for the sake of religion as well as physic, is that the authors of works of this kind intermingled a little more common sense with their piety.

The writer of this odd little work seems in character to be the converse of that prince of whom we are told, "he never said a foolish thing, and never did a wise one." Judging from his book only, we should set him down as a man who was theoretically a visionary; but actually a benevolent, useful, well-principled, and charitable man. Spinning upon paper the thinnest and most unsubstantial theories; drawing large conclusions from the slenderest premises; generalizing from a single and a very doubtful observation; vain, self-opinionated, dogmatic, but with right and sound principles of duty; sincere, in earnest, actively benevolent to his poorer fellow-men; always right-hearted, however wrong-headed. The bulk of the volume is a religious theory; sent to the editor, we presume, from its containing an explanation of the "origin of disease;" and with this it is our province alone to deal.—Before the flood the earth was watered by a mist, not by rain; and the patriarchs lived on an average 912 years. But the flood came, and the earth was for a year saturated with moisture; and the average of the life of the post-diluvial patriarchs was only 332 years. Such is Mr. Smith's statement of facts; and the conclusion he draws is, that as wet is injurious to health the dampness of the earth, from its submersion for a whole year and from rain subsequently, was the cause of this shortening of life! But although Mr. Smith quotes the Bible copiously he has not read it attentively, for we are distinctly told that *before* the flood God shortened man's life to 120 years (Genesis, vi. 3,) so that this theory of wet is good for nothing.—There is, says Mr. Smith, another cause, an internal one, and this is anger, "a general term which implies many others; by some it is called passion, choler, irritability." (p. 27.) Recollecting that the world was destroyed by the flood on account of its being "corrupt and filled with violence," we felt curious to know on what grounds it could be asserted that we must explain our shortened span through the medium of so common an infirmity. And the only evidence brought forward to ground such a broad statement is "the fact (as recorded by Moses, and clearly shown in the diagram,) that Nahor,—the simple translation of whose name is 'Angry,'—and who was the eighth patriarch from the flood, *lived the shortest time* of any of the first ten post-diluvial patriarchs, being only 148 years of age when he died." (p. 27.) An exquisite example of a crotchet leading common sense astray!

Besides this lack of judgment and this marvellous defect in the reasoning powers, there is observable in the little book many indications of a vanity which is at once pitiable and ludicrous. This leads Mr. Smith to congratulate himself on his own deficiency of scientific knowledge: "I have digressed,—I may be wrong—I confess to being *willingly* unskilled in political economy, to being also *willingly* unskilled in the use of the geological hammer," &c. This, and much like this in the same strain, shows that if, like Dr. Johnson's opponent, Mr. Smith thanks God for his ignorance, he has very much to be thankful for. But, after such de-

bility and puerility it is refreshing to reach the appendix, and to find the theoretical writer to be a practically benevolent man; originating the self-supporting dispensaries, taking pains to carry into effect in his own neighbourhood the allotment system, and executing kind-hearted schemes for the amusement of the poor. Engaged in such active duties we respect him and wish him well; but he should leave "the patriarchs" to theologians, and avoid theorising.

Art. VII.—*Tic Douloureux, or Neuralgia Facialis, and other Nervous Affections: their Seat, Nature, and Cause. With Cases illustrating successful methods of Treatment.* By R. H. Allnatt, m.d. a.m. f.s.a. Second Edition.—*London*, 1843. 8vo, pp. 224.

We made a brief note of the character of Dr. Allnatt's book on its first appearance, in October, 1841. (See vol. XII. p. 524.) We notice its reappearance in its present form, chiefly for the purpose of inquiring whether the author is justified in now announcing it as a Second Edition. There has been no *reprint* of the work; this so-called second edition differing only from the former in having a new title-page, containing, in addition, the words "second edition" and the date "1843," in place of "1841," and in having a "superadded sequel to the appendix" of some thirty or forty pages, containing "additional cases successfully treated by the author." Let us put these questions to Dr. Allnatt: Suppose any one of our protegées, Drs. and Messrs. Ramadge, Turnbull, Yearsley, Curtis, &c. &c., was to print *annually* a "superadded sequel of cases successfully treated by the author," tack this to the tail of all the remaining copies of his book, and then announce the cobbled fabric as a New Edition,—could he not plead the same justification as Dr. Allnatt himself? By such a proceeding, would not our respected protegée obtain great advantages, necessarily attaching to the credit and flowing from the reputation of having written so good a book that the public call for a new edition every year? What would Dr. Allnatt think of such conduct? Would he not be apt to set it down as one of the multiform aspects of that worst of all quackery—quackery within the professional sanctuary? And yet would not these learned doctors and surgeons have precisely the same good reason for calling their annual puff a new edition, as Dr. Allnatt has for calling the volume before us a *Second Edition*?

If we did not know Dr. Allnatt to be a gentleman of honorable feelings and intentions, we would not put these questions to him. But we are desirous that dishonorable men should not be able to obtain any sort of sanction from the proceedings of their betters: and this nuisance of spurious New Editions we are determined on abating.

Art. VIII.—*Mental Hygiène, or an examination of the Intellect and Passions, designed to illustrate their influence on Health and the duration of Life.* By W. Sweetser, m.d.—*New York*, 1843. 8vo, pp. 270.

This volume, though containing little not to be found in works of a similar kind, is yet an intelligent, entertaining, and a popular exposition of the relative influences of the intellectual and moral, and the bodily operations. The subjects are illustrated by a variety of facts in natural history, by apposite physiological references and explanations, and by a number of interesting anecdotes. The volume, in short, is one which, while it is not useless to the professional reader, will be perused with profit and pleasure by the public.

Art. IX —*A Practical Treatise on the Diseases peculiar to Women.* By Samuel Ashwell, m.d. Part II, Organic Diseases.—*London,* 1843. 8vo, pp. 260.

We noticed the first part of this work in our Twenty-first Number, (Jan. 1841,) and gave a very favorable report of its general character and contents. Like almost all authors who allow themselves to be seduced into the publication of their works in parts, Dr. Ashwell has found—and his readers to their cost have found—that performance lags sadly behind promise in such cases. In September, 1840, we were told by Dr. Ashwell in Part I, that Part II would appear in December of the same year, while Part III, completing the work, would be published in March, 1841. Such is the promise: now for the performance: Part II appears in the summer of 1843, and announces that Part III is nearly ready! We believe Dr. Ashwell has better reason than many authors for pleading the old excuse of want of time; still we do not acquit him of blame for having consented to allow of the publication of a small portion of a work, bearing the specific promise of speedy completion, while the remainder was yet unwritten, and subject to all the accidents of a busy profession and uncertain life. We trust that the promise now given will be better kept. We shall wait until we see that it is so, before we give any detailed account of the present part. We will merely inform our readers that, besides chapters on hysteria and irritable uterus, it treats, in succession, of the following subjects: Tumours of the uterus; Premature labour complicated with organic disease; Organic diseases of the os and cervix; Congestion of the uterus; Metritis; Cancer; Ulcer and excrescence of the uterus; Rigidity and occlusion of the cervix; Diseases of the mucous membrane and polypus of the uterus. We may add, in conclusion, that the present part bears out the character we gave of the former. There is nothing very novel or brilliant in it; but it gives a good account of the different diseases treated of, and contains many plain, practical directions which we know not where to look for elsewhere under one head. To young practitioners, in particular, Dr. Ashwell's book cannot fail to be very useful.

Art. X.—*Mémoire sur l'Emploi du Carbonate d'Ammoniaque dans la Scarlatine.* Par le Dr. Rieken.—*Bruxelles,* 1843. 8vo, pp. 118.
Memoir on the Employment of Carbonate of Ammonia in Scarlatina.— By Dr. Rieken.—*Brussels,* 1843.

This memoir deserves attention, although the author appears to fall into the common error of exaggerating the virtues of the remedy which he recommends. Besides an historical notice of the use of carbonate of ammonia in scarlet fever, (for Dr. Rieken is not the original proposer of this agent,) there is an accurate account of the nature and symptoms of the disease, of the cases and periods in which carbonate of ammonia is to be had recourse to, of the ends it fulfils, and of its therapeutic modus operandi. The theories quoted on this last subject are so various, that we must refer the reader to the work itself for an account of them.

We have to add, that Dr. Rieken, though strongly impressed with a belief of the powers of carbonate of ammonia, as an anti-scarlatinal agent, by no means relies on it alone, but has also recourse to other very various and enlightened methods of opposing this formidable malady.

PART THIRD.

Original Reports and Memoirs.

REPORT ON THE OVUM OF MAN AND THE MAMMIFERA, BEFORE AND AFTER FECUNDATION.

By T. Wharton Jones, F.R.S.

Lecturer on Anatomy, Physiology, and Pathology at the Charing-Cross Hospital; Corresponding Member of the Imperial and Royal Society of Physicians and Surgeons of Vienna, &c. &c.*

Inasmuch as without a knowledge of the ovum before fecundation it is not possible to trace the first stages in its subsequent development, so the discovery of the ovum of the mammifera in the ovary must be viewed as the first great step towards the elucidation of generation and development in that class of animals. But as on its first discovery, the exact nature of the ovum in the ovary was mistaken, the subsequent detection in it of a germinal vesicle, by establishing its real import, must be viewed as the second great step towards the same important end. As a consequence, therefore, of these brilliant discoveries, the early development of the ova of the mammifera has now been worked out in as complete a manner as was previously the case only with the ova of birds.

I. Ova of man and the mammifera generally, before fecundation.

§ 1. *Graafian follicles.* The Graafian follicles, formerly supposed to be themselves the ovarian ova, were proved by the discovery of the real ova to be merely their containing capsules, capsules analogous to those which contain the ovarian ovum of the bird. The wall of the Graafian follicle is composed of interwoven fibres of cellular tissue and of blood-vessels. Its inner surface, where the capillary network is expanded, is smooth, and lined by a layer of nucleated cells, which, when the follicle is laid open, escapes in the form of membranous shreds. Originally the wall of the Graafian follicle is a simple structureless membrane, (*ovisac* of Barry, *membrana propria* of Henle and Bischoff,) around which are developed the cellular tissue and vessels which constitute its thickness subsequently.

§ 2. *Contents of the Graafian follicles.* The membraniform layer of nucleated

* This Report is founded on the following very excellent works of Professor Bischoff of Heidelberg:

1. Entwickelungsgeschichte der Säugethiere und des Menschen. Von Th. L. W. Bischoff.

History of the Development of the Mammifera and Man. By Th. L. W. Bischoff. Being Vol. Seventh of the new edition of Soemmerring's Anatomy.—Leipsic, 1842. 8vo.

2. Entwickelungsgeschichte des Kaninchen-Eies. Von Th. Ludw. Wilh. Bischoff, Doctor der Medicin und Philosophie, ausserordentlichem Professor der Medicin an der Universität Heidelberg, u. s. f. Gekrönte Preisschrift, ausgesetzt von der physikalisch-mathematischen Klasse der königlich Preussischen Akademie der Wissenchaften im Jahre 1840. Mit sechszehn Steintafeln.—Braunschweig, 1842.

History of the Development of the Ovum of the Rabbit. By Th. L. W. Bischoff, M. et P.D. Extraordinary Professor of Medicine in the University of Heidelberg, &c. Being a successful Prize Essay on the question proposed by the Physical and Mathematical Class of the Royal Prussian Academy of Sciences in the year 1840. With Sixteeen Lithographic Plates.—Brunswick, 1842. 4to.

Except when otherwise stated, it is to this second work that reference is always made.

cells* lining the Graafian follicle was named *membrana granulosa* by Baer. At the place corresponding to that side of the Graafian follicle which is prominent at the surface of the ovary, the membrana granulosa is thicker, presenting a greater accumulation of cells than in the rest of its extent. This accumulation of cells was, from a false analogy with the bird's egg, called by Baer, "discus proligerus." In the central part of it, which he called "cumulus," the ovum is imbedded, and may be seen as a minute opaque white speck, shining through when the wall of the Graafian follicle is transparent.

§ 3. *Dr. Barry's alleged tunica granulosa and retinacula.* The cells immediately surrounding the ovum have been viewed by Dr. Martin Barry† as constituting a distinct layer, which he names *tunica granulosa* of the ovum. Bischoff (p. 3) objects to this, remarking that the layer of cells in question is not distinctly defined, nor is it developed separately from the rest of the cumulus of cells in which the ovum is imbedded. As to the structure or structures consisting of a central mass containing the ovum, and of cords or bands extending from it to the membrana granulosa, which Dr. Barry describes under the name of *retinacula*, and which he alleges serve first to suspend the ovum in the fluid of the Graafian follicle; next to convey it to a certain part of the periphery of this follicle; subsequently to retain it in the latter situation; and finally to promote its expulsion from the ovary—Bischoff says he has never been able to see anything of the kind. He thinks Barry has mistaken irregular processes of the membrana granulosa in a lacerated state for a natural and definite structure.

§ 4. *Ovum.* The granular mass which the ovum presents in its centre is recognized by all observers, (including Dr. Barry in his 1st and 2d series, but excepting him in his 3d series,) as analogous to the yelk of the bird's egg.‡ Respecting the broad transparent ring or *zona pellucida* at its circumference there has been less unanimity of opinion; now, however, it is pretty generally acknowledged to be the optical expression of the circumferential doubling of a thick transparent membrane which incloses the yelk.

§ 5. *Yelk.* The yelk is composed of larger and smaller granules and globules, held together by a clear viscid substance, so closely and firmly in some cases that the yelk may be extracted entire in the form of a ball from its envelope, so loosely, on the contrary, in other cases, that when the external envelope is torn, it escapes in a formless flake. The smaller granules, Henle well remarks, are the most numerous, and resemble pigment molecules both in their appearance and in their peculiar motions, the larger, about 1-5000th of an inch in diameter, resemble fat or milk globules in their rounded form, dark edges, and shining surface.

§ 6. In the appearance of the yelk there is considerable difference in different animals and at different stages of development. The more mature the yelk the denser, opaquer and more coherent it is; the more immature, the more transparent and less coherent. According to Barry, (2d series, p. 349,) the difference between the mature and immature ovum consists in the yelk of the former containing no oil-like globules (vesicles), which that of the latter does. In the mature ovum, the yelk presents a peripheral stratum, sometimes appearing granulous, and at others seeming to consist of vesicles pressed together into a polyhedral form, its centre being fluid. In his 3d series, Dr. Barry substitutes for the name of "yelk" the expression "substance by which the germinal vesicle is surrounded," and points out (§ 337, et seq.) what he believes to be "the order of these different appearances, as well as the process to which they are referrible."

* The cells measure 1-2000th of an inch in diameter, but besides them, bodies four times larger are occasionally observed though in small numbers. In these larger bodies, Bischoff believes that he has distinguished a cell-membrane and nucleus, and throws out the conjecture that they may be intended for the formation of new follicles and ova.

† Researches in Embryology, 1st series. In Philosophical Transactions, Part ii. for 1838.

‡ Reichert distinguishes in the yelk of the bird's egg the central matter under the name of *formative vitellus*, the rest under the name of *nutrient vitellus*.

§ 7. *Zona pellucida. External membrane of the ovum.* That the zona pellucida is the optical expression of the circumferential doubling of a thick transparent membrane inclosing the yelk appears to have been first stated, independently of each other, by Coste and the author of this report. Bischoff maintains this view, but has fallen into error when he attributes it to Baer, who, on the contrary, describes the appearance of zona pellucida as owing to a transparent interval between a thin external membrane and the yelk. Valentin followed Baer in this view, and supposed the transparent interval was filled with fluid. Wagner formerly entertained a similar opinion, but he now admits the matter to be as above stated.

§ 8. Could any doubt remain on the point, the following *experimentum crucis*, which the author of this report made when he first investigated the subject, must remove it: Tear open the ovum under the simple microscope, and spread out flat a portion of the external envelope, then by delicate manipulation with fine needles double the membrane on itself, when the appearance of the double contour and broad transparent interval of the zona pellucida will be produced.

§ 9. Some observations published by Krause in Müller's Archiv for 1837, introduced great confusion into the discussions on the point in question. The albuminous mass around the ovum, which he describes as zona pellucida, is not what other authors describe as such, but must have been a part of the "discus proligerus," which in some cases is transparent immediately around the ovum. Dr. Barry has added to the confusion by attesting in his 1st series to the accuracy of Krause's statement, though what he says is actually an attestation to the opposite effect. In his 2d series, (Phil. Trans. Part ii. for 1839, p. 342,) Dr. Barry retracts this attestation.

§ 10. The thick transparent external membrane which, by being seen doubled on itself at the circumference of the ovum, gives rise to the appearance called "zona pellucida,"—is it analogous to the *vitellary membrane* of the bird's egg, or is there, in addition to it, another membrane entitled to the name of *vitellary?* The great importance of this question in reference to the tracing of the future development of the ovum was a few years ago forcibly insisted on by the author of this report, when he combated what will be shown to have been erroneous views promulgated by Dr. Martin Barry on the subject. Bischoff insists on the same point and adopts the same view as the author of this report.

§ 11. Before attempting to answer the question just proposed, let it first be inquired, what constitutes a vitellary membrane: The yelk of the bird's egg is immediately surrounded by a delicate, transparent, homogeneous, structureless membrane. It was to this that the name of vitellary membrane was originally given, and any part of the mammiferous ovum to be entitled to a similar appellation must be analogous to it in all its essential relations. That the thick transparent external membrane of the mammiferous ovum is the part analogous to the vitellary membrane of the bird's egg is maintained by Coste, the author of this report, Henle, Bischoff, &c., and, notwithstanding what has been said to the contrary by Valentin, Wagner, Barry, Meyer, and others, there is certainly no other membrane of the mammiferous ovum besides it to which the name of *vitellary* can be given.

§ 12. It has been above shown that Bischoff is inaccurate in saying that Baer described correctly the thick external membrane of the ovum; it must be here added that he is equally inaccurate in what he says regarding Baer's views of a vitellary membrane. Baer, in fact, considered the outer membrane of the ovum as analogous to the membrane of the shell in the bird's egg, and he accounts for a vitellary membrane by conjecturing that there forms on the outer surface of the yelk-ball a thin pellicle. But, as has been observed by the author of this report, "there is no such pellicle as that spoken of by Baer; and all analogy leads to the supposition that the external envelope of the ovum of the mammifera does not correspond with the membrane of the shell of the bird's egg, but rather with the vitellary membrane."

§ 13. The preceding quotation and a reference to papers in vols. xxi* and xxiv† of the Medical Gazette, will show that Bischoff is mistaken in classing the author of this report with those who admit a membrane in the mammiferous ovum, in addition to the thick external one as entitled to the name of vitellary. So far indeed from admitting any such membrane himself he criticised Barry in a very decided manner for doing so. (Med. Gaz. vol. xxiv., p. 592.) The author of this report, in fact, believes he was the first to maintain that the thick external membrane of the mammiferous ovum, is analogous to the vitellary membrane of the bird's egg in all its essential relations, before as well as after‡ further development.

§ 14. The inaccurate observations of Krause regarding the zona pellucida have been just referred to. What he describes and delineates as vitellary membrane, is in fact what in this report is maintained to be not only vitellary membrane, but also the membrane which, by being seen doubled on itself at the circumference of the ovum, gives rise to the appearance called *zona pellucida*, whereas, as has been seen, Krause attributes the latter to a different cause.

§ 15. Wagner, in his Physiology, rather infers the existence of a vitellary membrane distinct from that which yields the appearance of *zona pellucida* than says he has actually observed such. The ground of his inference is that in some cases the granules and cells composing the yelk hold together independently of the external membrane. This however, it has been above seen, is owing to a uniting viscid substance, and not to any proper inclosing membrane.

§ 16. Barry does not, like Wagner, infer merely that a vitellary membrane distinct from the thick transparent external membrane of the ovum exists, but says he has seen, nay actually delineates such. In his first series he says it is not always to be found. In his third series, (Phil. Trans., Part ii, for 1840. p. 534,) he says that several such membranes successively arise and disappear in a single ovum; a circumstance, he remarks, which will perhaps serve to explain why some observers have never seen such a membrane under the thick transparent membrane. Further, according to Dr. Barry, there are formed at the periphery of the yelk in ripe ova successive layers of discs or cells, one layer liquefying, being replaced by another. It is around these successive layers, that his successive vitellary membranes are formed.

§ 17. Did such membranes really exist, they could not be admitted as analogous in all essential relations to the vitellary membrane of the bird's egg, and therefore not entitled to the name of vitellary in the conventional sense. But in regard to Barry's statements, Bischoff observes, p. 10: "Never before, nor yet since, have I, in the course of my most careful examinations of the ovarian ovum with very good instruments, (and such I possess, one by Schieck and one by Oberhäuser,) been able to perceive any trace of such a cortical layer of the yelk. . . . . It ought also to be observed that not one of the many careful observers of the ovarian ovum has seen anything of the kind. . . . . . "But," Bischoff continues p. 11, "if I have not been able to see this cortical layer of cells, much less have I ever been able to see a fine membrane investing it."

§ 18. As to Dr. Meyer, he alleges that liquor potassæ dissolves the membrane on which the appearance of zona pellucida depends, but leaves a membrane investing the yelk. On repeating this experiment, however, Bischoff found that liquor potassæ *does not dissolve the zona*, but merely causes great contraction of

* On the Ova of Man and Mammiferous Brutes, as they exist in the ovaries before impregnation; and on the discovery in them of a vesicle analogous to that described by Professor Purkinje in the immature egg of the bird. Read before the Royal Society, June 18th, 1835.

† Observations on the Ova of the Mammifera before and after impregnation, in reference to Dr. Martin Barry's "Researches in Embryology."

‡ On the first Changes in the Ova of the Mammifera in consequence of impregnation, and on the mode of origin of the Chorion. In Philosophical Transactions, Part ii. for 1837.

it, as well as of the yelk. The zona thus changed is what Meyer has mistaken for a different and distinct vitellary membrane.

§ 19. Bischoff comes to the conclusion already arrived at by the author of this report, that the yelk of the mammiferous ovum in the ovary is composed of a quantity of granules held together by a connecting substance, and *possesses besides the zona pellucida no other proper investment.* The zona pellucida, or rather the membrane on which the appearance of zona depends, therefore, if it must have a name, ought to be called *vitellary membrane.**

§ 20. *Germinal vesicle and germinal spot.* By the discovery of the germinal vesicle in the mammiferous ovarian ovum, the complete analogy between the latter and the ovarian ovum of the bird, &c. was established, and Baer's error regarding it dissipated. The correct view of the matter had been suspected by Purkinje, but he and Valentin had in vain searched for a germinal vesicle, and it was only on renewing their investigations after the announcement that such a vesicle had been discovered in the rabbit's ovum by M. Coste, that they, Wagner and others in Germany, were successful in finding it. M. Coste, therefore, as Bischoff observes, must, notwithstanding his very imperfect description and delineation of the germinal vesicle,be considered as its first discoverer. It is nevertheless, Bischoff continues, quite certain that the author of this report made the discovery independently, and demonstrated the vesicle in a much more definite and certain manner, inasmuch as by opening the ovum he exhibited it in an isolated state. (Kaninchen-Eies, p. 12, and Soemm. vol. vii, pp. 8 and 14.)

§ 21. Though he has made use of compression for demonstrating the germinal vesicle, Bischoff says that he succeeds with greater certainty by opening the ovum under the simple microscope by means of a very finely-pointed needle. This was the plan originally adopted by the author of this report. If the vesicle thus isolated, says Bischoff, be brought under the compound microscope, we see that it represents a simple cell with a very fine transparent and structureless membraneous wall with perfectly clear contents.

§ 22. The size of the germinal vesicle, Bischoff found pretty constantly to be in the mature ova of the rabbit about 1-600 inch. The size varies somewhat both relatively and absolutely, but within very narrow limits. It is relatively larger in small ova. The discrepancy in the statements regarding its absolute size given by different authors appears to depend more on the mode of examination than on actual difference. When compression has been used it appears of greater diameter than when observed in the perfectly unaltered state,—without the ovum being compressed or laid open.

§ 23. At one side of the germinal vesicle there is a small round dark spot discovered and described contemporaneously by R. Wagner and the author of this report. Wagner first gave it the name of "germinal spot," and recognized it to be of general occurrence throughout the animal series. This was a discovery of great value, and considered as such by both its authors, though it is to be confessed that the true import of the germinal spot became evident only after Schwann's application to animal structures of Schleiden's theory of development of vegetable structures from cells. In accordance with this theory, Wagner now calls it *nucleus germinativus*;—but more on this head below.

§ 24. Wagner says that he has sometimes observed in the mammiferous ovum the germinal vesicle with more than one spot, as in the ovipara. Neither Valentin nor Bischoff have ever seen such a case. The germinal spot of the mammiferous ovum is a disc about 1-3000 inch in diameter, composed of a finely-granulated substance, strongly refractive of light and attached to one part of the inner surface of the wall of the germinal vesicle which it causes to be more prominent externally there than elsewhere.

§ 25. As regards the position of the germinal vesicle in the yelk, it has been

* In the remainder of this Report, the name "vitellary membrane," followed by the word "zona" within brackets, will be employed to express the thick external membrane of the ovarian ovum.

already remarked by previous observers that it is found in immature ova more in the centre, in mature ova more towards the periphery of the yelk. Bischoff says that it does not appear to be in the yelk surrounded by any particular mass or formation, (*discus proligerus* properly so called,) as is the case in the bird's egg. Perhaps the true state of the case is that in those ova in which the yelk-granules and cells hold together in the manner above described, the germinal vesicle is imbedded in one side of the yelk-ball, and that in a definite manner as in a proligerous disc. The germinal spot, it is to be observed, is always towards the periphery, and is frequently the only part of the vesicle visible, the rest being covered by yelk-grains, a condition which can depend alone on the germinal spot being more prominent than the rest of the vesicle.*

§ 26. In his third series of Researches in Embryology, p. 531, Dr. Martin Barry says, under the head of preparatory changes in the germinal vesicle and germinal spot, that "the germinal vesicle does not 'burst,' 'dissolve away,' or 'become flattened,' on or before the fecundation of the ovum, as hitherto supposed. It ceases to be pellucid; and this perhaps is one cause of the mistaken views regarding it. But another cause is possibly a less transparent state assumed by the surrounding substance; and a third, is doubtless the almost entire absence of observations on mammiferous ova at this period." According to Dr. Barry, the germinal vesicle fills with cells, and these in their turn with the foundations of other cells, so that the germinal vesicle is gradually rendered nearly opaque, and becomes very much enlarged and flattened. The position of the germinal vesicle does not change, but it becomes more determinately applied to the investing membrane than in the immature ovum. The pellucid part of the altered germinal spot, at which the foundations of new cells arise, is directed towards the surface of the ovum. More particularly the part in question is directed towards an attenuated region or an orifice in the thick transparent membrane,—vitellary membrane, (zona.)

§ 27. From these statements of Dr. Barry, Bischoff dissents in the most decided manner. "Neither before," says Bischoff, (p. 15,) "at a time when I was endeavouring with the greatest care to make out the relations of the germinal vesicle in mature ova, nor yet since my attention has been again turned to the subject by Barry, could I observe the slightest indication of what he describes. I expressly instituted for the purpose examinations of the ova of a rabbit much in heat. Another experienced observer who assisted me did not succeed better."

§ 28. *First formation of the Graafian follicle.* Bischoff agrees with Henle in viewing the Graafian follicle in its early state when it has for its wall a simple structureless membrane as similar in nature to those vesicles which Henle and Goodsir find to be the elementary constituents of glandular structure.

§ 29. This view of Henle does not coincide with the representation given by Valentin of the development of the ovaries and Graafian follicles. The latter he describes as being formed within blind tubes, which originally constitute the structure of the ovaries, as the seminiferous tubes the testicles, but which become obliterated by the increasing growth of the Graafian follicles. Bischoff, however, says he has never been able to discover any such structure in the ovary as that which Valentin describes, notwithstanding every possible attention in the examination of the embryos of man, the cow, sheep, sow, dog, rabbit, hare, and rat.

§ 30. Henle's view is likewise not in agreement with what Martin Barry says on the subject. According to the latter, the first-formed part of the Graafian follicle, which he calls *ovisac*, is developed around the already existing germinal vesicle.

§ 31. Though Valentin differs from Henle and Bischoff in the account of the conditions under which the Graafian follicle is first developed, he appears to agree with them that the ovum is formed within the Graafian follicle, and not, as Barry states, the Graafian follicle around the rudiments of the ovum.

* "What first strikes the eye," says Henle, Allgm. Anat. p. 969, "is in general not the bright contour of the germinal vesicle, but the dark germinal spot."

§ 32. *First formation of the ovum.* Purkinje, the original discoverer of the germinal vesicle, conjectured that this might be the part of the ovum first formed. The observations of Baer and of Wagner on certain of the invertebrata showed that as regards these animals such is the case; and as regards vertebrata, Dr. Martin Barry believed his observations warranted him in stating that in the rabbit and pigeon at least the germinal vesicle is the part of the ovum first formed.

§ 33. The following is Bischoff's account of the first formation of the mammiferous ovum: The fundamental part of the Graafian follicle being formed in the manner above described, contains a clear fluid, in which are suspended nuclei and granules, the latter quite similar to the subsequent yelk-granules. The size of the Graafian follicle at this period varies from 1-1000th to 1-333d of a Paris inch. On the inner surface of the *tunica propria* of the follicle, there is deposited a layer of endogenous cells as an epithelium. After this there is found in the centre of the follicle a nucleated cell perfectly similar to the germinal vesicle, which indeed Bischoff holds it decidedly to be. Around this vesicle are then found deposited those granules similar to yelk-granules, and that in greater quantity the more advanced the development is. At the next stage, when he was able to satisfy himself of the precise state of matters, Bischoff found in the Graafian follicle the ovum with all its essential parts, viz. vitellary membrane (zona), yelk, germinal vesicle, and its spot. The smallest follicles in which such an ovum could be distinguished measured 1-100—1-20th of a Paris inch in diameter.

§ 34. In the ova at this period, the vitellary membrane (zona) is very faint, and its external boundary not well defined. The yelk also is still clear. These parts, therefore, are difficult of being seen through the wall of the Graafian follicle, especially as it is no longer so transparent as formerly, in consequence of its *membrana propria* being now surrounded externally by a quantity of fibre-cells, from which is developed the cellular and vascular coat which subsequently forms the wall of the Graafian follicle. Hence Bischoff has not been able to perceive the formation of the vitellary membrane (zona). Everything, however, appears to him in favour of Valentin and Henle's opinion, that the yelk-granules are deposited around the germinal vesicle, and then become surrounded by the vitellary membrane (zona). Bischoff here repeats that in the formation of the ovum he has never seen in addition to the vitellary membrane (zona) a trace of any other membrane to which the name *vitellary* might be given, such as Valentin and Barry allege exists.

§ 35. The earlier the stage of development, the larger is the ovum in proportion to the Graafian follicle. The ovum indeed is at first almost closely embraced by the Graafian follicle, as the ovarian ovum of the bird is by its capsule. The earlier also the stage of development, the larger is the germinal vesicle in proportion to the ovum.

§ 36. *Position of the ovum and its component parts in reference to the theory of cells.* Schwann doubted whether the ovum should be viewed as a primary cell, its nucleus being the germinal vesicle, its nucleolus the germinal spot, and its contents the yelk-granules, or the germinal vesicle and its spot as standing in the relation to each other of cell and nucleus. The latter is the view which appears most consistent with all the circumstances of the case; the germinal spot has the size and form, and in relation to the germinal vesicle also the position, of a cytoblast, though direct observation in the higher animals has not shown that the germinal spot is first formed, and that around it is developed the germinal vesicle as a cell around its nucleus; but in insects, according to Wagner, the spot appears first.

§ 37. Adopting the view that the germinal vesicle and spot are respectively primary cell and nucleus, the signification of the yelk and its membrane remains to be accounted for. Bischoff coincides with Valentin and Henle in the view that the yelk and its membrane are deposit formations around the germinal vesicle, which thus, without actually being a nucleus, stands in the relation of one to the deposit formation around it. The exact mode in which the vitellary membrane (zona) is formed is still obscure. Bischoff conjectures that it results from the

fusion of a peripheral layer of cells. Barry suggests (3d series, p. 538,) the consideration whether the "zona pellucida" may not be *formed* by a succession of his alleged vitellary membranes. He says further, it would appear that the ovisac is a great cell, as well as possibly the thick transparent membrane (vitellary membrane, zona.) The substance surrounding the germinal vesicle (the yelk of authors generally and of Barry's 1st and 2d series) in certain states exhibits changes similar to those presented by a nucleus, so that the substance in question seems to be a great "cytoblast."

§ 38. Is the part which the germinal spot plays concluded with the formation of the germinal vesicle around it, or has it still an important part to play? To the first question Valentin, Henle, Kölliker and others, to the second question Barry and Vogt, give the affirmative. Barry considers the germinal spot as the central point of new cell generations, a view which he extends to cell nuclei in general. Vogt considers the germinal spot not a nucleus, but a cell itself of the highest importance for the further development. This question will come to be considered further on.

II. On fecundation, and on the discharge of the ovum from the ovary—corpora lutea.

§ 39. In many of the lower animals it is well known that actual contact of the seminal fluid with the ova takes place; that this contact is a necessary condition for fecundation has been proved by the experiments of Spallanzani and many others. As regards the mammifera, though there is less direct evidence in support of the thesis that for fecundation actual contact of the seminal fluid with the ova is necessary, still that evidence appears conclusive. In the first place the observations of Leeuwenhoeck, Prevost and Dumas, Bischoff, and others positively show that, *post coitum*, all the surfaces over which the ovum or ova must pass in their course from the ovary to the uterus become spread over with spermatozoa—evidence of the presence of semen,—so that mutual contact of the male and female generative elements must necessarily take place. Secondly, the experiments of Haighton, Blundell, and Bischoff, to say nothing of the practice of sow-gelders,* constitute evidence of a negative kind. They show that when by destruction of the female genital passages anywhere from the upper part of the vagina to the fimbriated extremities of the fallopian tubes, the access of the seminal fluid to the ovum in the ovary, or of the ovum to the seminal fluid in the genital passages, is prevented, impregnation never occurs.

§ 40. But the question arises, does the fecundating contact take place first in the ovaries, or not until the ova have been expelled from them and received into the Fallopian tubes? If analogy with what is the case among those animals in the impregnation of which the fact and the necessity of actual contact of the seminal fluid with the ova have been most directly proved, be adopted as a guide in the determination of this question it must be said that fecundating contact does not take place until the ova have been expelled from their capsules in the ovaries and received into the Fallopian tubes.

§ 41. This view was maintained by Prevost and Dumas, not however on the grounds of analogy, but because they never discovered spermatozoa beyond the Fallopian tubes. The fact discovered by Bischoff and Barry, however, that the semen penetrates to the ovary overturns the force of this argument of Prevost and Dumas, but does it prove, as Bischoff and Barry think, that fecundating contact first takes place in the ovary; that contact of the seminal fluid with the ovary is a necessary condition for fecundation?

§ 42. The rarity of the occurrence of spermatozoa on the ovary, as observed by Bischoff and Barry, does not appear very favourable to this view, but Bischoff accounts for this rarity by supposing that there is one particular time only at which spermatozoa are to be found on the ovary, and at which the observer is not always fortunate enough to make his observation.

* In the operation of spaying, the operator appears more particular about removing the fimbriated extremity of the Fallopian tube than the whole of the ovary.

§ 43. The best proof that contact of the seminal fluid with the ovary is a necessary condition for fecundation, would be that Graafian follicles burst only after such contact, or when there was reason to believe that such contact had taken place. But, although Bischoff says, that when the semen comes into contact with the ovary, the Graafian follicles, previously much distended, become still more so, and at last give way, there are numerous facts to prove that Graafian follicles burst independently of any contact of the semen with the ovary. To say nothing of those cases to which there will be occasion to return, in which Graafian follicles have been found burst independently of the access of the male; the experiments of Haighton and Blundell on rabbits show that though by obliteration of some part of the female genital passages, the access of semen to the ovary was prevented and in consequence of that impregnation, still Graafian follicles were observed to have burst, and *corpora lutea* formed *post coitum.*

§ 44. Cases of ovarian conception, admitting the occurrence of such,* appear to argue most strongly in favour of the fecundating contact taking place in the ovaries; but from the cases on record no very certain inference in regard to the question can be drawn: at the most it can only be said that an ovum capable of being impregnated, not being expelled from the ovary in consequence of the access of the male, but being by some cause retained in that organ, there receives the fecundating contact of the seminal fluid which may have penetrated so far, as it has been found to do in the dog and rabbit. Such an abnormal occurrence, however, would constitute no proof that fecundating contact ordinarily takes place in the ovary.

§ 45. There are thus no other means left of determining the question but by inquiring whether there be any changes in the proligerous disc, or in the ovum itself while still in the ovary, sufficient to show whether it has or has not been fecundated. But before inquiring into these points, the following merits consideration:

§ 46. *Mode in which the seminal fluid is conveyed along the genital passages towards the ovaries.* It appears pretty certain that at the moment of ejaculation, the seminal fluid is received directly into the uterus. Bischoff found few or no spermatozoa in the vagina of bitches and rabbits after coitus, but the uterus quite full. Within the female organs the seminal fluid does not coagulate into a thick and viscid mass, and the spermatozoa are observed to be particularly active. It may be concluded therefore that the seminal fluid may reach the ovaries partly by diffusion through the mucus on the genital passages, partly by the movements of the spermatozoa. The rate at which spermatozoa can move is, according to Henle, one inch in seven minutes and a half, a rate quite sufficient to account for the arrival of some out of the great numbers poured into the uterus, within the time after coitus, at which they have been found on the ovaries. There is another means which appears adapted to promote the progress of the seminal fluid towards the ovaries, and that is quick and progressive, but not peristaltic contractions of the uterus and Fallopian tubes from the vagina onwards, which have been observed by Bischoff immediately *post coitum* in bitches and rabbits, especially the latter.

§ 47. The vibratile cilia of the epithelium of the uterus and Fallopian tubes, as their movement is from the ovaries outwards, if they do not hinder, can contribute nothing to the inward progress of the seminal fluid. Weak in the uterus, the ciliary motions are active in the Fallopian tubes, and are well adapted to carry the delicate ovum along them to the uterus. Bischoff in general found ciliary motion stopped in that part of the tubes already traversed by the ova, but still brisk in the parts yet to be traversed. He likewise found spermatozoa only *before* not *behind* the ova in the course of the latter along the Fallopian tube. Wagner found no ciliary motions in a pregnant animal, nor in one which had just littered.

§ 48. *The changes in the cells composing the membrana granulosa and proligerous disc* observed by Bischoff and Barry, *post coitum,* around ova from much distended Graafian follicles, and which will be more particularly noticed further

* It will by and by be seen that the existence of ovarian conceptions is closely connected with the question of the development of the chorion.

on, might be adduced as proof of fecundation taking place in the ovary, but this cannot be admitted as evidence of the fact any more than the presence of the corpora lutea which form in the ovaries, when it was impossible seminal fluid could have arrived at them.

§ 49. *Alleged changes in the ovum.* Dr. Barry believes that there are changes in the ovum, which favour the supposition that the ovary is the usual locality in which the ovum is fecundated. "*Post coitum,*" he says (in his second series, p. 350, &c.) "before the discharge of the ovum from the ovary, the germinal spot, previously on the inner surface, passes to the centre of the germinal vesicle; the germinal vesicle, previously at the surface returns to the centre of the yelk; and the (alleged) proper membrane of the yelk, previously extremely thin, suddenly becomes thickened. To these changes, Dr. Barry, in his third series, adds, that the orifice which he alleges presents itself in the thick transparent external membrane,—vitellary membrane (zona)—as a change preparatory to fecundation, probably closes before the ovum leaves the ovary, he having in no instance seen it after the discharge of the ovum from the ovary.

§ 50. Of all the above alleged changes, the return of the germinal vesicle to the centre of the yelk, is considered by Dr. Barry as sufficient in itself to show that an ovum has undergone fecundation. *Ante coitum,* it is as *near* as possible to the surface of the ovum, as if to receive the contact of the semen; *post coitum,* it is withdrawn as *far* as possible from that surface.

§ 51. That such a process as this takes place has not been affirmed by any other observer. A little reflection on the small size of the ovum and the relative size of the germinal vesicle and yelk, will incline the reader to think with the author of this report that the alleged *migrations* of the germinal vesicle are a mere conceit, that it is absurd to speak of the germinal vesicle being seen in its passage *half-way* between the surface and the centre of the yelk. A few granules, more or less, on this or that side of the vesicle is all the difference that could really exist.

§ 52. Bischoff has never seen such migrations of the germinal vesicle, and as to internal changes of the germinal vesicle and spot, Bischoff says that he considers the statements of Barry on this head very doubtful. He several times found the germinal vesicle in ova at the period in question quite unchanged, without any of the alleged remarkable metamorphoses of its spot which, had they existed, could not have escaped his observation. He therefore decidedly contradicts this statement of Barry. Bischoff thinks it probable, however, that the germinal spot is the point of fecundation; if so, and if proximity to the surface be a necessary condition, it has been above seen that such exists, for the germinal spot, even when the vesicle is covered by the yelk grains, projects free at the surface of the yelk. But more of this below. What Barry says about the thickening of the proper membrane of the yelk is quite unfounded, seeing that no such membrane exists, as has been above shown, and as Bischoff again repeats in reference to this period.

§ 53. As to the alleged orifice in the thick external membrane of the ovum, this leads to an inquiry into the *nature of the action of the semen on the ovum.*

That the spermatozoa are an essential part of the seminal fluid, may be considered as an indisputable fact, proved by their existence in the secretion of the testes of all animals capable of generating, and their absence in unfruitful animals, but do they immediately and materially act in the process of fecundation? That they do, appears to be supported by the experiments of Prevost and Dumas with the seminal fluid of the frog deprived of spermatozoa by filtration, in which no fecundation took place. But without adducing an experiment of Spallanzani, attended with contrary results, (for it might be objected to it that there is no certainty that all spermatozoa were really excluded,) it may be said, that as a sort of coagulation takes place in semen, the filter may retain other matter besides the spermatozoa, and give passage merely to serum, which it can easily be understood will be as inefficient for fecundation as serum of the blood is for nutrition, secretion, &c.

§ 54. When the spermatozoa were first discovered, it was fancifully supposed that in fecundation they got into the interior of the ova and became the embryos.

It is known that Prevost and Dumas revived this notion in a modified shape. Dr. Barry however is the first, indeed the only person who has alleged that he has actually observed spermatozoa in the interior of ova. This was first in the case of a rabbit of twenty-four hours, and a second time at a somewhat earlier stage. (Phil. Trans. Part I, 1843.) He had previously asserted (3d series, p. 533,) that on one occasion, in an ovum of five hours and a half, he saw in the alleged orifice of the thick transparent membrane—vitellary membrane (zona)—an object very much resembling a spermatozoon which had increased in size. Dr. Barry states that in his recent cases of spermatozoa actually within the ovum, the above-mentioned alleged orifice was no longer visible.

§ 55. In regard to this point, Bischoff says, (p. 31,) that in the cases in which he found spermatozoa in bitches and rabbits on the ovaries, or in which the ova had just escaped from the ovaries, (he once found, he says, in one bitch two ova in the tube, and one on the ovary escaped from the newly-burst follicle,) he never saw either a fissure or opening in the vitellary membrane (zona,) nor of course, a spermatozoon penetrating into it. He therefore does not hesitate to doubt very much this observation of Barry.*

§ 56. "I must here," Bischoff says, "call to recollection the condition of the ovum. It is surrounded by a pretty dense layer of cells of the membrana granulosa and "discus proligerus," which, especially in perfectly mature ova, closely invest its vitellary membrane (zona). For these reasons I hold it impossible to observe such an alleged opening in the vitellary membrane (zona), and such alleged changes in the germinal vesicle. But any one practically acquainted with the matter must hold it utterly impossible to recognize here, under this mass of cells, a spermatozoon in any such opening. But it may be said that the observation might have been made after the ovum was freed from the surrounding cells. The manipulation necessary for this, however, would be sufficient to remove the spermatozoon, although it had really been present. It is much to be wished," Bischoff continues, "that Barry's statements on this point may not be too hastily adopted."

§ 57. "We find the ova in the tubes," Bischoff goes on to say, (p. 32,) "always covered with numerous spermatozoa, but I have never been able to satisfy myself of the presence of one of these in the interior of the ova. I have indeed, on two occasions, in compressing an ovum taken from the tube, by the compressorium under the microscope, observed, when the yelk-grains had escaped from the burst vitellary membrane (zona), a spermatozoon very distinctly among the yelk-grains, and it seemed to come out from the ovum. But considering, as above said, that the ovum is covered round with spermatozoa, and that these spermatozoa are difficult of removal on account of being inclosed in layers of albumen, mistake is here both very possible and very probable. Moreover, it ought not to be forgotten that the possibility of the entrance of spermatozoa into the ovum is very doubtful, notwithstanding Barry's allegations. On this point we may refer to the many previous observations of others, in which in *no animal* has any opening in the vitellary membrane been seen. And how would it be with those ova which are fecundated only when they have already become surrounded by an albuminous layer, as in fishes and frogs?"

If a spermatozoon, Bischoff concludes, really penetrate, it must do so in some other manner than that alleged by Barry.

§ 58. The orifices in the thick transparent membrane of the ova delineated by Dr. Barry, appear to have been clefts accidentally produced by pressure or other violence. In regard to figs. 167 and 168 of his 3d series, representing what he supposed the spermatozoon in the orifice, it must be admitted with Bischoff, that they are little calculated to make the observation probable. Supposing spermatozoa really in the ova, it must of course be understood that they get in

* The observation detailed in his 3d series of Researches. Bischoff, when writing this, was not acquainted with Dr. Barry's more recent alleged observation of spermatozoa actually within the ovum.

only after the bursting of the Graafian follicle. But then, according to Dr. Barry, the orifice in the thick external membrane — vitellary membrane (zona) — is closed.

§ 59. Even although Dr. Barry's recent observations be correct, it would be premature to admit them as proof that fecundation is effected by the direct and immediate agency of the spermatozoa.*

§ 60. The opinion most in conformity with all known facts is that the liquor seminis is the material, which entering the ovum by imbibition directly fecundates it. The spermatozoa are to be looked upon as being to the liquor seminis what the blood-corpuscles are to the liquor sanguinis, essential to the maintenance of the proper constitution of their respective fluids. Such an opinion was recently published by Valentin.

§ 61. Bischoff thinks that it is while in its ovarian capsule that the ovum is fecundated, and that the liquor seminis is imbibed through the walls of the follicle in order to get to the ovum; the author of this report is not prepared to deny this, but taking into consideration all the circumstances above passed in review, he is of opinion that there is no proof that fecundation takes place until the ovum escapes from the Graafian follicle and comes into direct contact with the semen.

The only unequivocal evidence of fecundation having taken place appears to be the remarkable subdivision of the yelk which occurs, but as will be seen not until the ova are in the Fallopian tube, a subdivision of the yelk similar to that which is known to be the first evidence of fecundation in the ova of those animals which admit of being directly observed after the application of the seminal fluid, (e. g. frogs, &c.)

§ 62. How long *post coitum* is it before the Graafian follicles burst? On this head there has been considerable difference in the statements of different observers. But it is pretty certain that the time varies in different species of animals, if not in the same. As regards the rabbit, Barry's numerous observations show that the average time is between nine and ten hours. Bischoff agrees in this. In the dog the time is variable but later. As regards man, nothing certain is known. It was supposed by Prevost and Dumas, that in the rabbit all the ova of one impregnation are not discharged about the same time. Bischoff agrees with Barry in contradiction of this.

§ 63. *State of the ovum from the period post coitum until it leaves the ovary.* The reader will perceive that several of the changes in the ovum mentioned in the following quotations from Dr. Barry, have been already considered in the discussions on the process of fecundation; but it is necessary here again to bring them under notice in correlation with the other changes which take place, or are alleged to take place, in the ovum at the period now under consideration.

§ 64. "From my observations," says Dr. Barry (2d series, p. 314, § 140; Phil. Trans., Part ii. 1839,) it appears that there is no condition of the ovum uniform in all respects which can be pointed out as that in which it is expelled; though the same observations lead me to conclude that plate v., figs. 96 and 97, present a state which is frequent when the discharge takes place, viz. the germinal spot has a central pellucid point; it is situated in the centre of the germinal vesicle; the latter has a dark contour, and perhaps a double membrane; around it is an ellipsoidal mass; the yelk is a fluid containing granules; the proper membrane of the yelk (?) has thickened, highly refracts light, and is often reddish-brown in colour; there is a minute space filled with a transparent and colourless fluid between this membrane and the thick transparent external membrane—vitellary membrane (zona); and finally the tunica granulosa (?) and retinacula (?) ("discus proligerus") present the appearance of incipient liquefaction.

§ 65. The following is the substance of what Dr. Barry says and delineates in

* The process of fecundation in plants has been thought to favour this view; but without any good reason in analogy.

his 3d series (p. 534, Phil. Trans., 1840,) regarding the "changes in the ovum immediately after fecundation, and before the ovum leaves the ovary."

In plate xxiii., fig. 169, he delineates an ovum of five hours and a half, in which the germinal vesicle was apparently undergoing the change of place towards the centre of the ovum. The vesicle had begun to regain its globular form. The point of fecundation, however, was still visible at the periphery; whence, and from the unclosed state of the fissure in the thick transparent external membrane—vitellary membrane (zona),—he is disposed to think the ovum had not been long fecundated, for the point of fecundation is subsequently seen to occupy the centre of the germinal vesicle, and soon after fecundation the orifice in question is no longer seen.

§ 66. In his 3d series Dr. Barry, as was above mentioned, drops the expression "yelk" and employs that of "substance by which the germinal vesicle is surrounded." The same process of cell-formation which in the mature ovum he alleges had begun in this substance before fecundation, he describes as being continued after fecundation. In the ovum, fig. 170, the germinal vesicle having receded from the surface into the interior of the ovum, had become closely surrounded by a layer of cells, each of which presented a remarkably opaque nucleus. Subsequently this nucleus seems to resolve itself into cells, and the same origin of new cells appears to take place in its interior as that which he had described in a former page (noticed in this report under the head of Germinal Vesicle). It appears also that new layers of cells come into view internal to the layer just described, when a succession of the same changes takes place as those already mentioned. Layer after layer of cells makes its appearance in the interior,—often seen to have become circumscribed by a proper membrane,—while cells occupying a more external situation undergo liquefaction.

§ 67. The cells of the (alleged) tunica granulosa undergo, immediately after fecundation, a remarkable alteration in position, size, form, and internal condition. Being first loosened and made less adherent to one another, they become club-shaped, greatly elongated, and connected with the thick transparent membrane —vitellary membrane (zona)—by thin pointed extremities alone. They present in their interior, at the large extremity, a pellucid space apparently corresponding to the *enlarged nucleolus* of other cells. This space is surrounded by dark globules. Subsequently there is seen, instead of this pellucid space, a cell-like object which contains a colourless and transparent fluid; but does not exhibit any proper nucleus or "cytoblast," and the surrounding globules become scattered. At a later period, these cells of the tunica granulosa are found filled with other cells.*

§ 68. Bischoff tells us, that several years ago he observed that ova from much distended Graafian follicles, *post coitum*, presented a peculiar condition of the cells of the *membrana granulosa*, and especially of those of the "*discus proligerus*," around the ovum; an appearance which he never saw in other ova, although apparently quite mature. The cells appear larger, more transparent, the nucleus is then more distinct, and they adhere more closely together, so that when the follicle is opened they do not become scattered in the fluid, but the membrane *in toto* escapes as a viscid jelly-like mass. The cells of the disc become at first club-shaped, their pointed extremities being attached to the zona or thick external membrane. The clear nucleus is very distinct in them, but Bischoff never saw it as Barry represents it, nor yet filled with young cells. The cells afterwards extend into a spindle-shape, whereby the ovum acquires a peculiar stellate appearance. This appearance Bischoff found constant in dogs and rabbits, and as Barry has observed it also, it may be considered as certain.

§ 69. In the appearance of the ovum in other respects, Bischoff found nothing changed except that the vitellary membrane (zona) was generally somewhat thicker, and the yelk appeared quite full and dark. In Soemmerring, he repeats,

* The second and third of these conditions, however, so far as Dr. Barry's observations have extended, are not generally met with until the ovum has been discharged from the ovary.

that he never could observe any vitellary membrane distinct from the zona, and in this respect contradicts Barry in the most emphatic manner. He never saw, as above mentioned, any orifice or fissure in the vitellary membrane (zona), although he purposely examined six ova of a bitch eighteen hours and a half post coitum, after he had freed them from the cells of the disc. As little did he ever see the cell-layers of the yelk mentioned by Barry, but the yelk always appeared finely granular. On every occasion he looked most carefully for the germinal vesicle. Sometimes he found it, sometimes he did not. He observed, that in consequence of the greater density and coherence of the yelk, it was more difficult to observe and extract the vesicle than in other cases; so that it might have existed in those ova in which he did not find it. In those cases, however, in which he did find it, both it and its spot appeared quite unaltered, without any of the remarkable metamorphoses of the spot mentioned by Barry; which, had they existed, could not well have escaped his notice, especially in the case of the bitch above mentioned. The objection, that in the case in question the ova might not have been destined to escape from the ovary on this occasion, Bischoff meets by saying that the ova were at least perfectly mature; and Barry, as has already been seen, affirms that the changes of the germinal spot and germinal vesicle present themselves *ante coitum* and independently of it.

§ 70. Bischoff does not hesitate to express it as his present conviction, although circumstances render the proof very difficult, that the germinal vesicle at the end of the period between coitus and the escape of the ovum from the ovary in general dissolves. The moment of disappearance, however, is not definite; and he thinks it probable that the vesicle may not disappear until the ovum is in the Fallopian tube. From observations on other animals it is known that the germinal vesicle disappears sometimes before, sometimes after, the escape of the ovum from the ovary; sometimes before and sometimes after coitus; what alone is constant is that in ova in which development has actually commenced, the germinal vesicle is no longer found. The same thing he thinks holds in regard to the mammifera.

§ 71. *Corpora lutea.* It has been above seen that the experiments of Haighton and Blundell on rabbits show, that though by obliteration of some part of the female genital passages, the access of semen to the ovary was prevented, still Graafian follicles were observed to have burst and *corpora lutea* formed after the accomplishment of coitus. In similar experiments Bischoff found pretty-fully developed *corpora lutea*, which he says could not well have depended on a previous fecundation. In the page preceding that in which Bischoff mentions this experiment (p. 43,) he says that he has never found fully-formed corpora lutea without coitus having taken place; from which it might be inferred that he must suppose that the excitement of coitus alone determines the formation of corpora lutea, and that independently of the contact of semen with the ovary; but in Soemmerring (p. 35) he is found saying, in reference to man, that a corpus luteum is no proof of previous coitus, because the bursting of the Graafian follicle, of which he here says the corpus luteum is a proof, is evidently often the result of other causes, such as sexual excitement in general, and menstruation.

§ 72. There are indeed numerous facts to prove that Graafian follicles burst independently of the access of the male. The researches especially of Dr. Robert Lee, and subsequently those of MM. Gendrin and Negrier, have rendered it extremely probable, if not certain, that in the human female the phenomena of menstruation are intimately connected with the periodical spontaneous bursting of Graafian follicles.

§ 73. But the question here arises, is the corpus luteum, which is formed by the bursting of a follicle independently of the access of the male, similar to a corpus luteum which is formed in consequence of impregnation? Bischoff says in regard to the rabbit, as has been above seen, that he never found fully-formed corpora lutea without previous coitus; but he adds, that he has several times seen Graafian follicles filled with blood, but containing no ovum, independently of

coitus, and he thinks that the Graafian follicle does not burst, but only swells and becomes filled with blood, whilst the ovum is absorbed, and a false corpus luteum formed. It is to be remarked that Bischoff, though he here speaks of fully-formed corpora lutea and false ones, does not give any mark of distinction between them; and from what he says in Soemmerring (above quoted) it might be inferred that a corpus luteum is the same, whether resulting from, or independently of, the access of the male.

§ 74. The observations of Dr. Lee show that there is a difference between the corpora lutea formed under the two different conditions above referred to. In the false corpus luteum the yellow substance is at the inner surface of the wall of the Graafian follicle, and does not surround it, as is the case in true corpora lutea. In a specimen of false corpus luteum, which the author of this report examined with Dr. Lee, there was yellow matter deposited in the coats of the Graafian follicle, which were spongy and thickened. The cavity of the follicle was distended with coagulated blood: the inner layer had a peculiar wrinkled appearance, similar to what Baer has represented in his figure of the true corpus luteum, and the suspicion could not be avoided that it was a corpus luteum of this kind which Baer represents in his work 'De Ovi Mammalium et Hominis genesi,' as a true corpus luteum.

§ 75. Baer's opinion that the corpus luteum consists in a growth inwards of the inner layer of the Graafian follicle, is supported by Bischoff in opposition to opinions entertained in this country, viz.—that the yellow substance is contained between the two layers of the wall of the Graafian follicle according to Dr. Montgomery; or that it is outside both according to Dr. Lee. Barry's statements appear to favour Montgomery's view, though in reality he agrees essentially with Baer, and that apparently without knowing it. Barry considers his "*ovisac*" as the inner layer of the Graafian follicle; now Baer, in speaking of the inner layer of the Graafian follicle undergoing the change to form a corpus luteum, could not have referred to Dr. Barry's "*ovisac*," inasmuch as he did not know of the existence of such a structure, but to the inner layer of the cellulo-vascular wall of the Graafian follicle —the *covering of the ovisac* of Barry. All this indeed Barry is aware of; but in § 157 of his second series, unnecessarily regrets to find himself expressing an opinion at variance with that of Baer; and in his subjoined note coincides with Montgomery in his view of the corpus luteum, but inaccurately, according to his own principles, inasmuch as Montgomery speaks of the layers of the Graafian follicle in the same sense as Baer.

§ 76. The fact is, Dr. Barry's "*ovisac*," or, as Bischoff calls it, *tunica propria** of the Graafian follicle exists only at the commencement of development, and is not to be found afterwards. "From my observations on the formation of the Graafian follicles," says Bischoff, p. 45, "I have indeed admitted a *tunica propria*, which becomes overlaid externally by a layer of fibres, and with this represents the follicle. But I have never found that this *tunica propria* admits of being separated as a distinct layer of the follicle, but have found reason for admitting it only in the process of development." Barry says (2d series, p. 317, § 154), that in a few hours after the ovum has been discharged from the Graafian follicle, the "ovisac" may be readily removed by pressure from the burst Graafian follicle; but in § 155 he says, that at the end of several days the primitive ovisac is no longer met with in the ovary; and in a note he remarks, that he does not know whether in the interim the ovisac has been absorbed *in situ* or first expelled, but in the hog he has found what seemed the remains of ovisacs in the infundibulum. In reference to these statements Bischoff observes, that he has never seen that any *tunica propria* becomes loose from the rest of the follicle after the expulsion of the

* The terms *ovisac* and *tunica propria* are employed in the descriptive and illustrated catalogue of the museum of the Royal College of Surgeons, vol. v. in a different sense from that in the text. *Ovisac* is used for the whole Graafian vesicle or follicle; *tunica propria* is applied to the cellular and vascular wall of the Graafian vesicle or Barry's "covering of the ovisac;" whilst *ovarian vesicle* is used for what is referred to in the text under the names of *ovisac* of Barry or *tunica propria* of Bischoff.

ovum. The gelatinous-like mass found in the follicle in the first period after the escape of the ovum, he asserts, is by no means the tunica propria of the follicle or the ovisac, as it has been seen Barry thinks, but the fluid of the follicle and membrana granulosa which had not all escaped, changed and become more consistent, and which by great development of its cells has been converted into a viscid coherent mass." Barry's own figure 98, plate 5, is corroborative of this assertion of Bischoff.

§ 77. To return to Montgomery and Lee. The cases adduced by these gentlemen unequivocally show, that human true *corpora lutea*, at least, are not growths of the inner layer of the Graafian follicle—the inner layer in the sense of Baer and not of Barry—for in such corpora lutea not long after conception the inner layer is found but little changed, and not at all the seat of the yellow growth.

§ 78. The yellow substance, it is to be observed, is a new formation, and not a *conversion* of the cellular tissue of any part of the wall of the Graafian follicle, as appears to be the opinion of Baer, Barry, Bischoff, and the author of the notices in the Catalogue of the Museum of the College of Surgeons. The designation of the corpus luteum by the latter, as the "thickened parenchymatoid proper tissue, or tunic of the ovisac," must be held to be erroneous. From the statements in the Catalogue regarding the corpora lutea in the College of Surgeons' Museum, it cannot but be admitted, that the preparations support the views of Montgomery and Lee, so far as these two gentlemen concur, rather than those actually given in the Catalogue.

§ 79. As above stated, Montgomery is of opinion that the yellow substance is deposited between the two layers, into which the cellulo-vascular wall of the Graafian follicle may be separated by dissection, whilst Lee maintains that the yellow substance is wholly outside both, and that any appearance of membrane between the yellow substance and the stroma of the ovary, which may in some cases present itself, is due simply to condensation of the neighbouring part of that stroma. From the examination he made of the very recent corpus luteum described by Dr. Lee in the Medico-Chirurgical Transactions a few years ago,* the author of this report was led to concur entirely in this view of Dr. Lee; other cases which he has examined support the same view; and it may be observed that the statements in the description of the corpora lutea in the Catalogue of the College of Surgeons are unequivocally to the same effect.

§ 80. It may not be uninstructive in conclusion, to inquire how it is that Baer, Bischoff, and others, should have viewed the corpus luteum as a growth of the inner layer of the Graafian follicle. In regard to the human corpora lutea described by Baer: in several cases the author of this report has verified the correctness of Baer's description; but in all there was no doubt, from the history, that the corpora lutea had been developed independently of impregnation. As to Bischoff's account of the corpora lutea in the rabbit, it will be found on reexamination that the *whole wall* of the Graafian follicle has been contracted together and pressed by the increasing growth of the substance of the "corpus luteum" situated really between it and the stroma of the ovary towards the most prominent part, where by being inverted through the aperture in itself by which the ovum escaped, it forms the pouting papilla-looking body described by De Graaf, Cruikshank, and Barry. The description, in fact, given by the latter, viz.: "The mammillary process appears to consist solely of an inverted portion of the vascular and spongy substance which previously constituted the covering of the ovisac," completely bears out what is here contended for,† and does not in the slightest degree warrant the conclusion he himself comes to, and which he designates as *obvious*, viz.

* Valentin (Repertorium, vol. vi.) mentions his having observed a similar case to this. In a woman who died in the fourth month of pregnancy, there was what he calls "a hollow cyst, such as is described by Lee and Jones" in the corpus luteum.

† The same process appears to take place in the formation of the corpora lutea in the cow, &c. and is the explanation why a central cavity is not found as in human corpora lutea, but simply the smooth circular depression on the most projecting part.

that *the covering of the ovisac becomes the corpus luteum.* (2d series, p. 318, § 156).

III. Changes which the ova undergo in their passage along the Fallopian tubes.

§ 81. Von Baer was the first who, with a knowledge of the ovarian ovum of the mammifera, found ova in the Fallopian tubes. This was in a bitch. The conclusion he came to from his observation was, that the ova undergo but little alteration in their passage through the tubes. The author of this report, the next observer in order of time to Baer, described ova from the Fallopian tubes of the rabbit, and showed, on the contrary, that in that animal at least they undergo very remarkable changes. The subsequent and more extended researches of Barry and Bischoff, while they confirm generally the author's statements, supply more detailed information.

§ 82. It is purposed first, to consider under one head *the changes in the envelopes*, then under another, *the changes in the vitellus, including the germinal vesicle*, and after that to examine them in their correlations.

§ 83. *On the changes in the envelopes*. Baer represents the thick external membrane of the ovarian ovum as still constituting the external envelope of the ova, which he found in the Fallopian tubes of the bitch. And Bischoff (writing in 1838) says: "it is quite certain that the ovum receives no new external investment in the tube; it acquires no albuminous layer and cortical membrane like the ovum of ovipara; and I can therefore state positively, that the outer covering of the ovarian ovum also remains the outer covering, (always excepting the decidua,) a circumstance which had been spoken of as probable by Baer, Wagner, Coste, and others. Wharton Jones," continues Bischoff, "the only person who pretends to have observed this formation of albumen and cortical membrane around the ovum, was probably deceived by the 'discus.' He has either not been sufficiently acquainted with it from the ovary onwards, or no longer expected it in the fecundated ovum, (as indeed it does disappear at a later period,) and has taken its granules for the albuminous layer forming around it." (Wagner's Physiology, Leipzig, 1838, p. 96, § 69.—Willis's Translation, London, 1841, p. 141.)

§ 84. Dr. Barry, (writing in 1838 also,) not less positively, though in an indirect manner, contradicts the author of this report, inasmuch as he speaks of the thick transparent external membrane of the ovarian ovum, as continuing in the tubes and uterus the external membrane. Having, as already seen, admitted without any foundation a vitellary membrane, in addition to the thick transparent external membrane of the ovarian ovum, Dr. Barry discusses this thick transparent external membrane in a section of his 1st series, entitled "the true chorion, a structure superadded within the ovary in the class mammalia."*

§ 85. The observation by the author of this report, thus so decidedly contradicted, directly by Bischoff, indirectly by Barry, was that in a rabbit three days *post coitum*, he found in the Fallopian tubes, near where they enter the horn of the uterus, ova differing very remarkably from ova as they exist in the ovaries before impregnation; inasmuch as they each presented, in addition to the component parts of the ovarian ovum, a thick gelatinous matter surrounding it, similar to what is observed around the ovum of the frog in the oviduct.†

§ 86. When Bischoff wrote the remarks above quoted from Wagner's Physiology, his observations had been confined to dogs. Observations since made on

* It was from the views he propounded in this section, that Dr. Barry was led to substitute the word *ovum* for *ovulum*, which was that originally employed by Baer to designate the body he discovered in the ovary. But as the views were erroneous, so was the reason for the change of name founded on them. It is to be remarked, however, that the change of name from *ovulum* to *ovum*, which the author of this report was the first to make, is correct, and that for the reasons adduced by him.

† On the first Changes in the Ova of the Mammifera in consequence of impregnation, and on the mode of origin of the Chorion. In Phil. Trans. Part ii. for 1837.

rabbits have shown him "that the ovum of the rabbit, like that of many ovipara, becomes surrounded in its progress along the Fallopian tube with a layer of albumen." Barry's* subsequent observations have also convinced him that in the ovum in the Fallopian tube, "an outer membrane becomes visible, first as a dark circle around the thick transparent membrane," then by the imbibition of a fluid having "no small consistence," it becomes distended, and thus separated from the thick transparent membrane, so that the latter now appears as if surrounded by a gelatinous-looking substance.

§ 87. The existence of an additional investment† around the ovum of the rabbit in the Fallopian tube being thus admitted, and that by persons who previously denied it, it remains to inquire into its nature and mode of origin. It may in the meantime be observed that the author of this report indicated this additional investment as the basis of the future chorion. In this view he was followed by Barry,‡ but as will be seen to a certain extent only by Bischoff.

§ 88. As above seen, the author of this report described the additional investment around the rabbit's ovum from the tube, as a gelatinous-looking matter, similar to that presented by the laid ovum of the frog. Dr. Barry objects to this view, saying that the additional investment consists of a membrane having a sensible degree of thickness, retaining a fluid which is interposed between it and the thick transparent membrane. In proof of this he adduces cases in which he observed that when the thick transparent membrane was ruptured by crushing, and not the additional investment, the contents of the ruptured membrane (yelk) proceeding no further than the dotted line, (See his fig. 128, pl. viii,) showed this to be the inner surface of a thick membrane, which had been concealed by an equal refracting power in the fluid interposed between it and the thick transparent membrane. In accordance with his view of the additional investment now stated, Dr. Barry in all his figures employs a dotted line to indicate *diagrammatically* the distinction between the fluid and its retaining membrane, which together form, as he alleges, the additional gelatinous-looking investment of the ovum in the tube.

§ 89. In reference to these views of Barry, Bischoff observes: "I hold it an easy matter to determine, that we have here to do with a gelatinous-like substance, and not with a fine membrane inclosing a fluid. And for these reasons:

a. "We cannot by microscopical examination convince ourselves of the membranous nature of the outermost limits of the said structure.

b. "When pressure is applied, there is never produced the effect which would be exhibited on a fine bladder filled with fluid, but that which was to be expected from the crushing of a gelatinous-like matter.

c. "In the most certain manner however, we convince ourselves by dissecting the ovum with a fine needle, under a good simple microscope. By this means (the same originally employed by the author of this report,) we can cut off segments of every description from this layer, which according to Barry's view had been impossible. I have adopted this procedure not only as a proof of the nature of the layer, but also frequently for the purpose of cleaning the zona, in order to be able to examine more closely the contents of the ovum. There is no small difficulty in removing this gelatinous-like mass from the ovum with the needle. In short, I can say that there is scarcely any point of my subject on which I am

* It is to be observed that Bischoff still maintains that in the dog no albumen forms around the ovum in the tube. If this be found by further observation to be really the case, Bischoff's error consisted in applying too hastily to the rabbit what he had found in the dog; Barry's error, on the contrary, is due to inaccurate observation, inasmuch as rabbits were the subjects of his researches.

† It is to be remarked that Cruikshank must have observed this additional investment. It, in fact, appears to be what he designated as chorion; but the author of this report was the first who, with a knowledge of the ovarian ovum, discovered it, and recognized it as a superaddition analogous to the gelatinous-looking matter which the ovarian ova of the frog acquire in their passage through the oviduct.

‡ In the quotations from Barry it will be observed that the word *chorion* is used to express the additional investment.

so certain as on this. The albuminous investment is elastic and yielding to pressure; that it consequently may sometimes give rise to an appearance such as is represented by Barry in figs. 128 and 130 of his 2d series, in which the yelk mass is effused between the zona and the albuminous layer, is not to be wondered at."

§ 90. As to the origin of this gelatinous looking investment of the ovum: two observations were adduced by the author of this report, to show that it is first acquired by the ovum in the ovary, from a change of the granules of the "proligerous disc" immediately surrounding it, but that in the Fallopian tubes it swells out, and acquires a greater diameter, as the corresponding part of the frog's ovum does when extruded, by the absorption of the water and seminal fluid with which it comes into contact.

§ 91. From the concurrent testimony of Bischoff and Barry, however, it would appear that the addition is first made to the ovum in the Fallopian tube, and that although the cells of the "proligerous disc" around the ovum in the ovary continue to invest it after its arrival in the tube, they are, Bischoff says, eventually entirely separated from it, and contribute nothing to the formation of the gelatinous-looking investment. Bischoff never saw any such layer of gelatinous-looking matter around the ovum in the ovary, as the author of this report described in the two observations above referred to, and as Bischoff never found ova still in the ovary forty-one and forty-eight hours post coitum, as is alleged to have been the case in those observations, Bischoff considers that the author of this report has erred, misled probably by the contents of the Graafian follicle, which at this time have become dense and gelatinous-looking.

§ 92. Bischoff and Barry differ however, as to the mode of origin of this investment.

Dr. Barry in his 2d series described it as *rising* from the thick transparent membrane of the ovum—vitellary membrane (zona)—in the Fallopian tube, and imbibing fluid, and thus becoming distended. In fig. 53, pl. ix. 2d series, Dr. Barry delineates an ovum of seventeen hours from the middle of the Fallopian tube, showing the mode of origin of the chorion. This membrane is rising from the thick transparent membrane or "zona pellucida," surrounded by part of the tunica granulosa. The interpretation which might be given of this figure is, that the cells of the "discus" were coalescing to form the gelatinous-looking investment. This Dr. Barry himself thinks probable at p. 342 of his 2d series, but in his 3d series, though he finds that it is formed of cells, it is not of the cells of the "disc" brought from the ovary. They are much more minute, and have a very different interior and form. Successive additions of such cells collect around the thick transparent membrane and coalesce. Imbibition of fluid and consequent distention then take place. Having thus in his 3d series on Embryology, determined that the chorion is formed of cells arising in the oviduct, Dr. Barry is found in his 1st series on the Corpuscles of the Blood, announcing that the chorion is formed of cells which are altered corpuscles of the blood! an announcement for which he truly apprehended physiologists were not prepared.

Bischoff describes the new investment on its first appearance, as a very thin perfectly transparent layer around the ovum, of a gelatinous-looking substance, which might easily be overlooked, but does not say how this substance is first formed. The investment increases in thickness as the ova proceed in the tubes. It has a stratified texture, and deserves in every respect the designation of white of egg. From this it appears that Bischoff supposes that the investment of the ovum under consideration acquires its thickness by the addition of succeeding layers like the albumen of the bird's egg.

§ 93. If the chorion be recognized as an essential part in the development of the ovum, and if it be admitted that, in the rabbit at least, the additional gelatinous-looking investment has an important share in the composition of the chorion, it will be seen that an ovarian conception, and the opinion according to which the formation of the additional gelatinous-like investment does not take place until the ovum has arrived in the Fallopian tube are incompatible; either an ovarian conception cannot take place in the rabbit, or the additional gelatinous-like investment may be formed in the ovary. If Bischoff's observations, however, be ad-

mitted as correct in regard to the ovum of the dog, and his conjectures in regard to the human ovum, viz., that there is in them no such additional gelatinous investment, but that the basis of the chorion is the vitellary membrane (zona), the same difficulty does not occur.

Though the author of this report is not disposed to put much confidence in the two observations above objected to by Bischoff, in consequence of the unfavorable circumstances under which they were made, still it is evident, from the difference in the statements of Barry and Bischoff on the point, that further observations than those yet adduced by them are required to determine the place and mode of origin of the new investment. Barry found it on an ovum of forty-one hours, and advanced only one inch in the Fallopian tube. Here the difference between Barry and the author of this report is merely as to the place where the ovum was found. It remains to be proved that ova are never found in the ovary so late as forty-one hours.

§ 94. In their progress along the Fallopian tubes, the ova are found to become double, or even more than double the size of ovarian ova. This increase in size is principally due to the addition of the gelatinous-looking investment. The vitellary membrane (zona) only becomes a little thicker. An accumulation of fluid is observed between it and the yelk, but the room for this fluid is supplied rather by the condensation of the yelk than the distention of its membrane.

§ 95. In ova in the Fallopian tubes, Bischoff says he never could perceive, any more than he had done in ova in the ovaries, any proper vitellary membrane in addition to the zona. Barry, still adhering to his notion that such a membrane exists, says that after having suddenly thickened, and being remarkably distinct, it disappears by liquefaction in the tube; so that the yelk is now immediately surrounded by the thick transparent membrane (zona pellucida) of the ovarian ovum.

§ 96. *The changes in the vitellus, including the germinal vesicle*, now come to be considered. A process of division of the yelk into more and more minute and more and more numerous, globular masses in a geometrical progression with the factor two, has been found to accompany the first development of the ova in many animals. In regard to the first discovery of this process in the ova of the mammifera, Bischoff quotes a passage,* and refers to a figure, (Fig. iii,) in Baer's 'Epistola,' which show that Baer had observed in the ova from the Fallopian tube of the bitch, the yelk undergoing a *resolution into globular segments ;* though the nature of the appearance escaped him, viz., its analogy to that division and subdivision of the yelk of the ovum of the frog after impregnation, first discovered by Prevost and Dumas.

§ 97. The nature of the appearance, however, did not, as Bischoff asserts, (Soemmerring, p. 58,) entirely escape the author of this report, the next observer in the order of time; he observes in Part ii of his paper (Phil. Trans. 1837,) entitled "on the changes in the vitellus," that what he has to say regarding the changes in the vitellus in consequence of impregnation, relates chiefly to the ova of the batrachian reptiles, and is added merely for the purpose of throwing some light on the changes which take place in the yelk of the ova of the mammifera, previously to the commencement of the evolution of the embryo. Having determined, as he thinks, that the disappearance of the germinal vesicle is prior to, and not dependent on impregnation, he asks what is the first change which takes place in ova, in consequence of impregnation? In answer to this he says that of all ova, the ova of the frog are those in which such change can be most directly observed, and that in them the breaking up of the surface of the yelk as described by Prevost and Dumas is the first change he has seen. He then goes on to state, as the result of this breaking up of the surface of the ovum of the frog, that the blasto-

* Medium tenet globulus sub microscopio penitus opacus, *superficie non lævi et æquali, sed granulosa, totus enim globulus e granulis constat dense stipatis,* membrana cingente vis conspicua. Globulum circumdat, interjacente spatio pellucido arcto, peripheria quædam stratu tenui granulorum minimorum obtecta. Post nycthemeræ macerationem hujus pulveris majorem partem sejunctam inveni; quo facto membrana continua et simplex venit in lucem......... Mira est ovorum nostrorum parvitas. Quæ sub microscopio metitus sum 1-15 lineæ partem tantum diametro explebant. (p. 11.)

derma, consisting of an aggregation of clear globules, different from those of the rest of the yelk is now fully formed. After this, he says the change which takes place in the yelk of the bird's egg appears to be limited to the neighbourhood of the cicatricula. In the ovum of the mammifera, there being little more than a blastoderma to be formed, the whole of the vitelline grains undergo a change, and are resolved into a spherical blastoderma, presenting the same peculiar friable and globular texture as the blastoderma of the egg of the newt, frog, bird, &c.

What was here merely indicated by the author of this report, has been fully demonstrated and traced out by Bischoff and, though with erroneous interpretations, by Barry also, in the mammifera. It is in the invertebrata, however, especially viviparous entozoa, that the process has been most successfully traced, as will be seen below.

§ 98. Before considering this remarkable process, which, according to Bischoff, commences about the time the ovum enters the second half of the tube, according to Barry, between his "fifth and sixth stage" of development, the ovum being then about the middle of the tube, it will be proper to premise an account of previous changes spoken of by Barry and Bischoff in the first half of the tube.

§ 99. In his second series Barry states, that in an ovum in the "third stage" of development, found at the distance of one inch from the infundibulum, the germinal vesicle was visible in the ellipsoidal mass that occupied the centre of the fluid yelk. This latter was obscurely granulous. In an ovum of the "fourth stage," also about an inch from the infundibulum in the Fallopian tube, the yelk was also obscurely granulous, but the germinal vesicle was no longer seen. In an ovum at the "sixth stage," in which the alleged vitellary membrane has disappeared, there no longer existed a granulous yelk.* In the ovum delineated (fig. 106), the thick transparent membrane was filled with a transparent and colourless fluid, which, Dr. Barry says, it may be proper to designate the yelk.

§ 100. In his "third series," it has been already seen, Barry states among the "changes in the ovarian ovum preparatory to fecundation," that the germinal vesicle becomes filled with cells, and these again become filled with the foundations of other cells, so that the vesicle is thus rendered almost opaque, enlarged and flattened. This development of cells within the germinal vesicle proceeds from the germinal spot. In the centre of this spot there is invariably present at a certain period a dark point which enlarges, and is then found to contain a cavity filled with pellucid fluid. This central portion of the altered germinal spot, with its pellucid cavity, remains at that part of the germinal vesicle which is directed towards the surface of the ovum, and towards the surface of the ovary. At the corresponding part, the thick transparent membrane of the ovum, it has been already seen, Dr. Barry alleges, appears in some instances to have become attenuated, in others also cleft. Subsequently, the central portion of the altered spot passes to the centre of the germinal vesicle; the germinal vesicle regaining its spherical form, returns to the centre of the ovum, and a fissure in the thick transparent membrane is no longer seen. From these successive changes, it has been seen Dr. Barry infers that fecundation has taken place, and this by the introduction of some substance into the germinal vesicle from the exterior of the ovary. It may, he says, also be inferred that the central portion of the altered germinal spot is the point of fecundation. In farther proof that such is really the case, Dr. Barry affirms that there arise at this part two cells, which constitute the foundation of the new being,—that is the germ. The nucleus in each of these twin cells which together constitute the germ, undergoes essentially the same changes as those presented by the germinal spot, but seems to pass sooner than the centre of the altered germinal spot to the interior of its cell. These two cells which constitute the germ enlarge, and imbibe the fluid of those around them, which are at first pushed farther out by the two central cells, and subsequently disappear by liquefaction. The contents of the germinal vesicle thus enter into the formation of two cells. The membrane of the germinal vesicle then disappears by lique-

* Barry now questions the analogy which this term implies. He calls it, as above seen, "the substance surrounding the germinal vesicle."

faction. While the changes just described are in progress within the germinal vesicle, membranes form and then disappear successively around the layer of discs or cells by which this vesicle is surrounded (i. e. the yelk). Very soon, however, the substance they invest (i. e. the yelk), has wholly disappeared, and its place is occupied by colourless transparent fluid. (p. 538, § 380.) In this fluid are often found solitary cells, the remains of the substance just referred to. It is also very common to meet with these solitary cells in previous conditions of the ovum before the last of the membranes (alleged vitellary) has disappeared.

§ 101. In the first part of the Fallopian tube, according to Bischoff, the yelk has still quite the same appearance as in the ovary, uniform, finely granulous, and entirely filling the zona. A little further on, however, it no longer fills the zona, but is observed to be contracted, leaving between it and the zona a space in which there is a transparent fluid, in which for the most part two granules, often different in size, are seen swimming. Bischoff sought in vain for the germinal vesicle; neither under the compressorium, nor by opening and dissecting the ovum with a fine needle, could he observe anything like it. Sometimes only he thought he saw in the interior of the yelk a speck clearer but smaller than the germinal vesicle, but he never could come to any decisive conclusion regarding it. In these dissections of the ovum, however, he remarked that the yelk had evidently increased in coherence. By the action of water on the ova, the yelk which previously was contracted, became distended and again filled the zona. Denser fluids produced an opposite effect, the yelk became considerably contracted.

§ 102 Collating the results of his own and Barry's examinations of ova found in the first half of the Fallopian tube, Bischoff makes the following observations (p. 54:) "I cannot agree with Barry that the yelk, or as he calls it, the 'substance by which the germinal vesicle is surrounded,' of the fecundated ovum in the Fallopian tube, is composed of cells of any kind such as he has delineated in his third series, figs. 185, 188, 189, 190, 193, 194, 195, 199, 200, &c. It will I hope be allowed, that such an appearance is too remarkable to have escaped me with the use of good instruments. Moreover I can as little admit that, as Barry affirms, the substance in question which is considered by me yelk, gradually disappears by the solution of the cells forming it. The mass which in the preceding observations fills the interior of the ovum, corresponds in its appearance and condition so completely with that filling the unfecundated ovum, that it is impossible not to consider both as identical. The only difference by which, as I think, even Barry was led to his conclusion, is, that this mass no longer fills the whole interior of the zona, but is often considerably shrunk. How much soever this state is to be attended to, seeing that it probably stands in close connexion with after-changes, it gives little warrant for the supposition that a partial solution of this mass occurs. The increasing consistence of the yelk, and the reactions it exhibits with fluids, prove that the diminution of its size is owing merely to a closer aggregation of its particles, occasioned probably by the action of the fluids secreted by the Fallopian tubes.* The addition of water, as above mentioned, generally soon brings the yelk back to its former size." Bischoff considers as altogether erroneous the supposition of Barry, that the two granules or vesicles, which are often seen at this stage of development in the space within the vitellary membrane (zona) left by the contraction of the yelk, are a remains of the dissolved yelk-mass. He is rather inclined to view them as of great importance for the farther preparatory change of the yelk-mass. This view of Bischoff will be stated below. Farther Bischoff particularly observes, that it is impossible for him to coincide with Barry in his statements regarding the germinal vesicle. With the greatest care and attention, and in his last observations when he had become acquainted with Barry's paper and figures, he never could observe in the rabbit's ova from the tubes a germinal vesicle at all, and still less one enlarged and filled with cells. Even Barry's own delineations, figs. 187 and 193 *c*, Bischoff says are so uncertain and indistinct, that he is astonished at his assertions. Bischoff has indeed several times remarked in the yelk a bright spot such as Barry mentions, on the existence

* The seminal fluid?

of which he says undoubtedly great weight is to be laid, but he does not believe it to be produced by the unaltered germinal vesicle of the fecundated ovum.

§ 103. *The process of division and subdivision of the yelk* now claims consideration. In his communication of 1838 Bischoff observes, "I have occasionally observed such remarkable forms that I asked myself the question, whether the vitellus of the mammiferous animal's ovum underwent changes of form similar to those of the ovum of fishes and batrachia?" (Willis's Wagner, p. 140.) At page 152 he conjectures that these alterations in the form of the yelk are due to this: that all the yelk-granules are at first inclosed in two, then in four, then in eight cells, and so on until each at length comes to have its own cell, which coalescing with one another, the blastoderma is produced as a preliminary to the appearance of the embryo.

§ 104. Dr. Barry's observations in his 2d series, 1839, are as follows: In the centre of the fluid, which, as above seen, Barry in his 2d series designates yelk, "there arise (this quotation is from the summary, p. 351) several (at first two vesicles, then four or more, and so on) very large and exceedingly transparent vesicles. These disappear and are succeeded by a smaller and more numerous set. Several sets thus successively come into view, the vesicles of each succeeding set being more numerous and smaller than the last, until a mulberry-like structure has been produced, which occupies the centre of the ovum. Each of the vesicles of which the surface of the mulberry-like structure is composed, contains a colourless and pellucid nucleus; and each nucleus presents a nucleolus. This mulberry-like body is found to contain a cell larger than the rest, elliptical in form, and having in its centre a thick-walled hollow sphere, which is the nucleus of this cell. This nucleus is the rudimental embryo." In § 177 of his paper itself, in § 307 of the first postscript, and in § 318 of the second postscript, Dr. Barry remarks on the resemblance between the process of division and subdivision he had just described in the mammiferous ovum, and the divisions and subdivisions first noticed by Prevost and Dumas in the yelk of the ovum of the frog, and now known to occur in the ova of various other animals.

§ 105. In his first P.S. § 307, Barry remarks that the process of subdivision into smaller and more numerous vesicles suggests the idea, originally that of Schwann, that in the interior of each vesicle there arise two or more infant vesicles, the parent vesicle in each instance disappearing by liquefaction. In his second postscript, headed "Process by which the primitive ovum undergoes division and subdivision—Probable analogy in this respect between the mammiferous ovum and the ovum of batrachian reptiles and certain fishes—The so-called yelk-ball in the mammiferous ovum compared with the 'Discus vitellinus' in the ova of other animals—Resemblance between the fecundated ovum of mammalia and that of certain plants"—Barry says that in ova which he had since examined he had found, what he suggests as likely in his first postscript to be really the case, viz., "that in the interior of each vesicle there arise two or more infant vesicles, the parent vesicle in each instance disappearing by liquefaction."

§ 106. In his third series Dr. Barry says that each of the twin cells, which according to him as above seen succeed the germinal vesicle, presents a nucleus; which having, like the germinal spot in reference to its vesicle, first passed to the centre of its cell, resolves itself into cells in the manner above described. By this means the twin cells in their turn become filled with other cells, but only two of these in each cell is destined to continue, the others as well as the membrane of each parent-cell disappear by liquefaction. There are thus now four cells. Each of these four, in its turn a parent-cell, gives origin to two; by which the whole number is increased to eight. This mode of augmentation continues until the germ consists of a mulberry-like object, the cells of which are so numerous as not to admit of being counted. Together with a doubling of the number of the cells, there occurs a diminution of their size.

§ 107. These facts, Dr. Barry says, do not confirm Bischoff's conjecture referred to in his (Dr. Barry's) second series, viz., "that in the first place all the yelk granules are inclosed in two, then in four, then in eight, &c. cells." (Willis's translation of Wagner, p. 153.) But they strengthen, he thinks, the analogy pointed out in that memoir between the early changes in the ovum of the mam-

mifera and the divisions previously known to occur in the ovum of batrachian reptiles and some other animals; and he says it is almost superfluous to add, that he supposes the process in all to be essentially the same, and that it is in operation in other animals. Barry says that he believes it has hitherto been usual to regard the round white spot or cicatricula in the yelk of the bird's laid egg as an altered state of the discus vitellinus in the unfecundated ovarian ovum. So far from thinking that such is the case, Barry ventures to believe that the whole substance of the cicatricula in the laid egg of the bird has its origin within the germinal vesicle in the same manner as in the ovum of the mammalia, and therefore that the cicatricula in the bird's laid egg may properly be called the germ.

§ 108. Bischoff first observed the process of division and subdivision of the yelk in the ovum of the dog, but he finds that it can be more easily traced in the rabbit's ovum in consequence of its less density.

§ 109. Regarding Barry's account of the process, Bischoff remarks, "it will be admitted by every unprejudiced person that Barry did not in his second series express himself clearly on the subject; and has in his third series, as I am convinced, interpreted it quite erroneously. As above seen, Barry alleges that the yelk becomes entirely dissolved, and that the whole phenomenon, hitherto considered as a division and subdivision of the yelk, consists merely in a progressive development of cells from the germinal vesicle; from which, first two, then from these four, and from these eight, and so on, are alleged to be developed, and that not in a simple but in a very complicated manner.

"I have above taken from these allegations their grounds of support, by disproving the solution of the yelk and the persistence and metamorphosis of the germinal vesicle, before the process in question begins. I think it is straining things very far not to recognize in the globules filling the interior of the ovum during the period in question, exactly the same elements as those of which the contents of the vitellary membrane (zona) consist, as well before as immediately after fecundation." In the analogous phenomenon in other animals, Bischoff goes on to say that "it cannot for a moment be admitted that the yelk dissolves and that a new substance proceeding from the germinal vesicle produces those globules. Athough there be some differences in the mode of division and subdivision of the yelk in different animals still the process itself is in all essentially the same, and consists in a progressive resolution of the yelk into smaller and smaller globular segments. No other observer has attributed any direct part in the process to the germinal vesicle, but all agree that this itself has already disappeared when the subdivision commences."

§ 110. Careful examination of the globular masses of yelk themselves also contradicts Barry's view of their condition and mode of formation. There is, indeed, to be observed inclosed in each of them a particular central mass, a clear speck, a nucleus. But this does not at all resemble those forms described and so distinctly delineated by Barry. Bischoff has never been able to perceive in any of those globular masses of yelk the bright shining central point and the progeny of cells proceeding from it, as alleged by Barry. Lastly, he has never been able to discover, notwithstanding the greatest attention, any trace of that large elliptical cell and its nucleus with transparent centre and granulous periphery, which Barry says he has observed in ova at the end of the tube among the other globular masses of yelk, and holds to be the first trace of the embryo.

§ 111. Bergmann, Reichert, Vogt, Bagge, and more recently Kölliker, have occupied themselves with this remarkable process of division and subdivision of the yelk, but not in the mammifera. From the similarity of the process in almost all animals, however, their researches may be very advantageously applied to assist the inquiry into the nature of the process in the mammifera. They will be found to give a probable explanation of the observations on which Dr. Barry has founded his inaccurate and disjointed views and delineations.

§ 112. From his observations on the ova of the frog, Bergmann (Müller's Archiv, 1841) considered the first stage of the process as a mere division of the yelk, and that the segments are not inclosed in a particular investment or cell-membrane; and that therefore they cannot be called cells. In the later stages,

however, he satisfied himself that the segments are cells, i. e. they possess a very delicate investing membrane, by which the yelk-granules are held together. He then remarked also in each of these cells a clear round spot, which became distinct on slight pressure of the cell. From this he is of opinion that the cell-membrane is formed around a previously-existing globular mass, which then presents itself as cell-contents. As to the clear spots observable in these cells, Bergmann is doubtful whether or not they are to be viewed as nuclei. On the whole Bergmann considers the subdivision of the yelk as leading to the formation of cells.

§ 113. The result of Reichert's researches (Müller's Archiv, 1841) on the point is, that all the parts or globular masses which come into view during the process of subdivision, nay, even the whole as yet undivided yelk-ball as well as the elements which afterwards come together for the formation of the embryo, are surrounded by a proper fine investment, and are therefore cells. All these cells are from the very commencement of the process of division inclosed within one another, the smaller which appear later within the larger and lastly the two which first appear within the cell, forming the whole yelk. The process of division consists only in this, that the inclosed pre-existing cells become free when the inclosing mother-cells dissolve. In this manner the first two yelk-cells appear, when the mother-cell inclosing them and investing the whole yelk dissolves. When the two cells, thus become free, dissolve in their turn, the four next contained within them become free, and so on.

§ 114. According to Vogt* the process of division of the yelk in Alytes obstetricans is not only confined to one half of the ovum, which is also the case in some other animals, but he alleges that there is no division of the yelk-mass through and through, that the division affects the surface only, and is at last brought about internally by a folding in of the vitellary membrane. This process of grooving he alleges stands in no direct relation to the formation of the cells which afterwards appear and serve for the development of the embryo, seeing that this formation of cells begins only when the grooving process is finished and the yelk has again become smooth.

The multiple germinal spots of the germinal vesicle, Vogt considers to be cells, which are inclosed within the germinal vesicle as within a mother-cell. Vogt, like all preceding observers, satisfied himself that the germinal vesicle, which he had shortly before readily found and observed, has always disappeared after the egg is laid. Now, however, he succeeded in again finding the germinal spot-cells dispersed in the cortical layer of the yelk, which, it would appear, had become free by the solution of the germinal-vesicle cell. These germinal-spot cells he afterwards observed in the cells arising and arisen after the grooving process, in which they appear to play the part of a nucleus; and although he does not on that account connect the grooving of the yelk with this formation of cells, he yet thinks he has proved that the latter takes place in consequence of the germinal-spot cells, each becoming surrounded by a group of yelk-granules, and these by a cell-membrane, so that cells are formed around cells. Vogt, however, at the same time admits that, in addition to the process just mentioned, cells form in the centre of the yelk, and this without the concurrence of these germinal-spot cells, or any newly-formed ones similar to them, by individual masses of yelk-granules becoming directly surrounded by cell-membranes without the concurrence of a nucleus.

§ 115. Bagge's† researches were made on the ova of Strongylus auricularis, and Ascaris acuminata. In the ova of these two viviparous entozoa the process of division of the yelk takes place, and here also ends in the formation of the cells which immediately compose the body of the embryo. In the ova of these animals also the germinal vesicle disappears after fecundation. But then there appears in the middle of the as yet undivided yelk a small clear cell. This becomes somewhat elongated, contracts in the middle, and assumes a fiddle-shape.

* Untersuchungen über die Entwickelungsgeschichte der Geburtshülfer Kröte. — Solothurn, 1841.

† Diss. de evolutione Strongyli auricularis et Ascaridis acuminatæ.—Erlangæ, 1841.

At last it is divided in the middle, and there arise out of it two vesicles which withdraw themselves towards the two poles of the somewhat oval yelk. The division of the latter now begins, so that each half incloses one of the two vesicles. As soon as this has taken place, the same process is repeated with the vesicle in each half. These also divide, and the repeated division of the yelk follows this division, and each part of the yelk thus always contains a small vesicle inclosed within it.

§ 116. In regard to the nature of the process in the rabbit, Bischoff expresses himself to the following effect (p. 75) :—"I have already above expressed my conviction that the germinal vesicle collapses or dissolves when the ovum leaves the ovary, although the period at which this takes place does not appear to be quite definite; nay, I even hold it for possible, that the germinal vesicle itself sometimes does not disappear until the ovum is in the tube: I have however never seen it in an ovum in the tube. Instead of it, I then sometimes saw in the interior of the yelk, a clear somewhat smaller spot than is formed by the germinal vesicle. Hitherto, however, I have in vain endeavoured to obtain a more distinct view of this clear spot, and that in a separate state; still I have satisfied myself that it is not the germinal vesicle itself. In this stage there appear the two granules or vesicles above mentioned at the surface of the yelk, to which I cannot refrain from ascribing some definite and important use, as they are found also in the ovum of the dog at this stage. Hereupon the division of the yelk commences, and each successive segment has in its interior a similar clear spot like that which the whole yelk-ball shortly before appeared to contain. It is observable only under particularly favorable circumstances, and even when I succeeded in getting the best view of it, it was impossible for me to come to any certain conclusion regarding its nature. It is distinguishable by its bright shining transparent appearance, and its boundary is determined more by the yelk-granules surrounding it, than by any proper membrane. I could never recognize in these clear central parts of the globular segments of the yelk, either a cell-membrane or a nucleus, but the whole appearance corresponded most with that of an oil-globule, around which the yelk-granules had been collected. In this respect these clear points agree entirely with those which are also observed in the globular segments of the yelk of the frog's ovum, and which, as we above saw, Vogt considers as the germinal spots of the germinal vesicle and to be themselves cells; Bergmann is doubtful whether they should be called nuclei or cells; and Reichert does call them nuclei. Were I called on to choose from among these designations, I would give the preference to the last, because in fact cell-nuclei occur in other places, which are extremely clear and transparent, and because I believe that they are actually products or descendents of a cell-nucleus, viz. the germinal spot. I would however avoid using the term nucleus for those clear central parts of the globular segments of yelk, because it is usual forthwith to associate it with a cell, which I cannot recognize the globular mass of yelk to be.

§ 117. "I will therefore rather describe than name, and suspend any opinion as to the nature of the parts in question, until farther observations give greater certainty. I believe that the clear spots are descendants of the germinal spot of the germinal vesicle. When the germinal vesicle has dissolved, the germinal spot appears, probably in consequence of the action of the male semen, to become enlarged and converted into a clear body like an oil-globule, to become in fact more like a vesicle. Whether the germinal spot so changed again passes from the periphery of the yelk, where it must be after the solution of the germinal vesicle, to the centre, here to divide, and the two parts again return to the surface, or what is more simple and probable, the germinal spot remaining at the surface of the yelk divides into two parts, I cannot say, because my observations are not sufficiently complete, and because both opinions are possible."

§ 118. However that may be, Bischoff considers the two granules or vesicles above referred to, as being observed swimming in the fluid filling the space between the zona and the shrunk yelk before the process of division and subdivision commences, as the two parts and descendants of the germinal spot, around which

the yelk-grains collect in two groups, by which the first division of the whole mass is produced. In the dog especially he thought he perceived that those two granules at the surface of the yelk-mass consisted of such a clear corpuscle, as are remarked in the subsequently formed globular masses of yelk which were beset with a single layer of yelk-granules. By the continued accumulation of yelk-granules around them, the previously single yelk-mass becomes divided into two parts. Then there probably takes place in each of the clear central corpuscles, a new division, followed by a new grouping of the yelk-grains around, so that now there are in the whole four parts, from these four arise eight in the same way, and so on.

§ 119. For the reasons he adduces, Bischoff cannot, as he formerly did, consider as *cells* those globular segments arising from the division and subdivision of the yelk of the rabbit's ovum during its passage through the Fallopian tube, nor recognize in the whole process one of cell-development, (p. 79.) He considers it a process *sui generis*, which, as the result shows, appears to be a preparation for the development of true cells. Conglomerations of elementary granules, he continues, such as the globular masses of yelk here represent, are by no means uncommon. They are to be found in pus and plastic exudations, and here constitute Gluge's inflammation globules; in milk they form colostrum-corpuscles.

§ 120. Kölliker* has carefully followed the first processes in the fecundated ovum of several invertebrata. He finds (p. 84) that the germinal vesicle and its spot disappear after fecundation, but that in the centre of the yelk there is developed a nucleated cell which he calls *embryo-cell.* Within this cell are formed two young ones, which become free by the solution of their mother-cell. Each of these two cells become surrounded by one half of the yelk-mass. This constitutes the commencement of the division of the yelk. Within each of the two cells two younger ones are developed, which on becoming free by solution of their parent-cell, become surrounded by a subdivision of the yelk-mass. And so on the process goes, the cells of each succeeding brood drawing around them the yelk substance, until this is all consumed in the process, and there remains in its stead the mulberry-like body consisting of an agglomeration of cells, the descendants of the first embryo-cell. From this it appears that Kölliker agrees with Bagge, that the embryo-cell formation always precedes the process of division, and that the latter is in some manner caused by the former. He does not however agree with Bagge that the cells multiply by fissiparous generation, but believes that they increase by endogenous generation as above mentioned, and by the nucleus, first discovered by himself, of each of the successive embryo-cells, as in its turn it becomes parent-cell, dividing into two parts, around each of which a cell-membrane forms.

§ 121. The process of division of the yelk does not take place in Ascaris dentata, Oxyuris ambigua, Cucullanus elegans, Bothryocephalus and Distoma, and is therefore not to be considered essential. The development of embryo-cells, however, appears to occur in all, and is consequently to be considered the essential part of the process. Kölliker is the discoverer of this development of embryo-cells without division of the yelk. Partial division had been previously observed by Vogt in Alytes obstetricans. If, as above hinted at as probable, a division takes place in the bird's egg, it must be still more partial.

§ 122. From his observation of the first processes in the ovum of the invertebrata after fecundation, Kölliker is disposed to interpret the appearances presented by the rabbit's ovum as described by Bischoff, in the following manner: After fecundation the germinal vesicle and spot disappear. In the centre of the yelk, the first embryo-cell makes its appearance, as the clear speck described by Bischoff as of smaller size than the germinal vesicle, and which he considered might be the germinal spot enlarged. Around this first embryo-cell, the loose yelk closely contracts. Within this embryo-cell two new ones arise from its divided nucleus—for Kölliker (p. 119) thinks a nucleus exists, though not observed by Bischoff on account of the difficult nature of the investigation—and become free by solution of the mother cell. When this takes place, these two new cells become

* Beiträge zur Entwickelungsgeschichte wirbelloser Thiere. In Müller's Archiv, Nos. i. and ii. for 1843.

each surrounded by a mass of yelk. The previously single yelk-ball is thus divided into two. And so proceeds the progressive formation of embryo-cells by endogenous generation, attended by the division and subdivision of the yelk into segments to inclose the new embryo-cells which have become free, until the process is completed.

§ 123. Kölliker does not agree with Bischoff in the opinion that the two bodies observed swimming in the fluid which accumulates in the space between the vitellary membrane, and the shrunk yelk are the nuclei, around which, in the process of division of the yelk, the two first segments are collected, but he thinks they are simply the remains of the germinal spot dissolving.

§ 124. Speaking of the results of Barry's researches on this subject, Bischoff (p. 68, 71) says he is sorry to have to consider them as the effect of fancy and enthusiasm for the cell-theory; and Kölliker does not hesitate to apply to them the epithet *fabulous*. Dr. Barry, however, must surely have seen some of the realities at least, that are appreciable, though he has misinterpreted them in so extraordinary a manner. Supposing the yelk to have become fluid, he appears to have mistaken the real yelk-ball for the germinal vesicle enlarged and rendered opaque by being filled with cells. Then finding the two cells derived from the first embryo-cell, he has put them down as those which alone, out of the great number he supposed to be formed within the germinal vesicle, come to perfection. The yelk-granules he appears to have mistaken for those which he supposes do not come to perfection. And so on runs his mistake, which it will be perceived is traceable to his conceit regarding the fate of the germinal vesicle.

§ 125. The following table is given by Kölliker, to show the relation of the yelk to the development of the embryo-cells in different animals:

I. *Embryo-cells developed free in the Yelk.* (No subdivision of the Yelk.)		II. *Embryo-cells surrounded by the Yelk.* (Subdivision of the Yelk.)	
1. The first embryo-cells are small, and assimilate the yelk slowly.	2. The first embryo-cells are large and immediately take up the whole yelk.	1. The first embryo-cells surround themselves with a part of the yelk only. (*Partial subdivision.*)	2. The first embryo-cells surround themselves with the whole yelk. (*Complete subdivision*)
a. Yelk granulous. Bothryocephalus. Tænia. Distoma tereticolle.		*a. The last globular masses of yelk become cells.* (?) Coregonus palaea. Alytes obstetricans.	*a. The last globular masses of yelk become cells.* (?) Rana. Triton. Lepus cuniculus. Canis familiaris.
b. Yelk fluid. Ascaris dentata. Oxyuris ambigua.	*b. Yelk fluid.* Cucullanus elegans.	*b. The last globular masses of yelk never become cells.* (?) Sepia vulgaris. Loligo sagittata.	*b. The last globular masses of yelk never become cells.* (?) Ascarides permultæ. Strongyli nonnulli. Nereis. Botryllus. Ascidiæ compositæ Pycnogonum. Acolidiæ.

§ 126. In four ova found in the middle of the Fallopian tube of a rabbit, around which the gelatinous-looking investment was still very thin, in which the division and subdivision of the yelk had not commenced, but in which the yelk-ball was a coherent round mass, not filling the interior of the vitellary membrane (zona), but suspended in a transparent fluid, which, (together with two small yellow shining grains or cells in three of the ova), constituted the rest of its contents,

Bischoff observed a very remarkable phenomenon, viz., a rotation of the yelk-ball, produced by cilia on its surface, within the zona. The direction of the rotation was from the uterus towards the ovary. The motion was continuous, but after adding some aqueous humour to prevent desiccation, it ceased.

§ 127. Bischoff has not hitherto made a second observation of the same kind. He has observed no such appearance in the ovum of the dog, though examined under circumstances when it might have been expected. In ova of the rabbit farther developed, he has not observed the rotatory motion. His examination of ova at earlier periods were made before he was aware of the phenomenon. He expresses himself however quite convinced of the correctness of the observation above mentioned, and that there is the less reason to suppose there could have been any error, as similar rotatory phenomena are known to occur in the ova of other animals. It is to be remembered, however, that as Reichert observes, the rotation of the yelk in the ova of other animals does not occur until *after the process of division and subdivision*, by which the yelk is resolved into cells. It is with the cells on the surface that the vibratile cilia are connected.

§ 128. Dr. Barry mentions in his second series having observed rotatory motions of a mulberry-like object in vesicles under the mucous membrane of the uterus, but this can have nothing in common with Bischoff's observation. Barry is as decided in opinion that the object he examined *was not*, as Bischoff is that the object he examined *was*, an ovum.

§ 129. *Correlations of the changes in the ovum above examined.* Dr. Barry in his 2d series describes eight different stages of development as being presented by the ova of the rabbit in their passage along the Fallopian tube. All this, and his numerous subsequent stages in the uterus, Bischoff says are quite unnecessary, and it is to be observed that Dr. Barry himself in his 3d series, speaks of his subdivision into stages, as having been adopted for a "temporary purpose."

§ 130. The ova on entering the Fallopian tube are still surrounded by the cells of the "discus," and *membrana granulosa*, but these cells, according to Bischoff, have lost the fusiform shape they had assumed before leaving the ovary, and again become round; and it is very soon remarked of them that they are in process of solution, whereby, says Bischoff, their sharp outline is vanished, and they appear again to be fused together.

§ 131. Somewhat further on in the tube, Bischoff says, these cells have almost entirely disappeared; some remains of them only are observed on the vitellary membrane (zona), and at last these also disappear entirely, so that the ovum now appears naked, and the vitellary membrane (zona), is seen to be immediately beset with spermatozoa, but which Bischoff never saw still in activity. At this stage the yelk has the same appearance as in the mature ovarian ovum, but it soon shrinks and no longer fills the cavity of the zona. Bischoff never found a germinal vesicle in such ova.

§ 132. Barry observed the first appearance of the gelatinous-looking investment around ova, found at about an inch from the infundibulum in the Fallopian tube, and describes it as being already very evident in ova from the middle of the tube. Bischoff appears not to have observed the appearance of the investment in question, until the ova were in the middle of the tube; at the same time that he saw the rotatory movements of the yelk.

§ 133. On entering the second half of the Fallopian tubes, the process of division and subdivision of the yelk commences and goes on together with the thickening of the gelatinous-looking investment as it passes on towards the uterus.

§ 134. The process of resolution of the yelk into smaller and smaller globular masses going on and the ovum surrounded by a thick gelatinous-looking investment, it passes from the tube into the uterus. This occurs at the end of the third or beginning of the fourth day *post coitum.* So that the period of the passage of the ova in the rabbit through the Fallopian tubes falls between the eleventh and seventy-seventh hours.

§ 135. It appears that the ova pass through the first part of the tube more

quickly, more slowly along the latter part. As to the means by which the ovum is carried along: the vibratile cilia of the epithelium of the Fallopian tube appear to be one means, and it is to be remarked, in connexion with the more rapid passage of the ova through the first part of the tube, that here the cilia are most active. But the principal means, Bischoff thinks, are the contractions of the Fallopian tubes themselves; he has seen such contractions very evidently. It is to be remarked that the contractions are in a direction opposite to that by which the conveyance of the seminal fluid towards the ovaries was stated to be effected; but this difference in the direction of the contractions of the same part, Bischoff remarks, is not singular; contractions in opposite directions take place naturally, e. g. in the gullet of ruminants.

§ 136. It has been already seen that in the dog, Bischoff says, no albuminous investment is added to the ovum in its passage through the tube. Some remains of the granules or cells of the discus which surround the ovum when it enters the tube, and which have again become round, though almost dissolved and vanished, continue attached to the ovum when it enters the uterus, and indeed appear to have become united with the external surface of the zona, which therefore presents an uneven and slightly granular appearance. Might not this be looked upon as a trace analogous to the albuminous layer?

§ 137. The yelk undergoes the same process of subdivision as above described in the rabbit.

§ 138. The ova in the dog are received into the Fallopian tubes at a later date *post coitum* than in the rabbit, and they pass along the tubes more slowly, so that they are not found in the uterus until about the eighth day, though this is said to be different in different individuals.

§ 139. Of the human ovum in, and its passage through the Fallopian tube nothing is known.

IV. The development of the ovum in the uterus until the first appearance of the embryo.

§ 140. Of the two vesicles of which Baer found the ovum of the dog in the uterus to consist, he considered the outer the same as the outer membrane of the ovarian ovum, and now called it *membrana corticalis or chorion*, as he believed he had observed the development of villi upon it. The inner vesicle he considered as resulting from the fusion of the yelk-grains during development, and called it first *membrana vitelli;* and a dark spot observed on one side of it he first considered as the analogue of the *blastoderma* of the bird's egg. (Epistola, pp. 10 and 23.) He afterwards changed his opinion in so far that he considered the whole inner vesicle as blastoderma, which from the very commencement presents itself in the form of a vesicle. (Heusinger's Zeitschrift, ii. p. 174; Entwickelungsgeschichte, ii. p. 184.) The dark spot in it which he saw to be at first round, afterwards elongated, he recognized as the place where the embryo is developed from a primitive streak in exactly the same way as the embryo of the bird. (Entwickelungs-geschichte, ii. p. 189.) He says further, though in a very brief and unsatisfactory manner, that the blastoderma splits into two layers, an animal and a vegetative, as in the bird's egg, and grounds thereupon his whole account of the development of the embryo. (Entwickelungs-geschichte, ii. p. 192, r. u. 208-z.) But it does not appear certain whether this statement was the result of direct observation or only a conclusion drawn from the analogy between the ovum of the bird and the mammal.

§ 141. Coste also saw and described the ovum, first as a transparent vesicle, consisting of two membranes, the one inclosed within the other. The inner membrane he at first supposed to be the germinal vesicle enlarged, but afterwards repudiated the opinion. In his 'Embryogenie' he considered the outer membrane of the uterine ovum as the vitellary membrane (zona) of the ovarian ovum unchanged except that it was increased in size. He therefore still called it in the

uterus vitellary membrane. The inner he considered as a product of development and called it blastoderma. At the seventh day he observed on the latter a spot, already seen by Cruikshank on the sixth, and called it "tache embryonnaire," because from it the development of the embryo proceeds. The blastoderma, he says, must be considered as composed of two principal layers; an external and an internal, and an accessory layer enveloping the latter. In the rabbit's ovum of the seventh day we may, he says, but not without much difficulty, succeed in demonstrating what will be afterwards still more evident, that the "tache embryonnaire" may be separated into two concentric layers, which may be traced into almost the whole extent of the blastoderma, which is consequently also formed of two layers. The "tache embryonnaire" is at first round, then elliptical, then guitar-shaped, soon after this the head and tail ends of the embryo may be recognized.

§ 142. It is easily seen, however, Bischoff observes, that there is an important hiatus between the stage of development, at which the ovum is at the end of the Fallopian tube and the condition in which it is here represented. Dr. Barry supplies observations referring to the period of this hiatus when the ova are from one fifth to one half of a Paris line in diameter.

§ 143. In the uterus, according to Dr. Barry, a layer of vesicles of the same kind as those constituting the mulberry-like object (the yelk in its last and most minute state of subdivision or rather, as above shown to be probable, the agglomeration of the last descendants of the first embryo-cell) makes its appearance, and, like an epithelium, lines the whole inner surface of the membrane, which now incloses the yelk—the thick transparent membrane or zona pellucida (vitellary membrane). The mulberry-like structure then passes from the *centre* of the yelk to a certain part of the layer just described (the vesicles of it coalescing with those of the mulberry-like structure, where the two sets are in contact, to form a membrane; Dr. Barry says, the so-called germinal membrane, or what has been denominated its "serous lamina" by authors on the ovum of the bird, but really, according to him, the future *amnion*). The interior of the mulberry-like structure is now seen to be occupied by a large vesicle containing a fluid and dark granules. In the centre of the fluid of this vesicle is a spherical body composed of a substance having a finely-granulous appearance, and containing a cavity filled with a colourless and pellucid fluid. This hollow spherical body seems to be the true germ. The vesicle containing it disappears, and in its place is seen an elliptical depression (*area pellucida* of authors on the bird) filled with a pellucid fluid. In the centre of this depression is the germ, still presenting the appearance of a hollow sphere. In regard to this, his account of the true germ in his 2d series, Dr. Barry remarks in a note that perhaps it would be more correct to consider the vesicle itself (which forms the interior of the mulberry-like structure) with the whole of its contents as the true germ. In his 3d series, Dr. Barry substitutes the term "rudimental embryo" for "germ." This latter name he now applies to the "twin-cells, foundation of the new being, group of cells, mulberry-like object," which, as has been seen, he alleges is formed out of the germinal vesicle. Thus, according to Dr. Barry, the rudimental embryo is the nucleus of a cell. It passes from a spherical into a linear form; in some stage of which latter state it appears to correspond to the "primitive trace" of authors on the ovum of the bird. This alleged transformation of a nucleus into the embryo appears to Dr. Barry to be a process similar to what is in operation in other instances where the product does not extend beyond the interior of a minute and transitory cell. Making allowance, indeed, he says, for a difference in form and size, the description given of the mode of production of the one might be applied to the other.

§ 144. In the recapitulation of his 2d series Dr. Barry continues; the germ (i. e. the rudimental embryo of his 3d series) separates into a central and a peripheral portion, both of which at first appearing granulous, are subsequently found to consist of vesicles. The central portion of the germ (i. e. rudimental embryo) occupies the situation of the future brain, and soon presents a pointed process. This process becomes a hollow tube, exhibiting an enlargement at its caudal extremity, which indicates the situation of the future sinus rhomboidalis. Up to a

certain period new layers come into view in the interior of the central portion of the germ (i. e. rudimental embryo), parts previously seen being pushed farther out.

§ 145. From the region occupied by the germ there arises a hollow process, which by enlargement is made to line the inner surface of the ovum; that is to say, the inner surface of the membrane entering into the formation of the amnion, (which corresponds to the "serous lamina" of authors,) and the process now lining it represents an incipient state of the subsequently "vascular lamina" of the umbilical vesicle; a lamina continuous with the structure corresponding to the "area vasculosa" of authors on the evolution of the bird.

§ 146. There does not occur in the mammiferous ovum any such phenomenon as the splitting of a "germinal membrane," into the so-called "serous, vascular and mucous laminæ." Nor is there any structure entitled to be denominated the "germinal membrane;" for it is not a previously existing membrane which originates the germ (rudimental embryo); but it is the previously existing germ (rudimental embryo), which by means of a hollow process originates a structure having the appearance of a membrane.

§ 147. Bischoff affirms that in most of these statements Barry is in error, and in particular calls in question all that he says of the first traces of the germ and embryo, and the want of analogy with what is known of the bird's egg. Had Barry, Bischoff remarks, extended his observations to more advanced ova than he has done, he must necessarily have convinced himself of the erroneous interpretations he has given to many particulars otherwise correctly enough observed.

§ 148. Bischoff (p. 85) observes that at the stage under consideration the changes succeed each other so quickly, and the difficulty in finding and handling the ova are so great, that he should scarcely have had a sufficient series of observations in the number (70) which he examined, had he not hit upon the expedient of making use of the ova contained in the two horns of the uterus of one and the same animal for separate successive examinations. When he expected the ova in the uterus at a certain stage, he cut out from the living animal the one horn of the uterus after ligature of the mesometrium and uterus above and below, an operation performed in a few minutes without any loss of blood, and without any considerable suffering to the animal. He then examined the ova, and determined from their condition when to examine the other uterus, with or without killing the animal. As the ova are generally equally developed in the two sides of the uterus at the same period, he obtained in this manner a certainty regarding the succession of the appearances, which was not to have been obtained even by the sacrifice of a much greater number of animals.

§ 149. The ovum, high up in the uterus, has at first the same size and appearance as in the lower end of the tube, and is readily found on account of the glancing of its albuminous layer, although indeed with much more difficulty in the wide uterus than in the tube. In ova just taken out of the upper part of the uterus and examined without any addition, the yelk is seen to be no longer of a mulberry appearance, but uniform and finely granular on the surface, and quite filling the vitellary membrane (zona), so that it would be exactly similar to the yelk of an ovarian ovum, were it not much paler and more transparent. But if any fluid be applied it is observed for the most part, that after some time the yelk mass contracts, and by and by its former mulberry appearance again comes into view, quite distinctly and sharply defined.

§ 150. At the next stage, the ova are, on the whole, still very like the preceding. The albuminous layer is still present, likewise the vitellary membrane (zona), and the size of the ova is nearly the same. But instead of the uniform appearance of the interior of the ovum, there are now remarked, when the focus of the microscope is adapted to its surface, cells distinctly pentagonal or hexagonal, joined to each other so as to form a layer, which is applied against the inner surface of the zona. The cells are filled with pale, finely-granular contents, and possess a very clear nucleus, not visible in the recent state. When the focus of the microscope is adapted to a deeper part of the object, it is observed that the ovum now represents in its interior a globular hollow filled with a clear fluid, and that the cells

composing the layer above mentioned, as now lining the vitellary membrane (zona), whilst they are pressed flat against it, project towards the interior in a round form. At some one place there lies a dark heap of globules which are quite similar to, and evidently identical with those resulting from the previous division of the yelk.

§ 151. In this condition the ova grow, whilst they proceed farther down in the uterus, pretty rapidly, but in an especial manner, the vitellary membrane (zona), and along with it the layer of cells, which is closely applied to it, becomes more distended. By this the albuminous layer is rendered thinner. At last it and the vitellary membrane (zona) coalesce entirely with each other, and now as one common, still more or less thick, layer, constitute the outer investment of the ovum. The layer of cells at the inner surface of this investment is very distinct; the individual pentagonal or hexagonal cells, with their nuclei which are pressed against each other, are easily distinguished; and they unite together more and more in the form of a membrane, and begin to form a very delicate internal vesicle, which lies close to the external investment, but which, after some time and after contact with a fluid, begins to separate from it. At one place the dark accumulation of yelk globular masses is still seen lying, but it diminishes more and more.

§ 152. Barry has delineated ova of this kind in his second series, figs. 111, 112, 113, 114, 115, 116, and in his third series, fig. 234. He has also correctly recognized the membraniform layer of cells at the inner surface of the zona, and the heap of globular masses of yelk. He calls the membraniform layer, *amnion*; which, however, Bischoff observes cannot yet be spoken of as existing, although this membrane composed of cells at a later period contributes a part for the formation of the amnion. Barry says nothing farther of the heap of yelk-globules; but he asserts that in it he has still seen that large elliptical vesicle with that clear centre (true germ of his 2d series, rudimental embryo of his 3d series) from which he alleges the embryo is produced, but of which Bischoff as well as several other observers to whom he showed such ova, could never observe the slightest trace.

§ 153. The question now arises, how the condition of the ova here described is evolved from that in which the whole vitellary membrane (zona) was seen equally filled by the globular masses of yelk pressed closely together, and that uniform appearance of the ovum thereby produced? Bischoff considers the following to be the process (p. 89.)

§ 154. When the ova have arrived at the above-mentioned stage, the globular masses of yelk begin to be changed into cells, by becoming surrounded by a fine investment, a cell-membrane. The clear spot previously remarked in each globular mass of yelk and which, as above seen, Bischoff thinks is a descendant of the germinal vesicle, becomes the nucleus of such a cell; but the rest of the globular mass, which has evidently thinned by the absorption of fluid by which it had become clearer, becomes the fine granulous contents of this cell, in regard to which it cannot but be seen that it is identical with the elements of the yelk of the ovarian ovum.

§ 155. From the observations of Kölliker above noticed, however, it would rather appear that the cells, which at last replace the minute globular masses of yelk, are the last descendants of the embryo-cell which have taken up the last remains of the yelk granules.

§ 156. The globular masses of yelk are not all at the same time replaced by cells, but gradually, and, as it appears, those first which are in immediate contact with the inner surface of the vitellary membrane (zona). The rest form the heap which is still for some time to be seen, but by and by as the ovum grows become expended in the formation of cells, which at last at the end of this stage line the whole inner surface of the ovum as a membraniform layer. There arises in this manner a second interior vesicle in the ovum, towards the formation of which all the processes hitherto described tended; and it is this vesicle which is thus the first product of development on which the materials of the ovum were expended, and for which they were sufficient. This vesicle forms the foundation for

the development of the embryo, and Bischoff therefore calls it *vesicula blastodermica*.

§ 157. This process of formation of the blastoderma from the yelk is somewhat similar to that conjectured by the author of this report, but of course irrespective of the cell-theory which had not then been broached, to take place in the bird's egg, viz. that the central matter of the bird's egg serves for the extension of the blastoderma. According to this the yelk of the mammiferous ovum would represent the matter around the germinal vesicle and also that in the centre of the yelk of the bird's egg, (*Vitellus Conformationis* of Reichert.)

§ 158. The fourth and beginning of the fifth day, *post coitum*, is the period during which the above process goes on.

There now follows, in Bischoff's observations, a small gap. The last-described ova were one fifth of a line in diameter; the next observed were one half of a line. He thinks, from the condition immediately to be mentioned, that nothing important in this interval escaped him. Still he is sorry for it, because it is in this interval that Barry's wonderful figures as he calls them, 121-126, occur, which refer to ova two fifths, one fourth, and one third of a line respectively. In their distant resemblance to the area germinativa of the bird's egg, Bischoff says they will strike many. Of importance they cannot be, he continues, as nothing of them is afterwards to be found. But there is still perhaps something present which might give some explanation. As they stand, Bischoff declares the figures and their description to be quite unintelligible to him. The figures referred to, Dr. Barry states, represent the embryo and incipient amnion. In the ova represented there had certainly been "no fixed relation between their degree of development and their size, locality, or age."

§ 159. Ova of one half line and more in diameter are those which have often been seen and described by previous observers. They appear, when just taken out of the uterus, as simple transparent vesicles: they lie quite free in the uterus. When put into a fluid (not water, if it is wished to examine them further,) it is soon seen that they are composed of two vesicles hitherto lying closely applied one within the other, which now are probably loosened from each other by the entrance of the external fluid, and become more or less separated. Both vesicles appear to the naked eye almost equally transparent. But under the microscope it is readily seen that they have a different structure. The outer is quite homogeneous, pretty strong, but so thin that it does not, even under the microscope, present when folded a double contour as an indication of its thickness. When it falls together it forms sharply-defined folds, such as distinguish the capsule of the lens. The inner membrane, on the other hand, appears quite distinctly to be composed of primary cells, pressed into a polygonal form, the edges of which applied against each other are still to be distinctly recognized as lines. When just removed from the uterus, the nuclei of these cells are not distinctly seen; but they become more and more distinct, especially after the application of some reagent. The cell-contents have a pale, finely-granular appearance; and generally the molecules surround the nucleus in a circle. All this is recognized better when the outer vesicle is carefully torn open with two sharp-pointed needles under the simple microscope and the inner one allowed to come out. This inner vesicle then presents itself as extremely delicate, soft, and, after a short maceration, readily becoming diffluent. Though he subjected this second vesicle to the most careful examination, and that in not a small number of ova, Bischoff could discover nothing farther,—no trace of the future embryo, no darker spot, no larger vesicle, none of the wonderful appearances represented in the figures of Barry.

§ 160. From what has been said, it is evident that the outer of these vesicles is produced by the union of the vitellary membrane (zona) with the albuminous-like investment; and, as the ovum enlarges, becomes at last a fine membrane. Bischoff for the present calls this vesicle the *external membrane of the ovum*. The inner vesicle is evidently that the formation of which from the yelk has been above traced, and called by Bischoff *vesicula blastodermica*. It has grown and continues to do so by the increase of the cells forming it, but how this takes place Bischoff

has not been able to make out. It is possible, he says, that new cells are formed from new formative materials drawn in from the uterus.

§ 161. In the blastodermic vesicle (fifth day), an opaque spot becomes observable. The most careful examination did not enable Bischoff to ascertain more of it than that there is at the place an accumulation of cells and nuclei, by which a thickening of the blastodermic vesicle was here produced. Burdach and Baer have called this spot *cumulus germinativus:* Coste, and after him Wagner, *tache embryonnaire*. Bischoff calls it "area germinativa" (*Fruchthof*), as it is quite evident that it is in it the embryo is developed.

§ 162 Ova somewhat older resemble the preceding at first view, except that they are larger. They still lie quite free, are now easily found at different places of the uterus, and still appear as round transparent vesicles. But a closer examination showed Bischoff that an important step in development had been made in them. He found that at the *area germinativa* the blastodermic vesicle consisted of a *double layer;* a very thin stratum of delicate nucleated cells having here begun to form or to separate from it at its inner surface. This proves an important correspondence of the mammiferous ovum with that of birds, in which, as is known from Pander's researches, the germinal membrane soon after the commencement of incubation presents two layers, the upper called *serous* or *animal*, the under *mucous* or *vegetative*.

§ 163. These layers of the blastoderma in the mammiferous ovum have been denied most decidedly, as above seen, by Barry; but Bischoff expresses himself fully convinced of their existence and their importance. He therefore justly lays great weight on his observation, which is confirmatory of previous statements by Baer (part 2d of his Entwickelungs-geschichte, pp. 198 and 208,) and Coste (Embryogenie, pp. 113 and 460.) In the rabbit's ovum, then, from the time it has attained the size of from one and three quarters to two lines, two layers of the blastodermic vesicle can be demonstrated; which Bischoff therefore from this time calls *serous* or *animal* and *mucous* or *vegetative*. In the mammifera Bischoff has observed nothing like Reichert's "investing membrane;" and in the bird's egg he has not examined into the point.

§ 164. In ova about the sixth day, and about two lines in diameter, somewhat elliptical and still quite free and transparent, Bischoff observed with the naked eye, but better with a magnifying-glass, very small inequalities or processes on the surface of the external membrane of the ovum. More highly magnified under the microscope they appeared to be formed of a homogeneous transparent mass, in which numerous small molecules were imbedded. Neither cells nor nuclei were to be recognized in these processes, with or without the application of any reagent; at a later period, however, cells appear. As their progressive growth shows, these processes are nothing more than the so-called villi of the *chorion*. By this development of villi upon it, the external membrane of the ovum is proved to belong to the chorion; although, as will be afterwards shown, it does not yet, Bischoff thinks, alone represent that membrane.

§ 165. Barry states that he has seen the first trace of villi on ova half a line in size, and delineates them on an ovum of 162¾ hours, and one and a half line in size, much the same as Bischoff does. In another case Barry found none on an ovum two and a quarter lines in size. In regard to this Bischoff remarks, that he has never observed such a difference, but the smallest on which he observed the villi were, as already said, two lines in diameter. On larger ova the villi were in a corresponding degree more developed. But Barry affirms also, that he has seen in the villi from the beginning a cell-structure. This Bischoff says he can decidedly contradict, as he directed particular attention to the point.

§ 166. The ova next in degree of development Bischoff describes were elliptical, three lines in the long diameter, two and a half lines broad. Still loose in the uterus, transparent and very delicate, and requiring the uterus to be opened very cautiously for their removal. With the naked eye he remarked on the surface of the external membrane, the villi as small elevations, and on the blastodermic vesicle the *area germinativa* as an opaque point. The villi appeared under

the microscope as small broad laminæ, sessile on the external membrane, and of a finely granulated texture. The blastodermic vesicle separated from the external membrane on applying to the ovum aqueous humour, and presented the same condition as the previous ova, except that the vegetative layer had already extended farther on the inner surface of the animal layer—that forming the vesicle. Both layers partake in the formation of the area germinativa (*Fruchthof.*) The two layers of the blastodermic vesicle presented the difference, that the cells of the animal layer were already more completely fused together, and more densely filled with fine molecules, and so formed a denser membrane, whilst the cells of the vegetative layer appeared still distinctly separate and very delicate and pale.

§ 167. In a stage of development close to that of the last ova, Bischoff examined others. Their presence produced a distinct dilatation of the uterus from which it was difficult to remove them uninjured in the fresh state. No intimate union however had taken place between the lining membrane of the uterus and the external membrane of the ovum, but the two surfaces were closely applied to each other, and the ova lay as if imbedded in cells produced by the dilatation of the uterus. The two ends above and below were alone free in the cavity of the uterus. The ova were three and a half to four P. lines in size, and elliptical in form. The villi were still more distinct to the naked eye—scattered in irregular groups over the whole surface of the ovum. Under the microscope they presented round notched edges, and consisted of a homogeneous transparent mass, in which dark molecules were imbedded in single groups. No cells nor nuclei to be seen. In these ova, even when quite recent, and as yet untouched by any fluid, the blastodermic vesicle no longer lay closely applied to the external membrane, but between the two there was a transparent fluid. The *area germinativa* on the blastodermic vesicle was considerably extended, but still uniformly opaque and round. The vegetative layer had become developed so much farther at the inner surface of the serous layer, that it had already extended beyond the largest diameter of the vesicle. The two layers were most intimately adherent at the *area germinativa.*

§ 168. In the next period, from about the beginning of the ninth day, it is quite impossible to take the ova uninjured out of the uterus, as the villi of their extremely attenuated and fine external membrane is in such intimate connexion with the much developed mucous membrane of that organ. In opening the uterus the external membrane is necessarily torn; the fluid between it and the blastodermic vesicle consequently escapes, and the blastodermic vesicle is found lying free in the cell of the uterus, so that this vesicle itself might be mistaken for the whole ovum, which Bischoff thinks has been done.

§ 169. Bischoff has bestowed great care to satisfy himself by observation of the actual presence at this period, of the external membrane of the ovum, which was derived from the vitellary membrane (zona), and albuminous-like investment, and to obtain by observation of the villi developed on it, the certainty that it is an important part of the future chorion. The only successful plan of opening the uterus without destroying the ovum, Bischoff found to be, to remove very carefully layer by layer the coats of the uterus on the side opposite the insertion of the mesometrium over an ovum,—the muscular, cellular, and lastly the mucous coat. But this procedure is successful in exposing the ovum whole only when *the epithelium separates from the mucous membrane, and remains adherent to the external membrane of the ovum.*

§ 170. When the ovum is thus exposed, it appears as if covered by a granular efflorescence. When Bischoff observed this appearance for the first time, he supposed that a membranous exudation had formed from the uterus around the ovum, analogous to the decidua of the human ovum. He, however, denies the existence of a decidua in the mammifera; though it is the opinion of most writers that such a structure does form. The consideration of the question does not come within the scope of the present Report.

§ 171. The mucous membrane of the uterus, Bischoff says, is at this time raised into many minute folds and villi, which are covered by sheaths of epithelium. The

epithelium is no longer formed of ciliated cylinders, but of cells fused together, the nuclei of which are still quite distinct, so that a granular appearance is thereby produced. This epithelium now readily separates from the villi of the mucous membrane in a continuous layer, and remains adherent to the external membrane of the ovum, the villi of which are thrust in between the sheaths of the villi of the mucous membrane of the uterus. It presents in this form, when not highly magnified, exactly the appearance of lace, as pointed out by Coste, who called it *adventive membrane.*

§ 172. Ova exposed in the uterus in the manner above described were about half an inch in size, the blastodermic vesicle lay free within the external membrane. On that side of it turned to the insertion of the mesometrium was the *area germinativa,* still round, and either still uniformly opaque, or beginning to clear in the centre—the commencement of the central *area pellucida,* and the circumferential *area opaca,* similar to what occurs in the bird's egg. The vegetative layer of the blastodermic vesicle had extended all round, so as to form a vesicular layer within, and concentric with and closely applied to the animal layer. The clearing up of the centre of the area germinativa takes place chiefly in the animal layer.

§ 173. A few hours later the difficulty in the investigation is much increased. The blastodermic vesicle now no longer lies free, but begins to be united to the external membrane by its animal layer, and thus indirectly with the uterus. This union commences first at the side opposite the insertion of the mesometrium, and from this gradually extends to the mesenteric side. In exposing the ovum in the uterus, Bischoff proceeded as above; but now, in consequence of the alteration just described, he removed the coats of the uterus from the mesenteric side, by which however the external membrane is necessarily torn; but, as the blastodermic vesicle is not yet adherent to the external membrane at this place, it is exposed uninjured. And this place is the most important, as it is here that the area germinativa is always found.

§ 174. For examination, Bischoff cuts out a portion of the blastodermic vesicle with the *area germinativa,* and lays it on a glass plate, or in a small watchglass, in order to examine it with the microscope by transmitted light. This process is not always successful; but when it does succeed, it is seen how that the *area germinativa* not only extends farther and farther, but undergoes in quick succession changes of form. It is no longer round, but first oval and then pear-shaped, its long axis always falling on the transverse axis of the somewhat oval ovum and of the uterus. In all these forms it is composed of an opaque periphery, the *area opaca,* which incloses a clear space, *area pellucida.* These differences are owing to the different accumulation of cell-materials, especially in the animal layer; the cells appear to pass as it were from the centre towards the periphery, leaving the centre transparent. Sometimes while the *area germinativa* is still oval, but generally not until it is pear-shaped, there is seen in the long axis of the area pellucida a clear streak, at first very indistinct; with this commences the first trace of the proper embryo. This usually takes place on the eighth or ninth day post coitum.

§ 175. Although the ovum of the dog differs from that of the rabbit in some points, still the principal processes are the same in both.

In the rabbit the external membrane of the ovum, which becomes covered with villi, and by which the ovum becomes adherent to the uterus, is formed of the vitellary membrane (zona) of the ovarian ovum and the albuminous layer which is added in the Fallopian tube. In the dog, the external membrane on which villi are formed, and by which the ovum becomes adherent to the uterus, is simply, Bischoff affirms, the vitellary membrane or *zona pellucida* of the ovarian ovum without any superadded albuminous layer.

In the dog, as in the rabbit, there is formed, after a process of subdivision of the yelk and cell-formation, a blastodermic vesicle. This vesicle is composed of the two layers, animal and vegetative, which lie closely applied to each other, but yet

may be separated. The commencement of the germ is a spot in the blastodermic vesicle, at first round, then elliptical, afterwards pear-shaped, in which the first trace of the embryo appears as a clear streak with opaque accumulations of matter on either side.

§ 176. Bischoff considers that there is no doubt that in these essential points the human ovum in its first development in the uterus must agree with the ovum of the mammifera. By direct observation, indeed, little or nothing is known on the point. The very early ova observed by Velpeau, by the author of this report, and by Volkmann, Bischoff is inclined to consider abnormal, and gives the following sketch of the condition he should expect the normal human ovum to present at this period: It would not lie free in the uterus, but be invested and fixed more or less in the the substance of the decidua, and that probably in the region of the orifice of one of the Fallopian tubes. It would farther be at first still like the ovarian ovum, afterwards transparent, consist of two vesicles, the outer of which, so long as the embryo had not yet appeared, would show at most the first faint traces of villi. The inner vesicle would lie more or less close to the outer, and separate from it on the application of water to the ovum. This inner vesicle would present under the microscope a cell-structure, and at one part of it either a whitish spot or a round oval or pear-shaped area germinativa must be observable. The inner vesicle would in general be very delicate, and in its already advanced stage of development would present two layers, closely adherent to each other at the embryonic spot. The size of such an ovum might be expected to be from one eighth of a line to four or five lines.

§ 177. To these statements of Bischoff it must be excepted that in ova of the latter size, the blastodermic vesicle would no longer be found filling the external membrane or chorion; it should in fact be expected to be, relatively to the latter, small, when the small size to which the umbilical vesicle attains is considered.

The ovum, described and delineated by Dr. Allen Thomson, dating little more than fifteen days from conception, if admitted to be normal, shows how great this disproportion must be when the first trace of the embryo appears, and that the ovum described and delineated by the author of this report, before the appearance of the embryo, might not have been abnormal.

§ 178. The transparent fluid mentioned by Bischoff as at a particular stage separating the blastodermic from the external vesicle in rabbits, appears analogous to the albuminous or reticulated mass which, in the cases of Allen Thomson, and of the author of this report, filled the greater part of the interior of the external membrane or chorion.

V. AMNION, CHORION, UMBILICAL VESICLE, AND ALLANTOIS.

§ 179. In 1828 there appeared in the second volume of Burdach's Physiology a pretty complete history of the ovum of the mammifera, from the first appearance of the embryo to the development of all its important parts. The materials for this history were derived from the labours of Prevost and Dumas, Bojanus, Baer, to which may be added those of Oken, Kieser, Meckel, Rathke, J. Müller, and others.

§ 180. A complete continuous series of observations however was wanting. Coste made a step towards supplying this desideratum by tracing the development of the ovum in the sheep, dog, and rabbit. Though in these researches he confirmed much of what had been previously done in Germany, he did not establish anything new of importance.

§ 181. In the second volume of Baer's 'Entwickelungs-geschichte,' published in the same year (1837) with Coste's 'Embryogenie comparée,' there is brought together the history of the development of the embryo and ovum in almost all orders of mammifera and in man, and a perfect analogy is shown to exist in the development of the embryo, not only among the different mammifera but between them and birds. The completeness of this analogy, which had lately been called in question, Bischoff has established beyond all cavil.

§ 182. The first processes in the development of the embryo go on very rapidly,

seeing that in the rabbit, from the appearance of its first trace to that of almost all the important organs scarcely two days elapse,—the ninth and tenth days.

§ 183. As was above seen, after the area germinativa comes to present a central pellucid and a peripheral opaque part, and passes from the round to the pear shape; a bright streak appears on the long axis of the pellucid part. Close around this streak, but especially on either side of it, formative matter collects and constitutes a somewhat opaque plate of the same form as the area germinativa itself, consequently oval or pear-shaped. It is principally in the animal layer that these changes occur. The bright streak is a groove in that layer which is extremely thin, and therefore quite transparent at the place. Viewed from above, the two edges of this groove at the end corresponding to the broad part of the pear-shaped area germinativa, pass into each other by a small arch, whilst at the other end they unite in a point. The former is the head, the latter the tail end of the embryo about to be formed.

§ 184. The accumulations of formative matter on either side of the groove exist likewise only in the animal layer. The subjacent vegetative or mucous layer is almost uniformly opaque in the whole extent of the area germinativa by which the distinction between the pellucid and opaque divisions which the animal or serous layer presents at that place is greatly lessened. Corresponding to the bright groove in the pellucid part of the animal layer, there is in the vegetative layer a faint clear streak, which, however, appears to be merely an impression made by the groove of the animal layer. At the groove the two layers adhere so closely that it seldom happens that a separation is effected between them at this place without laceration.

§ 185. The groove in question is what has been called the primitive streak, and was considered by Baer and others as an actual linear accumulation of matter. This, however, was shown not to be the case in the bird's egg, by Coste and Delpech, and more recently by Reichert. The observations by Bischoff on the mammiferous ovum, just detailed, show that in it also the primitive streak is simply a groove. The same thing is agreeable to Reichert's and the author's own observations on the batrachian reptiles. Instead therefore of *primitive streak*, Bischoff employs the expression *primitive groove.*

§ 186. The two accumulations of formative matter on either side of the groove, Bischoff does not with Reichert consider to be the original halves of the nervous system, but the *original basis of the body of the embryo.*

§ 187. The *area opaca* having much extended itself and again become round, and the *area pellucida* and the basis of the body of the embryo having become fiddle-shaped, that portion of the formative matter which immediately constitutes the two sides of the primitive groove, rises up in the form of "dorsal plates." These meeting in the middle over the groove convert it into the canal in which the central organs of the nervous system are formed. Subsequently the circumferential part of the same formative matter becoming thickened, and bending downwards and inwards constitutes "visceral plates." The area pellucida is now reduced to a crescentic patch around the head end of the embryo.

§ 188. From this it is seen that the first development of the embryo of the mammifera is analogous to that of the embryo of the bird. The farther development of the embryo and its organs is so likewise. It is not, however, purposed here to follow the development of the embryo itself further in detail. Let it suffice to observe, that the dorsal plates having closed in, the formation of the nervous centres commenced, and the head end of the dorsal canal become dilated, the embryo bends forward on itself, and an elevation of its anterior, and in a less degree of its posterior ends takes place from and above the plane of the blastodermic vesicle, so that the embryo is, as it were, constricted off from it The sides still pass gradually into the blastodermic vesicle; but by and by the same thing takes place here also. This change is owing to the margins of the visceral plates all round, approaching each other below. A cavity is thus inclosed, named the visceral cavity, communicating with that of the blastodermic vesicle at the place of constriction, between it and the embryo.

§ 189. It is to be particularly remarked that the whole embryo is as yet composed only of the thickened central part of the animal layer; the vegetative layer lies quite smooth at its under surface. The vegetative layer, however, takes part in the constriction which occurs between the embryo and the blastodermic vesicle. Above and below it is drawn within the visceral tube, which is in the process of formation. If the embryo be now examined from the ventral side, the head and tail ends are seen covered by the vegetative layer from the place to which the constriction has extended, from the place of entrance therefore into the upper and lower part of the visceral tube. Those parts of the vegetative layer covering the head and tail ends of the embryo have been called *involucra*—of the head and of the tail.

§ 190. In the formation of the head and tail involucra, the animal layer does not take part in the same manner as the vegetative, in consequence of a change in the former now taking place, which gives origin to a new formation and a change in all the relations of the ovum.

§ 191. If at this time the head end of the embryo, (at the tail end the formation is still too little advanced,) be examined with the greatest care under a magnifier, and with fine needles, it is found that it does not lie free on the blastodermic vesicle, but is covered by a double fold of an extremely fine and transparent membrane. This membrane is a part of the animal layer of the blastodermic vesicle undergoing a metamorphosis towards the formation of the amnion.

§ 192. The fold of the animal layer extends more and more over the head end, in an arched line towards the middle of the back. While this is going on, the same process takes place at the tail end; and soon after from the lateral margins of the embryo also. In this manner the animal layer reflects itself from the whole periphery of the body of the embryo, which is merely its developed central part, over the back of the embryo, and then abruptly folding on itself, extends back peripherally. From all sides the folds approach more and more the middle of the back, until at last the margins of the folds meet together in a point. The inner layer of the fold thus comes to cover the whole body of the embryo, lying so close on it, and being so fine that without particular manipulation it is not at all to be recognized. The outer or upper layer again forms a continuous membrane. Only at the point of closure of the folds, the two layers continue for some time united, but eventually separate from each other entirely. The layer lying close on the embryo, and inclosing it is the *amnion;* the outer layer is still the animal, but is now called *serous* investment, (false amnion of Pander in the bird,) and lies, so far as it is entirely separated from the inner, close to the outer membrane of the ovum, with which Bischoff affirms it coalesces entirely. It is now by this coalescence of the serous investment with the outer membrane of the ovum that the formation of the *chorion* is, according to Bischoff, completed.

§ 193. The above-described mode of development of the amnion, first discovered in the bird, has been observed and traced by Baer in the sheep, sow, and dog.

§ 194. From his observations on the ova of the cat, sheep, and rabbit, Dr. A. Thomson is satisfied that the amnion is formed by the union of the cephalic and caudal folds of the serous layer of the germinal membrane in the mammalia, nearly if not exactly in the same way as was shown to take place in birds by Pander. In the animals mentioned he has examined the amnion in its open state before the union of its cephalic and caudal folds.

§ 195. Coste gives a different view of the formation of the amnion in the mammifera. He supposes that it is first formed by the detachment or rising of the outer covering or epidermis of the fetus.

§ 196. Dr. Barry, while he adopts the explanation which has been given of the manner of formation of the amnion in the bird, says he must be understood as maintaining, in opposition to the view of others, that the membrane so appropriated in mammalia, is no part of that structure out of which the embryo is formed. The membrane he refers to as forming the amnion, is that which was above spoken of as the blastodermic vesicle of Bischoff, the animal and vegetative layers inclusive.

§ 197. The gelatinous-looking investment which, as above seen, the ovum in

the Fallopian tube presents in addition to the vitellary membrane (zona) of the ovarian ovum, was first indicated by the author of this report as the future chorion. Dr. Barry, when he had become acquainted with the structure in his 2d series, and had in consequence renounced all he had said in his 1st series, (p. 316,) under the head of "The true chorion, a structure superadded within the ovary in the class mammalia," so far confirmed what had been indicated by the author of this report, that he traced the structure up to the period when villi formed on it in the uterus.

§ 198. The author of this report having observed that at a certain stage the vitellary membrane (zona) is no longer visible, supposed that in the progress of development it gives way, and the gelatinous-looking investment comes to constitute alone the chorion. Barry maintains that the vitellary membrane (zona), (not his vitellary membrane), on the contrary continues distinct. Bischoff admits that the vitellary membrane does cease to be distinct from the gelatinous-looking investment, but maintains that it does not give way and dissolve, but coalesces as above seen with the gelatinous-looking investment.

§ 199. Bischoff says, (p. 118,) "I believe I have thus proved that the amnion in the mammifera is a product of the development of the peripheral part of the animal layer of the blastodermic vesicle, and that the chorion is a very complicated structure formed, in the rabbit, of the vitellary membrane (zona) of the ovarian ovum, of the albuminous layer added in the passage of the ova through the Fallopian tubes, and likewise of the peripheral part of the animal layer of the blastodermic vesicle, which on its union with the chorion was called serous investment."

§ 200. In the dog's ovum, around which, as above seen, he affirms that no albuminous layer is formed in passing through the Fallopian tube, Bischoff says the vitellary membrane (zona) is itself the part on which the villi form. But Bischoff mentions (p. 119) his having observed appearances in the rabbit which might be taken to indicate that the external membrane of the ovum composed of the coalesced vitellary membrane (zona) and albuminous investment dissolves at a certain stage, and that then the serous investment derived from the animal layer of the blastodermic vesicle comes alone to form the chorion. The chorion is thus, according to Bischoff, either a combination of the external membrane of the ovum resulting from the coalescence of the albuminous layer and vitellary membrane (zona) and of the serous investment; or the chorion consists of the serous investment alone.

§ 201. Reichert* doubts the coalescence of the vitellary membrane (zona) with the gelatinous-looking investment, and is inclined rather to the opinion that the vitellary membrane sooner or later disappears. In regard to Bischoff's statement that the chorion is always a product of development from the ovum, and not derived from the maternal organism, Reichert, while he justly admits the correctness of it, observes that the proposition is not in agreement with what Bischoff says regarding the origin of the gelatinous-looking investment. (See § 85.)

§ 202. About the time the amnion begins to be formed, the vascular system first appears in the form of a *heart-canal*, in the anterior wall of the head end of the embryo, where it is continued into the blastodermic vesicle, of a *vena terminalis* at the periphery of the area opaca, and, throughout the latter now becoming area vasculosa, of slight traces of a *vascular network* between the vena terminalis and the under crura of the heart-canal. The vessels in the area vasculosa lie between the animal and vegetative layers, and constitute, according to Bischoff, a distinctly demonstrable *vascular layer*. He has quite certainly separated with the needle this vascular layer from the outer surface of the vegetative layer as an independent membraneous formation, as far as the circumference of the body of the embryo. (p. 122.)

§ 203. The amnion having been formed, the development of the intestinal canal goes on; and during this the embryo, by development and approximation of the visceral plates, becomes more and more separated by constriction from the vegeta-

* Beiträge zur Kenntniss des Zustandes der heutigen Entwickelungs-geschichte.—Berlin, 1843, p. 8.

tive and vascular layers of the blastodermic vesicle, which now constitutes the *umbilical vesicle*. The vessels of the vascular layer, consisting of one or two veins carrying blood to the embryo and an artery proceeding from it, are the omphalo-mesenteric. The canal to which the communication between the intestine and umbilical vesicle is now reduced is the *ductus omphalo-mesentericus* or *vitello-intestinalis*.

§ 204. Bischoff observes that this mode of formation of the umbilical vesicle is the same in all the mammifera. In regard to the open communication between the umbilical vesicle and intestine, which has been so much contested, there can be no doubt. He has examined numerous embryos of the dog, cow, rabbit, and rat, in which this communication was as evident to the senses as it must be to the mind after some knowledge of the whole process of development. In the further relations of the umbilical vesicle to the embryo and the ovum there take place in the different orders of the mammifera great differences.

§ 205. Whilst the mucous and vascular layers of the blastodermic vesicle begin to be separated by constriction from the embryo in the form of umbilical vesicle, there is observed sprouting out at the lower end of the embryo, already become free by constriction, a small very vascular vesicle which is at first round, afterwards pear-shaped. This production is the *allantois*, a part of the greatest importance in the further development of the embryo and ovum.

§ 206. It has generally been supposed, with Baer, that the allantois is first developed as a hollow diverticulum from the lower end of the intestine, and consists like it of a vascular and mucous layer. Recently it has been affirmed by Reichert that in the bird the allantois presents itself originally in the form of two small solid prominences at the end of the Wolffian bodies, and in connexion with their excretory ducts; but he does not derive the allantois from the Wolffian bodies. These prominences gradually coalesce and form at first a flattened elevation, which is soon converted into a vesicle. Coste again describes the allantois as an immediate production from the blastodermic vesicle. Bischoff, although he does not agree with Coste's views, admits his observation to be correct. From his researches on the rabbit, Bischoff confirms Coste's statement that the first appearance of the allantois takes place before the intestine is formed, and also before there is even a trace of the Wolffian bodies. In the embryos of rabbits in which the mucous and vascular layers of the blastodermic vesicle were still evenly continuous with the two sides of the embryo, and only the head and tail end, the latter still quite round, were separated from it by constriction, Bischoff has seen the first trace of the allantois at the tail end, in a form such as is represented by Coste. There was no appearance of the end part of the intestine, which is already formed in embryos a little older; and of the Wolffian bodies Bischoff could not discover any trace under the microscope. The basis of the allantois appeared to him as a growth from the visceral plate of the tail,—as a mass of cells not yet hollow, but in which there already existed a great number of vessels. Afterwards, when the allantois has become a vesicle, it is truly in connexion with the intestine and the excretory duct of the Wolffian body; but how this comes about Bischoff has not yet been able to determine.

§ 207. When the allantois has distinctly become a vesicle, it and its vessels grow quickly. The vessels are the future umbilical vessels. By the closing in of the visceral plates, in the formation of the abdominal walls and navel, the vesicle represented by the allantois is divided by a constriction into two compartments. That which is within the embryo is the urinary bladder; that which is without is the allantois, properly so called, and is disposed differently in different animals. The canal at the constricted place, and which afterwards becomes a mere cord, is the *urachus*.

§ 208. The peripheral part of the allantois soon forms, as is known, one of the most important parts of the ovum. Hitherto the ovum had received what was required for its development and nourishment, merely by imbibition through its external membrane, but now the important business of drawing nourishment from the mother is transferred to the vessels of the allantois. The allantois with its

vessels turning to the right side of the embryo, and at the same time twisting the lower end of the latter from left to right, grows rapidly towards the chorion, applies itself and becomes adherent to it. The allantoidal vessels not only pass into the chorion but are prolonged into the villi which here undergo a remarkable degree of development. At the corresponding place the mucous membrane and vascular system of the uterus in like manner become unusually developed. The two vascular systems, that of the allantois in the villi of the chorion on the one hand, and that of the uterus on the other, though not in direct communication come to be in very close connexion. There is thus formed the *placenta*.

§ 209. The development of the allantois and its vessels, like that of the umbilical vesicle is very different in the different orders of mammifera.

§ 210. Having traced the origin of the amnion, chorion, umbilical vesicle, and allantois in the rabbit, a brief statement may now be made of what is known, or supposed to be known on the subject in man. The closest correspondence, it will be observed, exists between the human ovum and that of the other mammifera.

§ 211. *Amnion.* From their observations Professors Baer and A. Thomson hold, that in the human ovum the amnion is formed in the same way as in the mammifera and birds, only it is to be remarked that the ununited state of the cephalic and caudal folds has never yet been observed. In the very early human ovum described by Allen Thomson, he did not detect an amnion. He says the embryo was fixed by its back to the chorion. This Bischoff interprets as follows: The folds of the serous layer which forms the amnion, had closed over the back of the embryo and the outer layer, the serous investment had applied itself to the chorion, whereby the embryo appeared fixed to the chorion by means of the amnion at the place of closure of the folds. From what he has observed in the mammifera, Bischoff thinks this union is afterwards dissolved, but the author of this report believes that in man the union at the point of closure never dissolves. In the human ovum the amnion is always adherent at one point to the chorion, and that appears to be the place where the amnion closed or tended to close in. It is the place also near which the allantois joins the chorion.

§ 212. *Chorion.* It has been seen that Bischoff is of opinion that the human ovum does not in its passage along the Fallopian tube become covered by an albuminous investment, but that the external membrane in the uterus is the vitellary membrane (zona) of the ovarian ovum. The chorion, he believes, results from this vitellary membrane (zona), combined with the serous investment of the blastodermic vesicle. That this is the case the author of this report is much inclined to doubt. The latter part of the statement in particular he denies; for if the early ovum described by Dr. Allen Thomson be admitted to have been normal, the blastodermic vesicle is too small for its serous layer to have yielded a serous investment to the chorion, and that after forming the amnion. From an observation which the author of this report himself made not long ago, he is inclined to believe, as was originally suggested by Baer, and observed by A. Thomson, that the serous lamina of the blastodermic vesicle in the human ovum does not go to form a serous investment to the chorion, between which and it there is so large a space filled with a gelatiniform substance; but after forming the amnion, the rest of it remains adhering as a very delicate film to the blastodermic, now umbilical vesicle.

§ 213. The chorion has originally no vessels of its own. It is only where it has been joined by the allantois that its villi contain vessels, and that only at the place where the placenta forms. In pachydermata, ruminants, and carnivora, the allantois being extensively developed, lines the whole of the chorion, which is thus found in the form of a vascular membrane, as indeed is also the amnion from the same cause.

§ 214. *Umbilical vesicle.* In regard to this there is a great difference between the human ovum and that of most of the mammifera; for whilst in the latter the umbilical vesicle attains a very large size, and remains distinct throughout the whole of fetal life, and even in some, as the rodents, plays an important part by

carrying vessels to the whole of the chorion with the exception of that part which corresponds to the placenta; in man it attains a very slight degree of development, quickly loses its importance for the embryo and ovum, and sooner or later wholly disappears. The great length into which the omphalo-mesenteric duct, now become closed, is drawn in the human ovum, is a peculiarity connected with another, viz., the length of the umbilical cord. The cases observed by Allen Thomson, Coste, Wagner, Müller, the author of this report, and others, remove all the doubt which was originally entertained as to the omphalo-mesenteric duct being really a free communication between the intestine of the embryo and the umbilical vesicle.

§ 215. *Allantois* In human ova between the third and fourth week, described by Wagner, Müller, Coste, Allen Thomson, Meckel, and Baer, the allantois presented itself as in the mammifera in the form of a small elongated vesicle, which protruded from the lower end of the embryo, and applied itself by its broad base to the chorion. In later ova this vesicle is no longer found, but there is in its stead a cord extending from the embryo to the chorion containing the umbilical vessels. The observation of ova after the disappearance of the allantois formerly led to the erroneous supposition that either no allantois exists in the human ovum, or that it quickly attains a great degree of development, and adheres by one layer to the inside of the chorion, and by another to the outside of the amnion as in the pachydermata and ruminants. Adopting this latter view, Velpeau maintained that the gelatiniform substance or *magma reticulé*, between the chorion and amnion is the contents of the allantois, but to say nothing of the erroneousness of the view that the allantois attains such a great degree of development, the observations of Dr. Allen Thomson, and of the author of this report, show that the *magma reticulé* exists before the allantois itself appears.

POSTSCRIPT.

§ 216. Since the preceding report was printed, the author has seen in the "Comptes rendus hebdomadaires" of the French Academy of Sciences for July 17th, 1843, an extract of a letter from Professor Bischoff to Professor Breschet, on the detachment and fecundation of the ova of man and the mammifera. In this communication Bischoff expresses himself as being now satisfied that the detachment of the ova from the ovaries does not, as he formerly supposed, take place only after coitus, nor of course depend on the direct contact of the seminal fluid with the ovary, but that ova come to maturity at more or less regular periods, and are spontaneously detached from the ovaries whether coitus take place or not; and consequently that in some cases it must happen that ova are detached before, and in other cases not till after coitus. Under ordinary circumstances, however, the detachment occurs after coitus; for, according to Bischoff, it is during the maturation of the ova in the ovaries that animals enter into heat, and are thus impelled to coitus before detachment takes place.

§ 217. The following case Bischoff adduces to show that ova may be detached from the ovaries before coitus. A young and strong bitch which had never pupped was watched at the time of its coming into heat, and after being in this state several days, was lined. Immediately after coitus, which lasted a quarter of an hour, Bischoff extirpated the left horn of the uterus along with the Fallopian tube and ovary of the same side. On examination with the microscope, semen was found to have penetrated as far as the superior angle of the horn of the uterus and the spermatozoa moved actively. No trace of semen was observed in the tube, but the ovary already presented burst Graafian follicles and distinct *corpora lutea*. In the Fallopian tube *five ova* were discovered, already advanced fifty-five millimeters from its abdominal orifice.

The animal was killed twenty hours after the coitus, and on examination of the parts of the right side, spermatozoa, still in motion, were found, not only towards the horn of the uterus, but advanced six millimeters in the Fallopian tube. There were five *corpora lutea* in the ovary, and *five ova* in the middle of the tube. No

spermatozoa were observed around the ova, which Bischoff thinks was owing to the ova on the one hand, and semen on the other not having met The ova, therefore, could not yet have been fecundated.

§ 218. Admitting, as is already done in the preceding report, that ova come to maturity at more or less regular periods, and are spontaneously detached from the ovaries, the question arises, Are such ova as are detached before coitus capable of being fecundated should coitus take place within a certain time after? Bischoff answers this in the affirmative, but has not yet brought forward facts to prove that it is so. The author of this report must confess that he did not contemplate the likelihood of the occurrence when he maintained the proposition that it is not until the ova have been detached from the ovaries and received into the Fallopian tubes, that fecundating contact takes place between them and the semen,—a proposition which it will be seen Bischoff also now adheres to. And though now that the question has been brought before him, the author of this report does not venture to deny the probability of the occurrence, he must observe that there appears to him a difficulty standing in the way in regard to *corpora lutea*, at least in the human subject. Were "corpora lutea" always formed in the ovaries after the detachment of ova, whether coitus has taken place or not, they ought to be found in the ovaries of every woman at the child-bearing time of life. Hitherto, however, the result of observation has been, that they occur occasionally only, and that mostly in cases in which there has recently been conception. The occurrence of yellow bodies in the ovaries, without previous conception, is not so common, and then there is reason to believe that they are different in their structure from those occurring after conception.

§ 219. Supposing, however, with Bischoff, that there is no difference in the *corpora lutea* formed under these two different circumstances, it would be difficult to account for their all but invariable occurrence after conception, and their occasional occurrence only independent of it. But if there is, as the author of this report believes, an actual difference in the *corpora lutea* formed under these two different circumstances, it must be admitted that the peculiarities of those corpora lutea which occur after conception are connected with some peculiarity in the process of detachment from the ovaries of ova destined to be fecundated; and such peculiarity, it is to be remarked, can only be determined in some way by the coitus, from which the conception results. If this be so, it would follow that the detachment from the ovaries of ova destined to be fecundated, must be subsequent to coitus. But the point is one on which much light may be thrown by farther observation.

REPORT ON THE EPIDEMIC AGUE OR "FAINTING FEVER" OF PERSIA, *A Species of Cholera, occurring in Teheran in the Autumn of the year* 1842.

By Charles W. Bell, M.D.
Physician to Her Majesty's Mission to Persia.*

The epidemic which has been of late so fatally prevalent in several districts of Persia, particularly in the capital Teheran, and in the neighbouring district of Cazveen, presents some points of analogy with Indian cholera (cholera asphyxia), both in its nature and habits. It seems to be following the same north-westerly course which that disease pursued when, some years ago, it made its first appearance in Europe: and it may be regarded as a direct continuation of that form of cholera which was prevalent at the mouths of the Indus, in the commencement of the year 1842; and which reached Bunder Abbass, in the Persian Gulf, a little later in the season. I have been assured by the prime minister of the King of Persia, who is the best authority to be had here on the subject, that a disease similar to that which I am about to describe, has appeared at several points in succession between Bunder Abbass and Teheran; and it was evidently making its progress west and northerly in the direction of Azerbijan, when it was checked, by the setting in of winter, on the high grounds that intervene.

If, unfortunately, this disease should extend itself farther, some previous knowledge of the nature of the complaint cannot fail to be interesting to practitioners; for though not perhaps very formidable when the principles upon which it is to be treated are understood, and sufficient time is given to carry them into practice, still it is far from contemptible. According to the best calculation I can make, it would appear that not less than four fifths of the whole population of this city were attacked by it, and that one sixteenth (of the whole population) have died of it.† It is perhaps true that the mortality would have been much less if the native physicians had adopted a proper line of practice; still a disease producing such havoc, under any circumstances, is a formidable one. And if by publishing the present account of it, I should be fortunately able to save to others the regret which I myself have experienced for the loss of my first patients attacked with it before I became acquainted with its nature (for subsequent experience has convinced me that these ought to have been saved), it will make some amends for the anxiety which I then underwent; for, without previous knowledge of its nature, in a disease so obscure at its commencement as this is, and with a termination so unlooked for, some lives must be sacrificed, even in the best hands, before the necessary experience be gained.

I. History of the Disease.

The disease of which I am about to give some account is, in my opinion, essentially a quotidian ague; but, as far as I know, it has not hitherto been described as an epidemic.‡ Although dangerous when left to nature, it is most amenable to correct treatment. It becomes more dangerous in proportion as the ordinary stages, shivering, fever, and sweating, are less distinctly marked. And, when in place of the shivering fit, there occurs an agonizing oppression of the heart with general congestion of the venous system, the case will, according to my experience, always prove fatal if unassisted by art; whereas in the very worst cases, unless the circulation have become absolutely stagnant, this congestion may be relieved and by proper means a recurrence of it be prevented.

The following are the principal modes in which this disease produces its dangerous or fatal effects: first, general venous congestion, and, consequently, impeded action

* This Report was drawn up at the request of Count Medem, H. I. M. the Emperor of Russia's Minister Plenipotentiary at the Court of Teheran.

† The population of Teheran ranges in different months between 700,000 and 100,000 inhabitants.

‡ I should here except an allusion to what is apparently the same disease mentioned as occurring in a district of India, in 1825, in Mr. G. H. Bell's work on Cholera, p. 22. It is to his instruction that I am indebted for the rationale of bleeding in cholera.

of the heart and cessation of the circulation ; second, sudden effusion into the lungs and exudation from the capillaries generally into the cellular tissue ; third, diseased spleen, constantly recurring ague, general dropsy, and failure of the powers of life.

The other sources of danger, which no doubt exist, I have been prevented from ascertaining by the impossibility of making any post-mortem examination in this country. But I have little doubt that one of the most common causes of sudden death was the effect of venous congestion upon a heart enfeebled by previous disease.

In describing this disease I shall divide it into several varieties, without pretending that there are not others, or that all I shall notice are always distinct and distinguishable from one another. I shall take the varieties as nearly as possible according to the number of persons so affected, beginning with the simplest and most ordinary form.

First variety. In this variety the disease assumed the form of a simple quotidian ague, and affected more than four fifths of the whole population of Teheran. This is a form of ague which I have rarely seen in Persia, the usual type being tertian. In general the patient got well in two or three days without medicine ; but if he dwelt in a low or damp situation, or if he were treated with the usual Persian prescription of manna dissolved in two or three quarts of the juice of water melon, or if he indulged in fruit, the disease usually passed into the second degree or variety.

Second variety. This was also quotidian ague but accompanied with considerable pain on pressure over the spleen and very rapid enlargement of that organ ; attended with remarkable blanching of the lips and a pale, clean, bloodless tongue. The rigor was shorter and less severe than usual, but the fever continued long and, though not running high, was accompanied with much thirst and dryness of the tongue. The sweating stage was short and imperfect, and the intermission of short duration. In some cases this fever was attended with severe headach, and was then liable to pass into the third degree ; or the shivering ceased and vomiting and purging came on instead of the cold fit, or it assumed some other severer form ; or if recovery took place, constantly recurring fever, always accompanied by pain on the left side, at every change and full of the moon, or on the slightest error of diet, were too often the consequences of it. I daily, however, met with cases where this fever has continued, with little intermission, for two or three months ; the powers of life gradually sinking under the disease : dropsy or dysentery being the usual termination.

Third variety. This form had several modes of commencement. Sometimes it began at once, by the patient becoming suddenly insensible without previous symptoms ; at other times, it was preceded by formal ague. In many instances, again, the patient would suffer for some time previously from intermitting headach daily increasing, and great want of sleep ; he would then have one attack of ague, and, next day, at the same time, would sink down insensible. This was the form of disease from which the greatest number of deaths took place, and obtained for the malady its Persian name TAB-I-GHASH, or "fainting fever." During the insensibility the pulse was feeble and the extremities cold. From this state many were never roused; but if they were, the pulse gradually attained power and the patient came slowly to his senses, complaining of intense headach and a feeling of oppression at the heart; a low kind of fever then came on which was succeeded by very imperfect perspiration, generally confined to the head and chest. Next day, about the same hour, insensibility returned, and each attack continuing longer than the preceding one, the period of death depended upon the strength of the patient or violence of the disease ; most frequently, however, death took place on the third attack. As the end approached the secretion of urine ceased,* the efforts of the heart at reaction became feebler, the skin felt like that of a corpse, cold and damp, the body became purple and mottled, and the pulse became less perceptible at the wrist: at length the patient was seized with tetanic convulsions and died. In these cases, as often observed in cholera, the feet began to get

* On one occasion I was called to see what was said to be a case of retention of urine, and found the patient in articulo mortis from this disease, the body cold and the legs beginning to get warm. I attempted to bleed, but only ℥iij could be obtained by drops. He was soon seized with convulsions, and died.

warm shortly before death, and just as the warmth had spread up the legs and reached the trunk the patient died. Indeed, were other symptoms wanting, I should consider warmth commencing in the feet while the rest of the body was cold, quite sufficient to mark the case as hopeless.

Fourth variety. Owing to their brief duration, I saw few cases of the severer form of this variety; but I received the details of many such cases as the following: the person was seized with sudden pain in the pit of the stomach, the belly at the same time becoming hard; there was neither vomiting nor purging, but the patient craved incessantly for ice, and died in about an hour.

The cases presenting such symptoms as the following, were probably milder attacks of the same kind. In this form there was no insensibility, no shivering, little or no perceptible fever, and no perspiration; the primary characteristic symptoms were a fixed pain in the pit of the stomach, extreme tenderness on pressure over the left lobe of the liver and region of the spleen, and extreme tension of the abdominal muscles. One or both of the recti abdominis became hard as a board, continuing for days in a state of constant tension, but without any painful cramps or spasms. Nearly at the same hour each day the patient was observed to become exceedingly anxious and restless, tossing from side to side, sighing and throwing the arms above the head, as in yawning, and the pulse became very small and frequent, and the body damp and cold. By and by, this oppression passed off, the body resumed its natural warmth, and the pulse nearly its natural volume, but this continued quicker than usual, and then to all appearance the patient had very little the matter with him. Each day, however, the oppression of the circulation became greater and the attack continued longer; the pulse now became weaker and an ice-cold exudation ran off the brow and back of the hands.* The struggling of the heart to overcome the load of blood which oppressed it was most painful to listen to,—now almost overcoming the obstruction, the pulse for an instant gaining power, and a partial warmth spreading over the surface; and, again, the force of the heart succumbing to the disease, and the icy coldness—much colder than death—returning. The craving for iced water was incessant so long as this state lasted. The evacuations meantime were bilious, and the quantity of urine daily diminished, and at length ceased altogether. At length the intermission between the attacks of oppression ceased to occur, the pulse was only perceptible at intervals, and the patient, who up to this time had been perfectly sensible and even able to walk to stool, fell into a state of stupor. The skin now became blue and mottled, and the patient gradually sunk or died in convulsions. Here also, as I remarked above, sometime before death took place the lower limbs recovered almost their natural warmth. In all the cases I saw of this variety there was much feeling of distension of the stomach and inactivity of the bowels, and sometimes a little vomiting.

Fifth variety. Here the disease was cholera, with the usual symptoms of vomiting and purging, and suppression of urine. The two former symptoms, however, were less in degree than in the ordinary Indian cholera; and the painful cramps of the limbs were either absent or less in degree. For about ten days cases of vomiting and purging of an intermittent kind were very frequent; as if a slight attack of cholera had taken the place of the cold fit of ague: generally, however, these latter cases yielded easily to the chalybeate draught with quinine, to be hereafter mentioned.

Sixth variety. I, myself, saw only one well marked case of this singular form of the disease, although I heard of several occurring in Teheran. This form was said to be especially fatal in a regiment quartered in the district of Boorojird; but perhaps I may be mistaken in classing it with this disease on premises so slight. According to the account which I received, the patients went to bed in good health, felt chilly in the

* It was my painful duty to attend an intimate friend, the Shah's chief physician, through an illness of this kind. He was one of the first attacked, and had not yet seen a case of the disease. He obstinately refused to submit to bleeding or to take the medicines which I recommended, having a great predilection for calomel. It is chiefly from his case that I describe the latter stages of this complaint. All that I subsequently attended were cured.

night, and next morning their head and neck were found swollen to an enormous size, they breathed with difficulty, and in the course of a few hours died suffocated.

The case which I saw was that of a courier just arrived from Boorojird. He was apparently quite well at night, and was to have started with dispatches in the morning, when, not making his appearance, he was sent for, and was found with his face and neck immensely swelled and almost suffocated. He was brought to me immediately, when I found that, besides the extraordinary anasarca of the head and neck, which was rapidly extending upon the chest, there was effusion going on into the lungs. The skin was cool, the pulse about ninety, soft and weak, and the tongue rather pale. I ordered an immediate large bleeding; blood was obtained with the greatest difficulty at first, but presently flowed freely, and he felt much relieved. He then took calomel and jalap in syrup, which was swallowed little by little with some difficulty. These operating smartly, there were no remains of the edema left next morning. Being quite recovered, he started on the following day and had no recurrence of the attack. The symptoms here were too obscure to enable me to say whether this ought to be considered as the cold stage of the disease, but the difficulty of obtaining blood inclines me to this belief.

Besides the above more formal varieties of this epidemic, there were observed several other forms less distinctly marked, or the same forms characterized by the predominance of particular symptoms or combinations of symptoms. Under this head I may enumerate—intermittent pain and swelling over the tibiæ; tic douloureux and nervous pains of almost every part of the body; tonic spasms, continuing for many days, of a portion of the colon, with obstinate constipation, but without other severe symptoms; jaundice, &c. &c.* What confirmed the accuracy of the view which classed these latter affections with the general epidemic, was the fact that they, in general, yielded easily to the same medicine which was found so effectual in it.

II. Treatment of the Disease.

As this Report is intended to be strictly of a practical character, it forms no part of my present object to enter, at any length, either upon the etiology or pathology of the disease of which I have given an account. Before proceeding, however, to notice the plan of treatment which I found most successful, I shall make one or two remarks which seem to have a direct and important bearing on the subject.

In the first place I have to state that while the disease was at its height in the town, the blood, even of those not sensibly attacked, was universally of a dark, dusky, reddish-brown colour, very different from that of healthy venous blood, and in general the serum did not separate from the clot. This fact there was abundant opportunity of verifying, for most Persians are in the habit of losing blood twice or oftener in the year, and a great number let blood once or twice every month. Secondly, in those affected with the severer forms of the disease, the blood drawn in the cold fit was always grumous, coming at first slowly or in drops, and coagulating as soon as drawn, even at the mouth of the wound; and no separation of the serum took place. Thirdly, during the epidemic the urine of the people in general was much darker coloured than at other times, while in those who were seriously affected it was, if secreted at all, like porter, and in very small quantity.

From these facts, as well as from the general history of the epidemic given above, it would appear that in the production of this disease there must have been some occult cause acting upon the blood, and producing effects similar, on the whole, to those which we find in cholera. The most marked of these effects are, 1, impaired fluidity and sluggishness of the blood in the veins; 2, consequent probably on this state of the blood, great general venous congestion; 3, a disposition to a daily periodical return

* A kind of jaundice was common during the height of the epidemic, which, in the absence of other symptoms, I attributed to spasms of the gall-duct, and, as there were slight symptoms of ague, treated with the chalybeate draught alone, to which it yielded easily. In the fever that I saw in the island of Karrach in 1839, all died jaundiced: dissection showed nothing in the liver, but in every one the spleen was soft, and broken with the slightest touch.

of this congestion; 4, in most, perhaps in all cases, more or less of disease of the spleen. In many instances pain is felt in the region of the spleen before the occurrence of any other symptom.

If these views are at all correct, or whatever be the mode of explaining them, if the four states there named be of constant occurrence in this disease, the attention of the practitioner must ever be directed to the means of obviating them, and he must not allow himself to be distracted from their consideration by other collateral symptoms of less importance.

In the treatment of this disease there were several obvious indications the means of fulfilling which seem to me to be these:

1. In the first place, it is essential to avoid everything likely to increase the disease. Under this head, among the most powerful are damp, and the use of fruit and uncooked vegetables, especially water-melons. The last in many cases I have seen acting as a direct poison, bringing on an immediate attack of fever, which terminated fatally in two or three hours. Therefore, removal to an upper room and careful diet I found to be particularly useful.

2. For the purpose of obviating the tendency to venous congestion, iron appears to me to excel every other medicine. The sulphate is the preparation to which I have given the preference, on account of its solubility and the ease with which it combines with sulphate of magnesia or aloes—one of which is necessary, both on account of the astringent quality of the iron, and because I found the bowels in this complaint generally either inactive or exceedingly irregular in their action.*

3. With a view to obviate the periodical attacks, quinine is obviously the medicine most to be depended upon. Alone, however, and uncombined, quinine appeared to be too exciting, increasing the thirst, headach, and pain at the pit of the stomach; and it produced astonishingly little specific effect upon the disease.

What I found by far the most effectual mode was to combine both these tonics with a cooling laxative, and give them all at the same time. For this purpose I made use of the following formula with little alteration in nearly a thousand cases, and with the most complete success:

℞ Magnesiæ sulphatis ℥j
Quinæ sulphatis gr. ix
Ferri sulph. gr. xij
Acid sulph. dilut. ʒj
Aquæ fontis Oiss. Dosis ℥ij.

I directed a dose of this to be taken, in ordinary cases, three or four times a day,—if possible at the fourth, third, second, and first hour before the daily accession; but if this took place either in the night or early in the morning, or if the time of the attack were uncertain, I directed the medicine to be taken at stated intervals during the day, whatever might happen to be the stage of the fever at the time. So far from increasing the thirst and fever, as quinine uncombined often does when taken in the hot stage, the effect of this combination was generally immediate in subduing the thirst and dryness of the mouth, which were always distressing. Thus administered, the mixture almost always produced its effect in cutting short the fever on the third day, and this, too, in cases where large doses of quinine alone had failed.

In the severer cases I reduced the quantity of sulphate of magnesia, and sometimes gave the medicine every half hour.† Very commonly the effect of the first few doses was to *increase* the strength and duration of the *shivering fit*, and *shorten the fever* of the first paroxysm; it then prevented the accession of the next fit: and comparatively few had a third attack after commencing its exhibition. I found it of great

* The value of iron in subduing irregular action of the circulation in general, or of an organ, as in uterine complaints, where it is alike useful in amenorrhea and in menorrhagia, has been often remarked. For its efficacy in the cure of spleen disease, I have only to refer to the testimony of Mr. Twining's invaluable work on the Diseases of Bengal.

† On two or three occasions I was surprised to find my patients, especially one who was very dangerously ill, recover much more quickly than I expected; but it afterwards came out that they had finished the whole supply of medicine at a single draught.

use, where pain in the region of the spleen was considerable, to aid its effect by the application of six or eight leeches, a few hours before the cold stage was expected: in cases of young children leeches were particularly beneficial.

In a great many cases, when the patients lived in a low or damp situation, the recurrence of fever was so frequent, especially just before the full and change of the moon,—each attack being accompanied with more and more enlargement and pain in the region of the spleen,—that I was obliged in several houses to lay it down as a general rule for those who had suffered, that on the 12th or 13th, and on the 26th or 27th of the Persian or lunar month, six or eight leeches should be applied to the region of the spleen, or six or seven ounces of blood taken from the arm, and the use of chalybeate medicine resumed for two or three days. I here allude especially to one district of the town where scarcely any one escaped fever, and particularly to several houses of one street under which a watercourse of sulphurous-smelling water ran, and where every one attacked, who was not either treated in this manner or escaped to another quarter of the town, died. Many of the survivors, notwithstanding this advice, which was too disagreeable to be literally followed after the immediate danger was over, are still suffering from fever at every change and full of the moon.

But cases occurred where no time was given for the comparatively slow operation of this medicine. This was where the attack was sudden and severe, or where the oppression, which unless relieved immediately is sure to be fatal, had become so great before assistance was sought, that it was quite beyond the power of medicine. Here there is plainly no resource but the lancet. But in order to use this with success, both the fullest confidence in the correctness of our views of the pathology of the disease, and some boldness too are necessary. It must be confessed that appearances are much against the practice; and our resolution is very apt to fail us when we see the man cold, with scarcely a pulse; his lips blanched and bloodless; himself anxious and terrified, and tossing his arms like one who is already bleeding to death. Very naturally, too, his friends object to a proposal so little according with their prejudices. If in this state a vein be opened freely, the blood comes only in drops or trickles in a feeble stream, dark and grumous or of a brownish colour, and coagulating on the arm. When about two or three ounces were drawn in several such desperate cases I found the patients get faint and throw themselves about in an agony of oppression; the blood stopped and they appeared dying in my hands; and I have no doubt that they would have died had the attempt here been abandoned. In such cases, strong ammonia must be held to the nostrils, the arms must be kneaded, and the patient must in some way be induced to cough or sneeze, whereby the blood receiving an impulse again begins to trickle, each moment acquiring force, till at length it flows in a full stream. The patient now sits quietly instead of tossing about in an agony as before; his faintness goes off, and, as the blood flows, he feels his spirits rise, all his oppression ceasing, and his conviction is now complete that he is cured. And so he generally is, if only enough of blood has been taken; but less than sixteen or twenty ounces will not be sufficient. I generally continue the use of the medicine for some days after the bleeding, especially if near the full of the moon,* when relapse is exceedingly likely to occur. But I have seen some very bad cases cured without any further after-treatment than a dose or two of castor oil.

Prejudiced, as I confess I had been, against the practice of bleeding in the cold stage of intermittent fever, having never till this season met with a case which really

* I am well aware of the ridicule to which I expose myself in thus attributing such marked effects on disease to the changes of the moon; but the evidence before me is so strong as to have removed all my doubts upon the subject; and I see the effects in such a number of patients who suffer from ague every full and change, that I should feel ashamed did the fear of ridicule prevent me stating a fact of which I am so entirely convinced, and which, if true, is so important. Unfortunately, no account is kept of the daily number of deaths in Teheran; if there were, I should be content to rest the settlement of the question upon that alone. The 15th I am satisfied has been by far the most fatal day of the lunar months corresponding to September, October, November, and December 1842, and the 12th and 13th, and the 26th and 27th, the days upon which a recurrence of fever has most frequently taken place, or upon which the symptoms have most frequently become aggravated.

required it, I at first only used the lancet in the worst and most hopeless cases of this disease. When, however, my stock of quinine began to run low, I was under the necessity of finding a substitute for it; and the lancet was that which most naturally suggested itself. But being particularly desirous to avoid interfering with the prejudice entertained by the people of this country against bleeding in the cold stage, I determined to try whether the same effect would not result from taking blood *before* the accession of the cold fit. I reasoned thus:—It was more probable, I thought, that the shivering itself was only a symptom marking a certain degree of venous congestion, than that the venous congestion and the shivering commenced at the same moment, and that congestion had probably existed for a considerable time, gradually increasing before it attained the degree necessary to produce so violent a proof of its existence; that if this were the case, I did not see any reason why bleeding should not put a stop to congestion before it had attained what may be called *the shivering point*, and while yet the circulation had power, quite as effectually—or even more so—as it would relieve the congestion when it had attained its maximum; and when, consequently, the circulation was most oppressed and enfeebled. Besides, the quantity of blood stated by the best authorities as necessary to be taken when bleeding in the cold stage is practised, and which my own experience had fully confirmed, was greater than what is usually ordered to be taken at once in Persia, and more than I was often willing to take from my patients during the prevalence of an epidemic of this character; and I was in hopes that a smaller quantity of blood taken at an earlier period of congestion would produce the same effects as a full bleeding in the cold stage. Being careful, then, to ascertain as exactly as possible the usual hour of accession, two hours before the attack was expected I took twelve ounces of blood and gave three grains of quinine immediately afterwards, and I had the satisfaction to find that the blood was easily obtained, without fainting or annoyance, and that no attack followed. I afterwards found that it was of little consequence at what period blood was taken, provided the sweating stage were fairly over; only that when too near the time at which the shivering should commence, it sometimes *hastened* the attack; but, then, it seldom recurred. I was usually in the habit of giving a little quinine or a cup of ginger-tea after the bleeding.

The advantages of this treatment were—that the disease was generally less likely to recur than when cured with medicine alone; and the blood was obtained without difficulty, without any disagreeable faintness, and without alarm to the patient.

In the intermittent or scarcely-intermittent headaches so prevalent at one time, bleeding sometimes cured them at once; and sometimes it was followed soon afterwards by a regular paroxysm, which did not however in general recur. This difference of effect was probably owing to the time at which the blood was taken with reference to the cold stage (which I supposed to exist in these cases, although nothing of the sort was evident to the senses,) there being nothing to show whether the accession of headache belonged to the cold or feverish state of ague.

In the severer cases the period of accession was sometimes so obscure that it was scarcely possible to discover it. The attack of oppression continued so long and went off so gradually,—there was so little fever, and altogether so little of the character of ague in them,—that the patient himself was not aware of the periodical character of the disease. I have generally found it easier to make out from the statements of the patient and his friends the time at which remission took place, than from my own observation directed to the periods of fever. For example: the patient is described as tossing about and extremely restless for eight or ten hours, then, although not apparently better, for some time he is comparatively still, so that he allows the coverlet to remain upon him; perhaps he himself after this perceives a warm moisture on the neck and chest, and then, feeling himself better, enjoys some repose (sleep was very rare); that on the following day about the same time he begins to toss about as before, &c. Here it is evident that the restlessness corresponds with the period of oppression or the cold stage, and that the period of repose is the remission. Having, therefore, begun the exhibition of the medicine immediately, it is the duty of the physician to be present at the time that appears to be the period of remission, when, if he have hit the proper moment, he will find the patient astonishingly well, the skin natural,

the pulse quicker and weaker than it should be, but little that is remarkable except pain on pressure over the pit of the stomach; presently, however, the patient's face becomes anxious, he stretches his arms, and frequently throws himself from side to side sighing. If the practitioner have listened to the heart on his first arrival, and does the same now that its struggling has commenced, he will be in no douht as to the correctness of his diagnosis: the period of venous congestion has begun, and now is the moment to use the lancet with success; unless the symptoms be sufficiently mild to permit of his waiting for the remission of the following day, in order to take blood before the oppression comes on. But it is seldom safe to wait; for even if no immediate danger be apprehended, it is to be remembered that this long-continued congestion is inevitably disorganizing the spleen as well as probably the liver and the kidneys, and is very likely to injure the valves of the heart.

I cannot too much urge the importance of most accurately ascertaining the time of the accession (which is more difficult in practice than appears in description), because if blood be taken at the wrong time the effects are most disastrous.

If the intention of bloodletting in this disease be not, as in inflammation, to diminish the force of the circulation, but, on the contrary, by means of abstracting some of that blood which is stifling the right side of the heart and impeding its action, to aid the efforts of nature to overcome this impediment and to give force to the circulation by restoring the balance of action between the right and left side of the heart, —it is quite evident that that object cannot be attained when the obstruction has been overcome and the balance has been restored,—in other words, that an attempt to remove what does not exist is an absurdity, and that whatever harm bloodletting may do after the oppression has been overcome, it can do no possible good.

If the hot stage have commenced it is a proof that instead of the balance of the circulation being on the venous side as formerly, it is now on the arterial side; and although, *theoretically*, bleeding from an artery might now be admissible, bleeding from a vein must necessarily tend still further to disturb the balance,—just as, during the period of congestion of the venous system, opening an artery instead of a vein would obviously still further disturb the balance between the venous and arterial system, which it is our object to restore.* But this fever is the effect of reaction, or, in other words, of that excitation of the circulating organs which is produced by and necessary to overcome the opposition to a free circulation of the blood; and we know that it will presently subside and be followed by a period of weakness, relaxation, and sweating: it is natural, therefore, to suppose that bleeding in either of these stages will tend only to increase the relaxation, to produce fainting, and to increase the tendency to collapse in the subsequent accession. This, I am sorry to say, I know from experience; for, in the first two cases I saw of this disease, not aware that it was an ague and urged by severe local symptoms, I bled in what I now know to be the hot stage: the blood flowed with perfect freedom, but both patients died collapsed soon afterwards.

The history of these and some other cases would be very illustrative of all that I have advanced, as well as of the obscurity of the symptoms; but this report has already drawn to such a length that I must forbear. In terminating it I have chiefly to regret that the prejudices of the country, by rendering post-mortem examinations impossible, have prevented me adding anything to the morbid anatomy of the disease; for many of the cases of dropsy and other of its chronic effects have been very interesting and very obscure.

To conclude, I cannot too strongly recommend the combination of iron with purgatives during the prevalence of this epidemic, whenever the tongue is pale, as especially at its commencement. Until I adopted this practice I found that class of medicines either not acting at all or doing so with undue violence; and that that addition of carbonate or sulphate of iron rendered their operation much more regular and more certain. But it is only when the tongue is pale and clean and bloodless that the good effects of iron are apparent. These effects will be partly evinced by the in-

* Mr. Twining, in his chapter on Cholera, relates two cases which well illustrate this subject,—one, where he bled just as the pulse was rising; the other, where blood was drawn from the radial artery: in both immediate collapse followed.

creased redness of the tongue, sometimes perceptible even in a few hours. If, however, on the other hand, there be a bilious fur upon the tongue, and it and the lips are red and rosy, iron does positive harm by producing excitement. This is especially to be guarded against as the epidemic is passing away, and people in general are returning to their natural state of health.

On the other hand, calomel and all preparations of mercury are especially contraindicated by the bloodless tongue and pain and swelling of the spleen. Although recommended by some of the highest authorities in Europe, calomel has proved injurious in every case of affection of the spleen, and in all the cases of this disease in which I have tried it; and also, very remarkably, in a remittent fever (in which also the tongue was pale) that was prevalent in the summer months before this epidemic made its appearance.

During the stage of collapse I have seen no benefit from the exhibition of stimulants of any sort. They appeared to me always to do harm, each dose causing increased oppression of the heart and coldness of the extremities.

After recovery the diet should be very light and the quantity of animal food allowed sparing.

It is worthy of remark that during the prevalence of this epidemic, for upwards of three months, I did not meet with a single case of acute inflammation nor of gout or continued fever, although in the same months of the preceding year these diseases, with ophthalmia, enteritis, acute rheumatism, and pericarditis, were the most prevalent complaints.

Teheran; January, 1843.

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OF THE

BRITISH AND FOREIGN MEDICAL REVIEW.

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C. AND J. ADLARD, PRINTERS, BARTHOLOMEW CLOSE.